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CARROLL
ROUND
PROCEEDINGS

The Seventeenth Annual Carroll Round
An Undergraduate Conference Focusing on Contemporary International Economics
Research and Policy

Editors in Chief:
Victor Li, Alice Ye

Associate Editors:
Margaret Underwood, Shine Aung, Yi Bao, Sang Jun Chun, Joshua Levy, Chloe Li, Victor Li, Peter Liu, Jacob Witt, Yuou Wu, Alice Ye, Crystal Zhu

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Appendix
What is the Carroll Round?
The Carroll Round is an international economics conference for undergraduate students held each spring at Georgetown University in Washington, D.C. It takes the format of a professional academic conference at which students present their original research in international economics (broadly defined) that are typically honors theses. The goal of the Carroll Round is to foster the exchange of ideas among the leading undergraduate economics students by encouraging and supporting the pursuit of scholarly innovation. To date, over 500 students from universities and colleges in North America, Western and Eastern Europe, Asia, the Middle East, South America, and Australia have participated, making the Carroll Round the premier conference of its kind. The conference also provides opportunities for participants to interact with prominent academic and policy economists. Alumni have moved on to top Ph.D., J.D., M.B.A., and other graduate programs, positions at the Federal Reserve, World Bank, and other public institutions, and major private corporations.

Notes on Paper Submissions and Conference Participation
The Carroll Round Proceedings is a publication of synopses and full-length papers from the Carroll Round Undergraduate International Economics Conference at Georgetown University. We do not accept paper submissions from the public. If you are interested in presenting at the conference, please log on to our website: http://carrollround.georgetown.edu. All undergraduate students who have written or are in the process of writing original work in the field of economics are encouraged to apply. Special preference is granted to papers focusing on international issues.

Notes on Published Papers
Many of the papers published in this journal have been shortened due to length requirements. In most cases, changes were minor. However, some essays were significantly abridged due to these constraints.
Acknowledgments

Organizing an economics conference as undergraduate students is no small feat. A large part of the success of the current Carroll Round can be attributed to the hard work and dedication of previous committees. Each successive committee harnesses an astonishing legacy in building the conference each year, united by the founding mission to expand the frontiers of knowledge and develop innovative policy applications through academic discourse. At the core of this legacy, of course, are the numerous individuals and organizations without whom the Carroll Round would not be possible.

The Carroll Round would like to acknowledge special individuals who have cared greatly about our cause. Alumna Marianne Keler and her spouse Michael Kershow have graced us with their support and presence every year. The Carroll Round Endowed Program Fund that they created for us provides us with a perpetual income stream to support the annual conference. We thank Ms. Keler and Mr. Kershow deeply for having been such an important part of the Carroll Round over all these years.

Our immense gratitude will forever to go to alumnus Yunho Song, who has personally supported the Carroll Round from its very first year. The first committee had the privilege of sitting down with Mr. Song at the Tombs to convey our dreams. Mr. Song designated part of his endowment fund for us, which now supports the Carroll Round year after year.

Among the Carroll Round alumni, Mr. Scott Pedowitz has provided tremendous guardianship and support. Mr. Pedowitz was a member of the founding committee, and the Carroll Round was able to gain his support in each of the seventeen years since his graduation. Mr. Pedowitz meets with the committee members every year to receive updates and to share the vision that has continued since the founding year.

The Carroll Round would have not been possible without the support of many other individuals. We would like to recognize Mr. Mario Espinosa, Mr. Oleg Nodelman, Ms. Colleen Murphy, Mr. and Mrs. Kenneth Kunkel, Ms. Sarah Osborne, Mr. Jonathan Prin, Mr. Jon Skillman and Ms. Luanne Selk, Mr. Geoffrey Yu, and former Carroll Round Steering Committee members Mr. James Arnold, Ms. Meredith Ballotta, Mr. Stephen Brinkmann, Ms. Amanda Delp, Ms. Stacey Droms, Mr. Brandon Feldman, Ms. Yasmine Fulena, Mr. Christopher Griffin, Dr. Andrew Hayashi, Ms. Rebecca Heide, Dr. Anna Klis, Mr. Michael Kunkel, Ms. Marina Linhart, Ms. Nancy Lee, Ms. Kristen Skillman, Mr. Shuo Tan, Dr. Erica Yu Wright, and Dr. Ariell Zimran. In addition, the Sallie Mae Corporation significantly funded the first five conferences, and we are most grateful for their foresight in supporting our conception and our growth into an established undergraduate research conference. Moreover, we express our gratitude to the Kanzanjian Foundation, which provided the startup funds to develop the Carroll Round Proceedings.

Within Georgetown, the Carroll Round was helped by past and present members of the advancement office: Mr. Mohamed Abdel-Kader, Mr. Thomas Esch, Ms. Carma Fauntleroy, Ms. Elizabeth Franzino, Ms. Reema Ghazi, Ms. Gail Griffith, Mr. Richard Jacobs, Dr. Venilde Jeronimo, Ms. Katerina Kulagina, Ms. Christine Smith, and Ms. Cara Sodos. The Carroll Round is presently pursuing an institutional research grant, and the guidance of Professor Shareen Joshi and Mr. Clark Bonilla have been invaluable. We would also like to provide special recognition to all the former steering committee members, beyond those already mentioned, who have contributed very generously to help the Carroll Round. Among them, we would especially like to thank Ms. Sue Bai, Mr. Albert Chiang, Ms. Daphney Francois, Mr. Edward Hedke, Mr. Dennis Huggins, Ms. Cindy Jin, Mr. Michael Karno, Mr. Dan Leonard, Mr. Jonathan McClure, Mr. Brendan Mullen, and Dr. Emy Reimao.

Beyond the financial viability of the Carroll Round, the conference also enjoys the grace of many proponents on Georgetown University’s campus to ensure its continuing and vibrant existence. We deeply thank each of the successive deans of the School of Foreign Service: Robert Gallucci, Carol Lancaster, Jim Reardon-Anderson, and Joel Hellman. Administratively, the Carroll Round was helped by SFS Dean’s Office members Dean Kendra Billingslea, Ms. Denisse Bonilla-Chaoui, Mr. Beau Boughamer, Ms. Rebecca Ernest, Mr. Franz Hartl, Dr. Dan Powers, Ms. Shelby Roller, Mr. Michael Volk, and Mr. Benjamin Zimmerman. Mr. Joseph Scafidi has helped us each year with organizing a Career Panel with World Bank and IMF alumni.

For the last eighteen years, the Carroll Round has been fortunate to enjoy the substantive quality of the papers of economics undergraduates from across the world. We are particularly grateful to those professors that steer their best students to the Carroll Round, especially Professor Nancy Marion of Dartmouth College, Professor Judith Shapiro of the London School of Economics, Professor Michael Seeborg of Illinois Wesleyan University, Professor Gianna Boero of Warwick University, and Professor Ian Walker of Lancaster University.

We are grateful to receive the professional experience and wisdom of some of the most respected economists in the field. For the Seventeenth Annual Carroll Round, we were particularly fortunate to have keynote lectures from Nobel Laureate Dr. George Akerlof of Georgetown University and Dr. Augusto Lopez-Claros of the World Bank. Also critical to the substantive development of the Carroll Round and our participants’ work are the session chairs who take the time to read participants’ papers and critique their presentations at the conference. We would like to thank the 2018 session chairs for their contributions to the conference: Professors Dan Cao (Georgetown), Robert Cumby (Georgetown), Christopher Griffin (Harvard), Shareen Joshi (Georgetown), Marko Klasnja (Georgetown), Rodney Ludema (Georgetown), Olga Timoshenko (George Washington), and Charles Udomsaph (Georgetown).

We thank the past Carroll Round Steering Committees, which have shaped and directed the development of the conference into its current state today. Their names are all listed in the Appendix section.

Finally, though not least importantly, we would like to express our ever-growing gratitude to Dean Mitch Kaneda, the Carroll Round Faculty Advisor. Without his support, time, and passion, this endeavor would not be possible.
A Brief History of the Carroll Round
(Revised March 2019)

Each year, as April arrives, I realize how the current Carroll Round is both completely recognizable from the first conference weekend and unlike anything my friends on the inaugural steering committee imagined. Accepted paper quality has increased exponentially, and the weekend's highlights are the students' masterful presentations of those manuscripts. None of these advances would be possible without the extraordinary work of the Georgetown students who organize the Round each year and, of course, the global contingent that gathers in the nation's capital to participate. Other alumni and I remain awestruck by the effort, dedication, and commitment of each successive participant group. Despite the perpetual need to look ahead, reviewing its origins is equally important. During the 2001-2002 school year, the ingenuity and dedication of a stellar group of Georgetown students, combined with the contributions of remarkable young scholars from around the country, showed how strong undergraduate economics always has been and can be.

The conference's birthplace, as many know by now, was an Oxford pub called the Radcliffe Arms. But the Carroll Round's roots extend firmly to the Georgetown University campus. For it was there that an incredible team of young men and women launched the event the next year. Throughout the 1999-2000 academic year, I had the great pleasure of meeting and learning alongside seven outstanding economics classmates. My first meaningful discussions about the discipline took place that year with fellow students Andrew Hayashi and Ryan Michaels. Andrew and I were both enrolled in Professor Mitch Kaneda's International Trade class in the fall semester, and Ryan suffered with me through the rigors of Microeconomic Theory and Introduction to Political Economy. I remember feeling intimidated at first by their boundless knowledge of theory and their irrepresible enthusiasm for learning. Over time I realized I was learning from them as much as from our instructors; their insights often proved more valuable than the content of weekly lectures. I also became acquainted with a second group of classmates, including Bill Brady, Josh Harris, Kathryn Magee, Brendan Mullen, and Scott Fedowitz. By the spring, our paths all pointed to Europe: Bill, Kathryn, and Scott were on their way to the London School of Economics; Brendan had chosen the University of Bristol; and Josh was destined for Poland and Hungary. Andrew, Ryan, and I planned to spend our year abroad at the University of Oxford studying a mixture of philosophy, politics, and economics. Before departing in October 2000, I knew our shared plans were not the product of mere coincidence. Something special would emerge from the experience.

Having established initial ties at Georgetown, the three of us began meeting on a regular basis to discuss our latest tutorial sessions, grueling problem sets, the future of macroeconomics and, occasionally, the latest gossip about luminaries in the field. Whereas C.S. Lewis, J.R.R. Tolkien, and the other Inklings made Oxford's Eagle and Child pub their intellectual home and watering hole, we adopted the Radcliffe Arms as our haven. Over pints and pub food, Andrew's twin passions for game theory and philosophy emerged. C.S. Lewis, J.R.R. Tolkien, and the other Inklings made Oxford's Eagle and Child pub their intellectual home and watering hole, we adopted the Radcliffe Arms as our haven. Over pints and pub food, Andrew's twin passions for game theory and philosophy emerged. The future of monetary policy and development began to vex Ryan's thoughts. I hoped to better understand the mechanisms of cooperation and conflict underlying international trade institutions. Meanwhile at Pembroke College, I encountered a group of students from universities across the country also spending their junior years at Oxford. I naturally befriended the other economists in our group, but I also developed close relationships with physicists, biologists, literary scholars, and art historians. In the Junior Common Room, a student lounge of sorts for undergraduates, or over traditional English dinners in the dining hall, we shared stories about life at our respective universities and the latest research we were conducting at Oxford. Thesis and postgraduate plans matured during these conversations, and I appreciated ever more my exposure to alternative experiences and approaches to scholarship. The year eventually came to an end, and I worried that these exciting connections would dissolve after returning to Georgetown.

One evening at the start of my final term in Oxford, I thought about the importance of this dialogue and my commitment to the study of international economics. I had a sense that undergraduates, especially in economics, were not afforded adequate opportunities to present their work in a serious setting. After all, I always felt privileged when Andrew, Ryan, and my fellow Pembrokeians shared their original ideas with me. I thought that undergraduate economists from around the country deserved an event at which they could interact significantly with each other and the professional academic community. In March 2001, I composed a memo that outlined my solution: the Carroll Round. The following paragraph from that proposal captures my motivating thoughts:

As they prepare for careers in academia, public service, and business, undergraduate students throughout the country also have joined a momentous dialogue in collegiate, national, and global fora. Many are involved in independent research representing the next generation of critical thought in international relations. Others have enjoyed unique experiences through jobs and internship programs that expose them to the front lines of economic policy-making and statecraft. Young women and men also have championed vociferously environmental and labor-related causes through awareness and service programs. Clearly, these timely economic issues are assuming greater importance for the future of international relations and are reflected in the abundance of attendant student research, interest, and initiative. Therefore, I propose to coordinate and host, in association with Georgetown University's School of Foreign Service and John Carroll Scholars Program, the next 'round' of economic and political discussion and debate—the Carroll Round.

I invited Andrew and Ryan to join me in this endeavor over pints at the Radcliffe Arms even though there was no guarantee they would think it a good idea. I was confident that if such rising stars believed in the concept, other students would join in time. Having worked out more substantive ideas over the summer, I finally was prepared to call upon the other economics celebrities in my class to collaborate on the project. Bill, Josh, Kathryn, Brendan, and Scott fortunately signed on and completed the senior circle. A few months later we welcomed four more students: Cullen Drescher, Mark Longstreth, Waheed Sheikh, and future Chair Meredith Gilbert to encourage younger students and ensure continuity for the future.

With the assistance of then-John Carroll Scholars Program Director John Glavin, the proposal was circulated among university
I then asked Mitch Kaneda, my most influential undergraduate teacher and a newly appointed Associate Dean of the School of Foreign Service, to review the proposal. Without hesitation—and somewhat to my surprise—he offered his assistance, embarking on an indefinite and irreplaceable stewardship of the Carroll Round. Former Dean Robert Gallucci and his staff also extended moral and financial support, which cemented our institutional place at Georgetown.

The first Carroll Round Steering Committee struggled through many difficult decisions regarding conference content, format, and funding. Should submitted papers be limited to topics in international economics? What elements must be included in submissions and presentations? How do we ensure that financial constraints do not prevent the best students from attending? Over marathon sessions in Healy Hall and the Tombs, we developed a model for the Carroll Round that has largely remained intact. Development Officers shared our ideas with generous alumni who responded favorably and pledged individual donations. Little by little, our initial concepts materialized into reality. When School of Foreign Service alumna Marianne Keler ('76) convinced the Sallie Mae Fund to contribute $10,000 to the Carroll Round, we both gained a lead sponsor and secured the long-term future of the conference. Since that year, Marianne and her husband Michael Kershow have been gracious in their support.

After distributing colorful brochures, contacting the top departments in the country, and preparing the Hilltop for the event, applications streamed in during the spring. We narrowed our list of invited students to thirty-two by March. Seniors traveled to Washington from as near as the University of Virginia and as far as Stanford University. The Committee was stunned by the participants’ and their home departments’ enthusiasm. Among the more notable responses, Illinois-Wesleyan University sent four young economists to the conference and soon after published a special Carroll Round edition of their undergraduate economics journal.

The first Carroll Round officially began on Friday, April 5, 2002, and the proceedings came to a close two days later. Participants enjoyed an exclusive audience with then-Director of the National Economic Council Lawrence B. Lindsey in the beautiful Riggs Library before hurrying to the Federal Reserve for another private meeting with former Vice Chairmen Roger W. Ferguson and Donald L. Kohn. The two monetary policy experts shared candid stories about the effects of September 11, 2001 on the nation’s banking system and the various roles that the Federal Reserve plays in American economic activity. Dr. John Williamson of the Institute for International Economics spoke about development issues over a splendid dinner at Cafe Milano, and Dr. Edwin M. Truman, former Assistant Secretary of the U.S. Treasury for International Affairs, closed the conference with words of wisdom to students considering careers in academia and policymaking.

A total of twenty-eight papers were presented over the weekend, showcasing the impressive work of men and women now at the forefront of academia, law, and business. Georgetown professors who served as panel discussants later observed that the quality of some presentations met or surpassed the sophistication of recent graduate-level dissertations. Judging by their comments, the conference brought together some of the best young prospects in economics.

I never imagined in the spring of 2001 that the first Carroll Round would attain the heights realized one year later, or for that matter even exist. The event has grown since then in size and scope beyond my initial hopes. The participation of Nobel Laureates from John F. Nash, Jr., in 2004 to George Akerlof in 2015 and 2018, as well as Susan Athey, the first female recipient of the John Bates Clark Medal, in 2008 mark special peaks in the evolution of the conference. Indeed, this historic slate of speakers could not be more finely tuned to the spirit of the Carroll Round. The groundbreaking work that each has contributed to the study of international economics, including numerous articles and books designed to influence lay readers and public policy decision-makers, serve as exemplars for other scholars and practitioners.

Looking to the Carroll Round’s future, I still hope that students from the developing world eventually will be able to attend. Regardless of their home institutions, I continue to enjoy meeting participants and learning about their research interests. I feel humbled to be among such gifted individuals as they share in the excitement of presenting their work and the occasional trepidation of fielding questions. In fact, alumni from the first two decades have been graduate students at Berkeley, Chicago, Cornell, Duke, Harvard, MIT, Michigan, Minnesota, Northwestern, Oxford, Princeton, Yale, and Wisconsin as well as top government and finance positions around the country. Past participants now are tenure-track or tenured members of economics, law, and public policy faculties. The cadre of former conference participants truly has grown into a professional and academic network unlike any other for young economists.

As always, I thank the Kazanjian Foundation for their generous support, which makes possible the annual publication of these Proceedings. I also would like to extend my gratitude to the members of the inaugural Carroll Round Steering Committee without whom this history would have remained fiction. I have great respect and admiration for successor Chairs from Seth Kundrot in 2003 to Meggie Underwood in 2019. Those leaders, and all in between, ensure the success of the Carroll Round each year and deserve our appreciation.

The Carroll Round received a donation several years ago, much like the original Sallie Mae Fund contribution, which created an endowment for the conference, thanks to the largesse of School of Foreign Service alumnus Yunho Song ’86. I distinctly remember meeting with him and some of my closest friends at the Tombs to discuss our fledgling project, uncertain during the fall 2001 semester whether it would ever see the light of day. He was instrumental then in making the Carroll Round a reality, and he now has solidified its place within the fabric of Georgetown and the School of Foreign Service. For that, all of us who have watched the conference are thankful. The spirit of his gift, though, should live on through us. Support from alumni, not just of the financial sort, maintains the conference’s vibrancy long after the proceedings conclude. I encourage each of you to return to Georgetown in April and to consider making any donations to the Carroll Round fund when possible.
Finally, I salute Mitch Kaneda, who has miraculously preserved my vision for the Carroll Round over the years and watched over past Committees as they built upon its initial success and joined the ranks of distinguished alumni. With his continued collaboration and the eagerness of a new generation of Georgetown students, the Carroll Round’s future will dwarf the accomplishments of its past, creating even more exciting opportunities for undergraduate economists to learn from the best in the field and, more importantly, from each other.

Christopher L. Griffin, Jr.
Georgetown Class of 2002
Carroll Round Founder
Why I Support the Carroll Round

I often boast to colleagues from other universities that Georgetown undergraduates are amazing, and Georgetown undergraduates in economics and political economy are even more amazing. What happens when you combine such amazing students with the best and brightest from universities around the world to talk economics on a beautiful spring weekend in Washington? You get the Carroll Round, and I run out of superlatives.

The Carroll Round is more than a showcase of talent. It is a demonstration of the pedagogical value of undergraduate research. Students spend years in classrooms learning the facts, theories and skills necessary to conduct research, which is all very valuable. However, it is when they finally bring to bear all they have learned on a real, practical, self-directed research question that the magic happens. Students finally get to study a problem of their own formulation. They finally begin to realize the usefulness of their tedious econometrics class. They finally get an inkling of the power of economic theory to make sense of what they observe. Most importantly, they gain the confidence of knowing that they are capable of making a true, original contribution to the greater body of knowledge of world they inhabit.

Every university graduate should be able to point to at least one thing they are proud of from their undergraduate experience. For the senior thesis writer, the thesis is often that thing. It is usually the best paper they have ever written and may end up being the best thing they will ever write. No matter. It is the honesty, discipline, knowledge and courage gained from having written it that will accompany them along whatever paths their lives take. I can think of no greater purpose for an undergraduate education, and I can think of no better way to celebrate it than the Carroll Round.

Rodney D. Ludema
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Carroll Round Proceedings

The Seventeenth Annual Carroll Round Undergraduate Research Conference
Poor-Targeted Social Programs and Labor Informality in Latin America: The Case of Mexico’s PROGRESA CCT

DAVID ALZATE

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ABSTRACT

The expansion of poor-targeted social programs in Latin America has brought the concern that they might unintentionally promote labor informality, thereby negatively contributing to poverty and economic growth despite their other positive effects. This paper looks at whether poor-targeted social programs have a short-term effect on labor informality when their design is not expressly tied to recipients’ informal labor status or formal labor income by studying the case of Mexico’s PROGRESA CCT. It exploits PROGRESA’s randomized rollout design and finds that PROGRESA does not statistically significantly impact recipients’ probability of informal employment, a finding which is robust to a consideration of PROGRESA’s impact on employment as a whole. These results can be helpful in elucidating and empirically demonstrating when poor-targeted social programs can avoid having unfavorable side effects when it comes to labor informality.

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1 I would like to acknowledge and sincerely thank the outstanding support and advise that I received from Prof. Charlotte Cavaillé, my thesis advisor. This paper was submitted as an undergraduate honors thesis in International Political Economy at Georgetown University’s School of Foreign Service.
1. Introduction

The wave of poor-targeted social programs in Latin America in the 1990s and 2000s has been heralded as successful in reducing poverty and inequality around the region (Fiszbein and Schady (2009), Holland and Schneider (2017)). Such programs - which came in the form of Conditional Cash Transfers (CCTs), noncontributory pensions, and noncontributory health insurance - supplanted a preexisting contributory social insurance system that had traditionally excluded those outside the formal economy. However, despite the positive impacts of such programs, some scholars warn against their possible unintended side-effects in regard to labor market informality; where informal jobs are defined as jobs that lack access to social security benefits (Levy (2008), Levy and Schady (2013), Bosch and Manacorda (2012), Azuara and Marinescu (2011), Camacho, Conover, and Hoyos (2013), Bergolo and Cruces (2012), Aterido, Hallward-Driemeier and Pagés (2011), Duval Hernandez and Smith Ramirez (2011), amongst others). When these programs tie their benefits to recipients’ informal labor status or formal income sources, they arguably create disincentives for workers to search for employment in the formal sector, where workers must contribute a part of their salary to receive social security benefits (Bosch and Manacorda (2012), Duval Hernandez and Smith Ramirez (2011)). In the words of Levy (2008), by incentivizing recipients to stay in the informal sector, these poor-targeted programs can “trap poor workers proportionately more than non-poor workers in low-productivity informal jobs”, thereby contributing to the persistence of poverty as well as to a reduction in social security contributions and “negative impacts on poverty and economic growth” (Levy (2008), Amarante, Manacorda, Vigorito and Zepra (2011)). As a result, any poor-targeted social program must take into account its possible negative effects on labor informality if it is to have a holistically successful approach in combatting poverty.

The impact of poor-targeted social programs on labor market informality in Latin America has been empirically studied (see Bosch, Manacorda (2012) for an overview of the literature). However, the vast majority of studies have focused solely on the impact of social programs whose benefits are conditioned to their recipients’ informal labor status or formal income. To determine whether or not poor-targeted social programs have an impact on labor informality when their benefits are not expressly conditioned to recipients’ labor status or formal income, this paper looks at the case of Mexico’s PROGRESA Conditional Cash Transfer (CCT). PROGRESA was informality-neutral in its design, meaning that its benefits were given to poor households regardless of their labor status or formal income. By exploiting the random assignment of PROGRESA in its initial stages (1997-1999), this study compares the probability of informal sector employment between program recipients and their counterparts in the control group. It finds that PROGRESA had no statistically significant effect on informal employment, a finding which is robust to a consideration of PROGRESA’s impact on employment as a whole. Thus, this paper contributes to the preexisting literature by being the first to focus on PROGRESA’s impact on labor informality during its initial randomized period and by considering the causal effect of a poor-targeted social program on informality when the program isn’t expressly tied to recipients’ labor status or formal income. This abridged version of this study is organized as follows. Section 2 elaborates on the paper’s main research question and hypothesis. Section 3 presents the paper’s data and methodology, and Section 4 presents its results. Section 5 concludes.

2. Research Question and Hypothesis

The randomized rollout of PROGRESA allows one to determine its causal impact on labor informality by comparing the probability of informal employment between eligible individuals in the treatment group and in the control group. Put differently, it allows for an answer to the following research question: Did PROGRESA cause an increase in the probability of informal employment amongst its recipients?

Given the fact that eligibility for PROGRESA is not conditional on an individual’s formal or informal labor status, or his or her formal income sources, one might expect PROGRESA to have no effect on labor market informality, ceteris paribus. That is, PROGRESA does not make workers more likely to choose an informal job over a formal job, or to switch out of formality and into informality (or to remain in informality), since they get to keep the program benefits in any case. This expectation falls in line with Azuara and Marinescu (2011)’s findings on Oportunidades and with Amarante, Manacorda, Vigorito, and Zepra (2011) and Gasparini, Haimovich, and Olivieri (2007)’s findings on PANES and Plan Jefes y Jefas in Uruguay and Argentina respectively; all of which seem to indicate that poor-targeted social programs, and CCTs in

2 Omitted sections include a literature review, a description of PROGRESA, a more in-depth methodological and descriptive-data section, a section about the implications of the results and their shortcomings, and appendix tables.
particular, incentivize informality only when their benefits are tied to the recipient's formal income sources or informal employment status.

However, as Bosch and Manacorda (2012) point out, a program's impact on informality not only depends on its substitution effect prompting individuals away from the formal sector and into the informal sector. Programs might also have an income effect; whereby formal and informal workers alike are pushed away from employment and into inactivity. If a program incentivizes unemployment, and particularly so for formal workers, then the share of informal jobs as a proportion of total jobs would increase even if no workers transferred from the formal to the informal sector. As a result, a program's impact on overall employment must also be considered when determining its impact on the amount of informally employed individuals. According to the results in Skouffias and Di Maro (2006) and Parker and Skouffias (2000), however, PROGRESA did not impact overall adult labor force participation (as measured by employment status). If this is the case, one can still expect PROGRESA to have no effect on labor market informality as it does not incentivize recipients to move to the informal sector nor to enter unemployment.

Despite that most of the evidence and the theory suggests that PROGRESA should not have an impact on labor market informality given its informality-neutral design, there may be still reasons to believe that the program could have a significant effect on recipients' probability of informal employment. First, as pointed out by Azuara and Marinescu (2011) and Skouffias and Di Maro (2006), PROGRESA could incentivize informal employment as households may attempt to hide their income sources by joining or remaining in the informal sector in the hopes of becoming eligible for the program in future rounds of benefit expansions. This might occur even when eligibility to PROGRESA is not tied to recipients' formal income, as recipients may not be aware of the exact conditions for eligibility and may assume that the more their income is hidden, the better their chances are of getting the program. By avoiding the formal sector, where income is registered and formally taxed on, recipients can try to increase their perceived odds of receiving future benefits. Second, PROGRESA's impact on informality might operate in the other direction: individuals who receive the benefit may be more likely to join or remain in the formal sector if they use the benefit to cover the costs of finding and staying in formal employment, such as transportation costs and mandatory contribution payments to social security. This impact would reflect Skouffias and Di Maro (2006) and Parker and Skouffias (2000)'s findings that, at least initially, “[PROGRESA] families may have used some part of the grants to seek work in salaried activities and to reduce their participation, perhaps, in less profitable family enterprises” (Skouffias and Di Maro (2006)). In other words, just as how PROGRESA may have led individuals to seek work in more profitable activities, it could have also incentivized individuals to leave low-paying informal jobs for higher-paying formal jobs that they could not previously afford given the costs of finding and staying in a formal job.

In sum, PROGRESA, according to the theory behind its design and to previous studies on programs that do not tie eligibility to informal labor or formal income sources, should not create incentives for informal employment. If the results show, however, that this is not the case, and that PROGRESA increases informality, then it may be because PROGRESA prompts recipients to hide their income sources. Conversely, if the results show that PROGRESA increases formality, it may be because PROGRESA allows recipients to cover the costs of acquiring a formal job.

3. Data and Methodology
This paper uses PROGRESA's randomized rollout data and tests whether PROGRESA has an impact on the probability of informal employment amongst its recipients by comparing the probability of informal employment amongst PROGRESA eligible individuals in the treatment group with the probability of informal employment amongst PROGRESA eligible individuals in the control group. To do so, it uses the ENCASEH (1997) survey as a baseline survey and the ENCEL (October 1998) and ENCEL (November 1999) as two different endline surveys. The data is therefore in a panel format, where individuals and households are identified and tracked through time (there are three entries per individual, one per year, in the cases where individuals were surveyed for all three years). Given that this study is interested in an individual's probability of being informally employed, it uses the individual as the main unit of analysis. It restricts the analysis to individuals who were eligible for program benefits at the baseline, as determined by the ENCASEH (1997) survey's poverty
classification, and to individuals who were surveyed at the baseline. It also restricts the analysis to individuals who are over 16 as this is the minimum legal age for employment in Mexico.\(^4\)

The ENCASEH (1997) baseline survey asks recipients whether or not their job grants them access to social security-based health care. While the ENCEL (October 1998) endline survey also tracks responses to this question, the ENCEL (November 1999) survey does not include it (and neither does the March version of the 1999 ENCEL survey). This means that the October 1998 ENCEL survey is the last one to ask individuals about their informal labor status based on this study’s definition of informality. However, the results may not exhibit much variation if the analysis is restricted to an October 1998 endline period, as this implies a treatment period of about two months from the moment households received PROGRESA to the moment where their ENCEL (October 1998) data was collected. In order to allow for a prolonged exposure to the treatment to take place, while making up for the fact that the key informality question is not asked in subsequent endline surveys, this study assumes that individuals’ responses to the question of interest were the same in 1999 as they were in 1998. While this assumption may pose an important limitation, it also allows the analysis to take into account the role of other covariates, such as employment, that may affect the probability of informal employment between 1998 and 1999.

Given the resources available within the data, this study employs a Differences-in-Differences (DD) approach in order to measure the effect of PROGRESA on informality. Such an approach allows us to account for small yet statistically significant differences between the treatment and control group at the baseline (which exist, despite random assignment, given small standard errors across several variables) and to estimate the causal effect of PROGRESA on the probability of being informally employed. It is the approach that many previous studies on PROGRESA have taken (Alzua, Cruces and Ripani (2010), Parker and Skoufias (2000), Skoufias and Di Maro (2008), Parker and Vogl (2018); see Parker and Todd (2017) for a review) and that the program’s own methodological document recommends (Nota Metodológica General Rural (2006)). PROGRESA’s effect, the main independent variable of interest, is operationalized under a treatment effect on the treated (TOT) instrumental variable approach, which captures “the average treatment effect for those who always comply with their assignment” (Imbens and Angst (1994)) or, in other words, “the effect of treatment on the treated (TOT)” (Angrist and Pischke (2008)). This operationalization of the effect of PROGRESA stands in contrast to the literature on PROGRESA and on other poor-targeted social programs mentioned thus far, all of which use ITT (Intention-To-Treat) estimates whenever they employ a Differences-in-Differences approach.

4. Results
Table 1 presents the first regression results of the analysis, using the DD and TOT model from Equation (1) in section IV. Standard errors are clustered on the individual in order to account for the fact that the data’s panel structure means that there is more than one observation for each individual, which induces serial correlation that can result in misleading standard errors if they are not adjusted (Angrist and Pischke (2014)). First-stage results for the instruments (Equations 2 and 3) are reported in the appendix in Table A1, finding the assignment to treatment to be a strongly relevant instrument with a t-statistic of above 430 across all specifications.

The results in Table 1 provide some evidence for a negative effect of PROGRESA on the probability of informal employment. Columns (1) and (3), respectively using the October 1998 and November 1999 ENCEL surveys as endlines, show that PROGRESA decreased the probability of informal employment between the baseline and endline years for PROGRESA beneficiaries relative to their counterparts in the control group (the DD estimator of interest in row 2: \textit{Progresa x Endline Year (IV)}). The results on these columns, however, are not statistically significant for column (1) and only significant at the 10% level in column (3). Consistent with the descriptive information in Chart 3, the coefficients on \textit{Progresa (IV)} in the first row are positive and statistically significant, meaning that informality rates between the treatment group and control group were different at the baseline (although only by about 0.6 percentage points). Columns (2) and (4) add control variables to each endline specification. Results for the 1998 endline specification’s DD estimate turn statistically significant at the 5% level, while the 1999 endline specification remains statistically significant at the 10% level. In sum, these initial

\(^4\) Employment between the ages of 16-18 is only allowed for certain job types, such as specialized workers, technicians, and sports players, according to Mexico’s federal work law.
results suggest the PROGRESA decreased the probability of informal employment by about 0.6 percentage points, giving some evidence for the hypothesis that PROGRESA incentivizes formal employment by allowing recipients to use the benefit to cover the costs of finding and staying in formal employment. Thus far, then, the null that PROGRESA does not influence recipients’ choice between the formal and informal sector is rejected.

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<td>Age Controls</td>
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Robust standard errors clustered on the individual in parentheses: *** p<0.01, ** p<0.05, * p<0.1
However, it may be the case that – despite their random assignment – treatment and control group localities differ in the formal and informal employment opportunities. If this is the case, the effect of PROGRESA on informality in Table 1 might actually show the effect of a PROGRESA-unrelated presence of more formal job opportunities (or less informal job opportunities) in treatment localities relative to control localities. To account for these PROGRESA-unrelated differences in informality between treatment and control localities, Table 2 adds locality fixed effects to the model in Table 1 and Equation 1. Its results show that, under both endline year specifications, the results of the DD coefficient of interest (row 2, Progresa x Endline Year (IV)) are not statistically significantly different than zero under conventional significance thresholds. Thus, it fails to reject the null hypothesis that PROGRESA does not influence recipients’ choice between the formal and informal sector and supplants the evidence in Table 1 that PROGRESA decreased the probability of informal employment. Since the DD estimator of interest and the coefficient on Progresa (IV) (row 1) are no longer significant at the 10% level or above after adding locality fixed effects, one can attribute the baseline differences in informality between the treatment and control group (row 1), as well as the differences in informality between the treatment and control group between the baseline and endline years (row 2), to non-PROGRESA related differences in employment opportunities between the treatment and control localities. The positive direction and statistical significance of Endline Year (row 3) coefficient under the 1999 endline specifications indicates that individuals in the control group became more informal between 1997 and 1999.

Table 2. 2SLS D-D Linear Probability Results, Locality Fixed Effects

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As Stata does not allow for the clustering of individuals when adding locality fixed effects, given that individuals are a subgroup within the locality, standard errors are clustered on the locality for all specifications on Table 2.
Overall, the findings suggest that PROGRESA has no statistically significant impact on workers’ incentives to choose between the formal and informal sector. As previously stated, however, PROGRESA might also prompt workers to enter into unemployment. The ENCEL and ENCASEH data can be used to test for PROGRESA’s impact on worker’s choice between employment and unemployment in order to further account for PROGRESA’s impact on the informality rate as a whole. This is an important consideration because PROGRESA might appear to have zero impact on informality (as in Table 2) when, in reality, it is simultaneously prompting formal workers to move to informal jobs as well as prompting informal workers to move to unemployment – the former effect causes an increase in the informality rate, while the latter decreases the proportion of informal workers under employment. These two forces might cancel each other out and give the erroneous impression that PROGRESA did not have any impact over the probability of being informally

Likewise, PROGRESA might prompt workers to move from the informal sector to the formal sector – thereby lowering the informality rate – whilst prompting workers to move from the formal sector to unemployment – thereby increasing the informality rate. These two effects might cancel each other out to give the impression that PROGRESA’s
In order to account for PROGRESA’s impact on recipients’ choices between employment and unemployment, the model in Table 2 is replicated, and the dependent variable (informality) is replaced by a binary measure of employment status from the ENCEL surveys. The measure for employment equals one when respondents say that they worked or had a job last week, and zero when they say that they did not. The replication is conducted two different times– one limited to formal workers at the baseline and the other to informal workers at the baseline – in order to discern all the possible mechanisms through which PROGRESA might have an impact on overall employment. Results from these replication analyses (omitted from this abridged paper) show that the coefficient on the DD estimator of interest are statistically insignificant and close to 0 across all specifications and across all samples. That is, PROGRESA did not alter formal or informal workers’ choice to become unemployed, mirroring the findings by Di Maro (2006), Parker and Skouffias (2000), and Alzúa, Cruces, and Ripani (2010). Therefore, these results assert that PROGRESA does not appear to have any impact on recipients’ choice between formal and informal employment, and that its statistically insignificant impact on the probability of informal employment is not an erroneous measurement altered by PROGRESA’s effect on employment as a whole.

5. Conclusion

This paper has looked at whether poor-targeted social programs have a short-term effect on labor informality when their design is not expressly tied to recipients’ informal labor status or formal labor income by studying the case of Mexico’s PROGRESA CCT. It exploits PROGRESA’s randomized design and finds that, consistent with the theoretical framework of the effect of programs with an informality-neutral design, PROGRESA did not impact recipients’ probability of being informally employed. This finding is robust to an inclusion of locality level fixed effects that account for differences in employment opportunities between PROGRESA localities and those in the control group. It is also robust to a consideration of PROGRESA’s impact on formal and informal workers’ choices between employment and unemployment. It consequently does not find statistically significant evidence to support the alternative hypotheses that PROGRESA, despite of its informality-neutral design, may have nevertheless prompted recipients to the informal sector in an attempt to hide their income to qualify for future benefits; and, conversely, that PROGRESA may have prompted recipients to the formal sector by helping them cover the costs of securing formal employment.

This study adds to the literature on the effect of poor-targeted social programs on informality by considering the case of a CCT with a random experimental design and with no informality-related eligibility conditions. This contrasts with findings by Gasparini, Haimovich and Olivieri (2007) and Amarante, Manacorda, Vigorito and Zerpa (2011), which have looked at CCT programs with eligibility criteria tied to formal income sources or informal employment; and with findings by Azuara and Marinescu (2011) and Gonzalez-Rozada and Llerena Pinto (2011), which relied on quasi-experimental designs (RD in the case of Gonzalez-Rozada and Pinto) or did not utilize the same level of randomization as PROGRESA did in its initial stages. It is also a study that considers the effect of the program in question on overall unemployment – which can be interpreted as the program’s income effect prompting individuals away from employment and into unemployment – and therefore contrasts with most studies of social programs and informality, which “emphasize exclusively the substitution effect” of individuals moving from the formal sector and into the informal sector (Bosch and Manacorda (2012)). Furthermore, it employs TOT rather than ITT estimates to operationalize the effect of the program in question - a step that the studies mentioned thus far do not take - in order to more accurately distinguish the average causal effect on those who were actually treated rather the effect of the assignment to treatment itself, which can underestimate the effect on the treated (Angrist and Pischke (2008)).

It is key for poor-targeted social programs to, along with their benefits, minimize their unintended adverse side-effects if a poverty reduction strategy is to be successful. Consequently, it is essential that poor-targeted social programs avoid unintentionally incentivizing informal employment as this can lead to a situation of entrenched poverty for their recipients, to aggregate productivity losses, and to reduced fiscal revenues for governments that require funds in order to combat poverty. The finding that PROGRESA did not increase labor informality can be helpful in elucidating and effect on the informality rate, or the average probability of having an informal job, is zero.
empirically demonstrating when poor-targeted social programs avoid having such unfavorable effects. Insofar as a poor-targeted social program does not expressly tie conditions to recipients’ informal labor status or formal income sources, as is the case in this CCT, then the problem of labor informality may be prevented.
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Do Renewable Energy Commitments Reward Shareholders?
A Quasi-Experimental Event Study

OLIVIA BISEL
Georgetown University

ABSTRACT

Does corporate commitment to renewable energy (RE) reward shareholders? 131 of the world’s largest corporations have already signed on to an initiative called RE100, pledging to adopt 100-percent renewable energy practices by 2020 or sooner. In this paper, I examine the effect of a company’s announcement of joining the RE100 initiative on the company’s stock returns immediately following the announcement, testing whether this relationship varies depending on firm, announcement, and time attributes. I hypothesize that a company’s decision to utilize renewable energy increases stock returns for consumer-based companies—especially for those that announce at a major climate convention—but that this relationship is weaker for non-consumer and energy-intensive companies, which incur greater direct costs and reap less consumer-based benefits. A preliminary model examining RE100 companies alone suggests that an RE100 announcement improves the abnormal returns of companies in the consumer services, healthcare, oil & gas, and industrials industries, but has a reduced—and sometimes negative—effect on companies in other industries, especially the basic materials industry. Moreover, under a more rigorous matched difference-in-differences (DID) model, the effect of an announcement on consumer-based industries (including both goods and services) is positive, while non-consumer-based industries on average experience a negative, though statistically insignificant effect. Disaggregated further by industry using DID, positive announcement effects hold for consumer services, health care, and industrials industries, while the effects on the oil & gas industry and basic industrials industry become negative and positive respectively. Results are inconclusive for heterogeneity in announcing at climate conventions, and no time-based trends are apparent. The results suggest that the effect of a renewable energy commitment on shareholders is contingent upon the energy-intensity and consumer exposure of an industry, but not on the publicity or time of announcement.

I express my sincere gratitude to Professor Bob Cumby for his helpful suggestions throughout the duration of this research. I would also be remiss not to thank Professor Rod Ludema for his advice on the initial model of this paper. I am indebted to Professor James Habyarimana for his useful insights into further refinement of the model as well as his general research mentorship. Thank you to Dean Mitch Kaneda for inspiring me to pursue economics, and to my parents for supporting me in every endeavor. Any remaining errors are my own. Thesis submitted in partial fulfillment of the requirements for Honors in International Economics at the Walsh School of Foreign Service, Georgetown University. Email: olb3@georgetown.edu.
Gentrification and its Impact on Disadvantaged Households in London

TRISTAN BYRNE
University of Warwick

ABSTRACT
In this paper I attempt to estimate the effect gentrification has on the rate of neighbourhood outmigration for different demographic groups. I take a cross sectional approach to my data analysis using a variety of different sources, including a household survey conducted by the Greater London Authority in 2002, house price data, and demographic statistics from the 1991 and 2001 Censuses. I find that households in the private rental sector are most likely to move out as a result of gentrification. I also provide moderate evidence that the reasons people are moving out are consistent with notions of displacement. This builds on the previous work by academics such Lance Freeman (2016), Jacob Vigdor (2002) and Rowland Atkinson (2001).

1 The author is incredibly grateful to Dr. Ali Moghaddasi Kelishomi for his unwavering support and for always bringing humour to our seminars. The author is also grateful for the valuable advice and support from Dr. Gianna Boero.
1. Introduction
In June 2017 a fire broke out in Grenfell Tower, a block of social housing apartments in West London. This resulted in a tragic loss of 71 lives, and served as a reminder that low income families are finding it increasingly difficult to find safe, good quality, and affordable housing in the UK’s capital. One key aspect of this housing crisis is gentrification. This is the process whereby a traditionally working-class neighbourhood experiences an influx of wealthy residents, which often leads to increased rents for the incumbent residents (Glass, 1964).

This paper attempts to examine the effect gentrification has on outmigration for economically disadvantaged residents in London. My hypothesis is that disadvantaged households – particularly households in the private rental sector – will face higher rates of outmigration in gentrifying areas compared to their counterparts in non-gentrifying areas, primarily due to increased rent. This study takes a cross-sectional approach to analysing this hypothesis. The main source of data is from the Greater London Authority Household Survey, 2002, which asked 8,158 residents about their housing situation. The key dependent variable is whether respondents report they are likely to move in the next five years or not. I match this data with neighbourhood house price data and demographic data from the 1991 and 2001 censuses, which inform us whether a neighbourhood is gentrifying or not. Previous studies have taken a similar approach to this (Atkinson, 2001; Vigdor, 2002; Freeman et al., 2016 etc.), but have only found weak evidence of gentrification induced outmigration. This paper, however, finds robust evidence that gentrification leads to outmigration, particularly for households in the private rental sector. These results are more compelling than previous papers due to the fact that previous studies have tended to use datasets that don’t focus on urban populations and aren’t intended to record outmigration. This paper also investigates the reasons given for households moving. This provides moderate evidence that outmigration is consistent with notions of displacement.

2. Literature review
The sociologist Ruth Glass coined the term ‘gentrification’ in 1964. It was used to describe working class neighbourhoods experiencing an influx of wealthy residents. Originally this was considered an interesting quirk of urban geography but it has now become widespread (Butler and Hamnet, 2009). One contentious issue is whether or not gentrification increases outmigration of the incumbent residents. Most papers (Atkinson, 2001; Freeman et al., 2016 etc.) describe this as ‘displacement’, however I mostly avoid this term because it implies the move is forced. While it may be forced for some households, other households will simply prefer lower rent or will be moving for reasons unrelated to house prices. However, in section 5, I examine whether the outmigration is indeed due to displacement.

This literature review will examine the evidence for gentrification induced outmigration. The empirical evidence for this from previous studies is – at best – mixed.

2.1 Mechanisms leading to outmigration
There are a variety of different mechanisms through which gentrification can cause outmigration. Most papers (Atkinson, 2001; Vigdor, 2002; Freeman et al., 2016 etc.) take into account the conventional mechanism, whereby increased demand from incoming residents leads to increased house prices and increased rents. This leads to outmigration for incumbent renters, as they prefer cheaper rent. It also leads to outmigration for home owners as they have better outside options for housing, because their current house has increased in value.

Economists have identified additional mechanisms that lead to outmigration. Freeman and Braconi (2004) observe that in gentrifying areas of cities with rent stabilisation policies, landlords often decrease the quality of their services to their tenants or harass them, because they can only increase rent when tenants move out. Freeman (2011) and Butcher and Dickens (2016) conduct interviews with large samples of residents in gentrifying areas. They find that original residents often feel pressure to move out because the new services (e.g., expensive shops and cafés) don’t cater to their needs.

2.2 Empirical studies
Most empirical studies of gentrification and outmigration follow a similar methodology to this paper: gentrifying areas are identified using census/house price data and migration patterns are observed through survey data. Outmigration rates are
then compared in gentrifying and non-gentrifying areas. Atkinson (2001) uses the ONS Longitudinal Study to find high rates of outmigration of working-class people in gentrifying areas of London. However, Atkinson fails to include suitable life cycle controls and his geographical spatial units are only disaggregated to borough level. As such, this study is severely limited. Freeman et al. (2016) provide slightly stronger evidence for displacement. He applies a multivariate model to the British Household Panel Study (BHPS) at the more acute geographic unit of Lower Super Output Areas. He finds moderate evidence that low-income households have higher outmigration rates in gentrifying areas. However, Vigdor (2002) found that disadvantaged households are less likely to experience outmigration in gentrifying areas. This uses data from the American Household Study (AHS) in the Boston area. These results are consistent with studies by McKinnish et al. (2010), Ellen and O’Reagan (2011) and Freeman (2005), who use survey data to find similar results for the rest of the US. Similarly Ding et al. (2016) use a dataset of people’s credit ratings and find no evidence of residents with bad credit being disproportionately affected by outmigration. Freeman et al. (2016) argue that one reason for these counterintuitive results is that gentrification is associated with improved neighbourhood quality, so people are less likely to want to move.

One limitation of these studies is that they use data that wasn’t intended to measure urban displacement. Many use national surveys, meaning there are relatively few observations in metropolitan areas. For example, the BHPS used by Freeman et al. (2016) only has approximately 900 households observed in London (iser.essex.ac.uk, 2017).

Many studies also fail to use sufficiently small geographical units. Some studies use borough level statistics (Atkinson, 2001; Freeman and Bracconi, 2004). However, common sense tells us that you cannot identify gentrification from such large areas.

The data is also limited because many studies use the AHS and similar surveys, which sample housing units rather than families. Displacement is thus observed when a new family moves into a unit. This excludes cases when households are being renovated or demolished, as is often the case with gentrification. This may be a substantial problem, given that for every sample period typically 15% of the units from the previous period are ineligible or otherwise missed the interview (U.S. Census Bureau, 2007).

In contrast, this paper uses a survey of 8,158 households specifically in London. The survey links the observations to the ward level geographical unit, and the purpose of the questions were to assess London’s housing situation.

### 2.3 Quasi-empirical studies

Sims (2016) uses public records in LA to identify four areas with clusters of evictions. He then conducts a qualitative analysis of these areas and found that they all experienced phenomena consistent with gentrification, such as increased investment from financial institutions and property intermediaries. DeVerteuil (2011) identified 81 social service facilities intended for low income residents in gentrifying areas of London and LA such as foodbanks and job centres. Through interviewing their employees, he found that 21% were displaced and 69% experienced displacement pressure, partly due to the displacement of their clientele.

### 3. Data

#### 3.1 Greater London Authority (GLA) Household Survey, 2002

The GLA Survey conducted 8,158 face-to-face interviews with the head of the household or their partner. It was commissioned by the GLA to inform policy decisions in a wide variety of areas such as housing affordability, poverty and health. The key dependent variable from this dataset is whether the respondent reports that they are likely to move in the next five years or not, which I use as a proxy for outmigration. This is not a perfect proxy, as some households will have predicted incorrectly. However, moving house is a big life decision which people have fairly good foresight about.

The survey asks the respondent the reasons they expect to move which I examine in section 5. It also includes useful socio-economic information about the household such as ethnicity, household income, and housing tenure.

This survey was based on a random sample of pre-selected addresses, within a non-random sub-sample of wards, which
were designed to over represent disadvantaged areas. This selection bias is controlled for through the ‘pweights’ option in Stata.

The geographical unit in this dataset is wards, which have a population of approximately 10,000. This is a suitable neighbourhood size to examine gentrification and migration patterns. I match the individuals from this dataset with ward-level aggregate statistics from Land Registry data and Census data.

It is not ideal that this dataset is from 2002, as it may be less relevant for today. However, 1995 to 2008 was a period of dramatic house price inflation in London, so it is interesting to study in the context of gentrification. Additionally, there are few other surveys focused on housing that have acute geocoded variables available, due to privacy issues.

3.2 HM Land Registry Price Paid Data
HM Land Registry Price Paid Data tracks all residential property sales in England and Wales from 1995 onwards. Cumulative house price inflation by property type (flat, semi-detached, terraced etc.) is calculated in each ward from 1995 to 2001. This ward level house price inflation value is matched to each respondent in the survey data. Due to data constraints the sample period of the house price data (1995 to 2001) does not align to the sample period of the census data (1991 to 2001). This should not significantly affect my results because most house price inflation in the 1990s occurred after 1995 (Nationwide, 2018).

3.3 Census Data
From the 1991 and 2001 Censuses there is data on the proportion of the population in each ward who have an undergraduate degree or equivalent, and the proportion of the population who work in professional or managerial jobs. This is matched to the respondents in the survey.

3.4 Summary Statistics

<table>
<thead>
<tr>
<th>Table 1: Neighbourhood descriptive statistics</th>
</tr>
</thead>
<tbody>
<tr>
<td>All London Wards</td>
</tr>
<tr>
<td>-------------------</td>
</tr>
<tr>
<td><strong>Cumulative House Price Inflation (1995 to 2001)</strong></td>
</tr>
<tr>
<td>118%</td>
</tr>
<tr>
<td><strong>Proportion with a degree, 1991</strong></td>
</tr>
<tr>
<td>18%</td>
</tr>
<tr>
<td><strong>Proportion with a degree, 2001</strong></td>
</tr>
<tr>
<td>30%</td>
</tr>
<tr>
<td><strong>Proportion in professional or managerial jobs, 1991</strong></td>
</tr>
<tr>
<td>11%</td>
</tr>
<tr>
<td><strong>Proportion in professional or managerial jobs, 2001</strong></td>
</tr>
<tr>
<td>34%</td>
</tr>
</tbody>
</table>

Table 1 illustrates that mean ward-level cumulative house price inflation was 118% from 1995 to 2001 in London. This has a standard deviation of 37%, which gives us suitable ward-level variation to investigate the effects of house price inflation. Based on census statistics, Table 1 shows that the proportion who have an undergraduate degree or equivalent grew from an average of 18% in 1991, to 30% in 2001. Similarly, the proportion in professional or managerial jobs grew from 11% to 34%.

Table 2 shows that 39% of the 8,158 households interviewed in the GLA survey reported that they were likely to move in the next 5 years. This is a fairly large amount of observations, which is important because this is the key dependent variable in my analysis. 55% of households are home owners, 12% are private sector renters and are 31% social renters. The average household income in this period is £30,700 and 31% of households have one or more non-white member of the family.
As Figure 1 shows, the probability of reporting that you are likely to move is higher in wards that experienced above average house price inflation, increasing from 41% to 44% for renters and from 33% to 37% for owner occupied households. Figure 2 shows that house price inflation was highest in central and east London, in areas around Shoreditch, Canary Wharf, and Brixton. This is consistent with local knowledge of the area.

![Figure 1: Probability that you report being likely to move](image1.png)

![House Price Inflation by Ward](image2.png)
4. Linear Probability Model
I use a linear probability model (LPM) to predict the probability of a respondent reporting that their household is likely to move. An LPM is used over a probit as it makes the interpretation of interactive coefficients simpler.

The key independent variable is ward level cumulative house price inflation. This serves as a good measure of gentrification because increased house prices are a key mechanism that causes an increase in outmigration. House price inflation may lead to outmigration for home owners, because they have better outside options if they sell their house. It may also lead to outmigration for private renters because they prefer to pay lower rent. Social renters, however, will not necessarily see their rent increase in line with the market rate, so they may not be affected by house price inflation. I have also outlined some behavioural and frictional mechanisms in section 2.1 of the literature review.

Based on these mechanisms, the effect of house price inflation is investigated for different groups through interactive dummy variables. Given that this study focuses on gentrification, rather than house price inflation more generally, in Columns (2) to (5) my analysis is isolated to neighbourhoods that were disadvantaged at the beginning of the sample period. This is defined as having below median levels of professional/managerial workers in 1991. This was due to the fact that many of the areas that experienced high house price inflation were areas that were already affluent at the start of the sample period, in particular West London neighbourhoods like Richmond, Chelsea and Knightsbridge.

Given that house price inflation is calculated at ward level and then matched to multiple individual households from the same ward, I use robust-clustered standard errors. Additionally, I control for life cycle variables such as age, children and marital status. When 'low income' is not the interactive term, dummies for household income band are included as controls. In summary, the specification of the LPM, in the case of Column (3) is:

\[
\text{Outmigration}_{in} = (\beta_0 + \beta_1 \text{Inflation}_{n} + \beta_2 \text{LowInc}_{i} + \beta_3 \text{Inflation}^*\text{LowInc}_{i}) \\
+ (\delta_0 + \delta_1 \text{Inflation}_{n} + \delta_2 \text{LowInc}_{i} + \delta_3 \text{Inflation}^*\text{LowInc}_{i})^* \text{Advantaged}_{n} \\
+ ... + \epsilon_{in}
\]

Where ‘Outmigration,’ indicates whether household \(i\) in neighbourhood \(n\) reported that they were likely to move. ‘Advantaged,’ indicates whether a ward was advantaged in 1991. In Table 3, only the \(\beta_j\) coefficients are reported.

Table 3 (Appendix) reports the results of this model. In Column (1) there are no interactive terms and all neighbourhoods are included. The results from this specification indicate that a 10-percentage-point increase in ward level cumulative house price inflation leads to a 0.56-percentage-point increase in probability of being likely to move (significant to a 0.1% level). This falls to a 0.39-percentage-point increase when effects are isolated to disadvantaged neighbourhoods (significant to a 10% level). While this seems like a small effect, it is important to remember that the average cumulative house price inflation across this period (1995 to 2001) was 118%.

Column (3) and (4) suggest low-income and non-white households are no more likely to be affected by inflation than others.

Column (5) reports the different gentrification effects by household tenure and the predictions of this are graphed in Figure 2. At means, private renters are 22-percentage-points more likely to move than homeowners or social renters. Private renters are also more sensitive to house price inflation. If house price inflation increases by 10-percentage-points, then the probability of moving increases by 1.1 percentage-points (significant to a 5% level) for private renters, compared to only 0.58 percentage-points for homeowners (significant to a 10% level) or -0.14 percentage-points for social renters (not significant). Column 5 has the highest percent correctly predicted, suggesting it has the best functional form of the models in Table 3. This implies tenure is important in determining a household’s sensitivity to gentrification. This is consistent with the mechanisms outlined in the previous section.
5. Reasons for outward mobility

In this section I investigate the given reasons for being likely to move. The GLA Survey asked respondents the main reasons why they expect to move. I define ‘direct displacement’ occurring if households say they expect to move for reasons such as ‘cannot afford rent’ or ‘to move to cheaper accommodation.’ ‘Not upwardly mobile’ is defined as households that expect to move, but excludes households that give reasons such as ‘to move to a better neighbourhood’. The logic behind this variable is that households will prefer to report positive reasons for moving (e.g. ‘to move in with romantic partner’), than reporting displacement. Therefore ‘not upwardly mobile’ can be seen as a broad proxy for displacement. I also include variables for households who say they expect to move to another borough or leave London. I use a simplified LPM to investigate whether each reason for moving is higher in wards with higher house price inflation. I use a simplified model because we have fewer observations of the dependent variable, therefore a more parsimonious model is desirable. The model is:

\[ X_{in} = \beta_0 + \beta_1 \text{House Price Inflation}_n + \ldots + \epsilon_m \]

Where \( X_{in} \) is reason for moving, as defined by the title of the Column.

The results from this model are reported in Table 4 (Appendix). Column (2) and (4) indicate that direct displacement or displacement out of London is not significantly affected by house price inflation. This may be because there are fewer observations so it is difficult to find significant results. This is an interesting result in itself, as it shows that reported displacement is relatively low in London. Column (3) and (5) show that a 10 percentage-point increase in house price inflation leads to a 0.46 percentage-point increase in likelihood of a non-upwardly mobile move and a 0.28 percentage-point increase in likelihood of leaving your home borough. This provides some evidence that outmigration is consistent with notions of displacement.

6. Discussion of results and concluding remarks

The linear probability model in section 4 provides robust evidence that households in the private rental sector face a higher rate of outmigration in gentrifying areas. Additionally, section 5 provides moderate evidence that some of this outmigration is consistent with notions of displacement. These results are in line with people's lived experience, as evidenced by anti-gentrification movements in London and cities across the world (Hancox, 2018).

From a free market perspective, this is simply supply and demand: low income households substituting their consumption away from expensive areas is just a form of market clearing. I don't entirely dismiss this argument, however a socially...
and ethnically diverse neighbourhood could be viewed as a benefit to society as a whole. Society gains when people from different classes and ethnic groups can grow up as neighbours. One just has to look at the counterfactual to see that racial tensions and class divisions arise when a city is highly segregated like Paris, where most of the city's poor live in the banlieue. This can be roughly translated as suburb-slums.

Model 1 and 2 suggest that families living in social housing are less likely to move house as a result of gentrification. This can be seen as a positive result as it shows that government intervention offers some protection against gentrification. However, the proportion of households living in social housing in London has been steadily falling from its peak of 35% in 1985, to 31% at the time of the survey, to only 21% in 2016 (Greater London Authority, 2017). This suggests fewer households have protection against gentrification.

There may be some benefits of gentrification. There is some evidence that residents benefit from better services (Butcher and Dickens, 2016) and lower crime (Papachristos, 2011). Increased house prices also benefit incumbent home owners, and section 4 provides moderate evidence that some home owners take advantage of this by selling their property.

Further research is needed to quantify these benefits against the costs to society. The effects of gentrification reported in this paper and other empirical studies do not show the whole picture. Lived experience shows the effect of gentrification can be damaging both to individuals experiencing displacement and to communities as a whole. As such, these results speak to the need for further research into this topic, and for policy makers to assess the availability of affordable housing.
References
Appendix

Table 3: Predictions from the LPM for reporting that you are likely to move

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
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<tr>
<td>Inflation</td>
<td>0.0556***</td>
<td>0.0390</td>
<td>0.0483</td>
<td>0.0325</td>
<td>0.0581</td>
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<tr>
<td></td>
<td>(3.41)</td>
<td>(1.93)</td>
<td>(1.51)</td>
<td>(1.35)</td>
<td>(1.94)</td>
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<tr>
<td>Low/Inc</td>
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<td></td>
<td></td>
<td></td>
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<tr>
<td></td>
<td>(-1.23)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Low/Inc*Inflation</td>
<td>0.00346</td>
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<td></td>
<td></td>
<td></td>
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<tr>
<td></td>
<td>(0.07)</td>
<td></td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>Non-White</td>
<td>-0.0404</td>
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<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(-0.89)</td>
<td></td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>Non-White*Inflation</td>
<td>0.0207</td>
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<td></td>
<td></td>
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<tr>
<td></td>
<td>(0.66)</td>
<td></td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>Private Renter</td>
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<tr>
<td></td>
<td>(1.86)</td>
<td></td>
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<tr>
<td>Private Renter*Inflation</td>
<td>0.0329</td>
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<td></td>
<td></td>
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<tr>
<td></td>
<td>(0.33)</td>
<td></td>
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<td></td>
<td></td>
</tr>
<tr>
<td>Social Renter</td>
<td>0.133**</td>
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<td></td>
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<tr>
<td></td>
<td>(2.70)</td>
<td></td>
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<tr>
<td>Social Renter*Inflation</td>
<td>-0.0720*</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(-2.00)</td>
<td></td>
<td></td>
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</tbody>
</table>

Effects isolated to disadvantaged neighbourhoods
Additional income controls
Percent correctly predicted
N
8158
8158
8158
8158
8158

* p < 0.05, ** p < 0.01, *** p < 0.001

Table 4: LPM predictions for different reasons of moving house

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
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<td>0.00501</td>
<td>0.0462**</td>
<td>-0.00510</td>
<td>0.0279***</td>
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<tr>
<td></td>
<td>(5.14)</td>
<td>(1.15)</td>
<td>(3.13)</td>
<td>(-0.61)</td>
<td>(3.55)</td>
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<td>N</td>
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<td>8158</td>
<td>8158</td>
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<tr>
<td>Obs. of</td>
<td>3144</td>
<td>156</td>
<td>1678</td>
<td>671</td>
<td>447</td>
</tr>
</tbody>
</table>

* p < 0.05, ** p < 0.01, *** p < 0.001
Eliminating the Penny in Canada: An Economic Analysis of Penny-Rounding on Grocery Items

CHRISTINA CHEUNG
University of British Columbia

ABSTRACT

Canada has eliminated the penny from circulation and adopted a nearest-nickel rounding scheme since 2013. Theoretically, if every final digit of the price of a good has an equal probability of being “0” to “9”, the net effect of rounding is zero. However, this is not the case, since most prices end in “8” or “9”. For instance, approximately 82.5% of goods in a representative convenience store (Lombra 2001) and 60.8% of this dataset end in “9”. This suggests zero net effect may be unlikely, which gives rise to the possibility that – when the penny is rounded to the nearest nickel – penny-rounding noticeably adds up to advantage either the firms or the consumers.

In this paper, price data from representative Canadian grocery stores are used to assess whether the current rounding system imposes a monetary loss on firms or consumers. Specifically, I evaluate how one-to-ten-item purchases and the six different Canadian provincial sales tax rates influence penny-rounding. The results show that penny-rounding financially benefits the firms at the expense of the consumers, imposing a net transfer of approximately $3.27 million CAD from consumers to grocery vendors every year. This amount averages to $157 of additional revenue for a typical grocery store per year.
Bounded Rationality in Rules of Price Adjustment and the Phillips Curve

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London School of Economics and Political Science

ABSTRACT
This paper presents a model of endogenous bias in rules of price adjustment that allows one to analyse the behaviour of inflation and output continuously throughout the entire spectrum of rationality, from one end to the other. Specifically, it proposes an alternative microfoundation for both the New Keynesian sticky-price and the sticky-information Phillips Curve by considering a possibility where price setters are constrained by the length of the time horizon over which they can form rational expectations, and they use the growth of past prices at the rate of the central bank’s inflation target as a heuristic alternative in place of their own expectations beyond this horizon. Three interesting results emerge. First, how price setters form inflation expectations and whether these expectations are accurate or heterogeneous do not matter when they are able to gather information or change prices more frequently. Second, should policymakers expect private agents to similarly adopt the inflation target as a nominal anchor for their own expectations, then even the choice of this numerical target could prove to be pivotal to output stabilization. Third, larger degrees of bounded rationality increase the persistence of inflation, and, under sticky-information, raise the possibility of discontinuous jumps and oscillatory dynamics of inflation and real output.

¹ EC331 thesis prepared in partial fulfillment for the degree of B.Sc. Econometrics and Mathematical Economics, and in participation for the XVII Carroll Round at Georgetown University and the 2018 New Economic Talent at CERGE-EI. I am immensely grateful to A. W. Phillips Professor Ricardo Reis for advising me on this work, and for always being a beacon of inspiration to me in the field of macroeconomics. I am also thankful towards Dr. Matthew Levy for offering insightful comments on the part for behavioural economics, and towards Dr. Judith Shapiro for constant support. This work will not be possible without my family's perpetual belief in me, and also if not for the support of Kenneth Tan, Benjamin Toh and Fan Jia Rong who gave comments to an initial draft. Contact: s.ding5@lse.ac.uk.
The Effects on Social Welfare of Emergency Low Emission Zones: an Application to Barcelona

BEATRIZ GARCIA QUIROGA
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ABSTRACT
Research shows that air pollution from road traffic is responsible for a substantial amount of deaths and diseases in today’s world. Can environmental policy make urban mobility sustainable? We review road traffic air pollution health impact assessment methods and propose a methodology that focuses on emergency low emission zones that target NO2 emissions from road traffic. We apply this approach to the Zona de Baixes Emissions (ZBE) policy, implemented in Barcelona, and estimate variations in emissions, concentration, exposure, expected mortality and morbidity. Finally, we provide a valuation of the overall economic savings generated by the policy and we test its efficiency. Results show that the policy achieves compliance of NO2 regulative standards, and does so in an efficient manner. Furthermore, both mortality and morbidity are reduced.
1. Introduction
It is estimated that, by 2050, 70% of the world population will live in cities (ISGlobal, 2016). The advantages of such a change in lifestyle are widely acknowledged. Glaeser & Maré, (2001) cite easier access to education, health services, and job generation. However, recent developments in science and technology are unveiling its costs.

Among them, air pollution is one of the major concerns. The World Health Organization estimates that around 3 million deaths per year are due to ambient air pollution (WHO, 2014). Other studies have reported direct effects between air pollution and brain development at young ages (Amoly et al., 2014). Taking into account that nowadays 92% of the global population lives in places where air pollution exceeds recommended standards, the rapid urbanization process calls for solutions.

Current policies mostly target average yearly levels of pollution. As a consequence, low emission zones in cities have proliferated in the latest years, and many studies have reported their effects around the globe, such as in London, Oslo or Hong Kong (for all, see Carslaw and Beevers, 2002; and Ellison, Greaves and Hensher, 2013). However, several cities that comply with average yearly standards for NO$_2$ or PM$_{10}$ instead suffer from high pollution peaks in particular days. Yet in contrast, emergency low emission zones, which are enforced only during pollution peaks, are still a rare phenomenon. Barcelona has recently provided a case to study this phenomenon. Mueller et al. found in 2016 that almost 20% of causalities in Barcelona could be avoided through proper urban planning. Pollution causes almost 5% of all preventable deaths in Barcelona. Moreover, the current deviation from international pollution standards has a cost of as much as 2,100 million € per year. Research by public authorities shows that road traffic is responsible for 64.6% of the NO$_2$ and 53% of PM$_{10}$ in the city. Pollution patterns demonstrate that Barcelona complies with average yearly standards for NO$_2$. However, pollution levels can peak above the standards for short time periods during ‘pollution episodes’ between 1 and 3 times per year. As a response, local policymakers have implemented the “Zona de Baixes Emissions de Barcelona” (henceforth the “ZBE policy”). It consists of road traffic restrictions to highly-polluting vehicles during pollution episodes, activating an emergency low emission zone during highly-polluted days. It will be implemented from December 1st, 2017 to December 31st, 2019. From 2020 on, bans are intended to be permanent.

In this paper, we estimate the effects of an emergency low emission zone (ELEZ) implemented in Barcelona on NO$_2$ levels and life quality using health impact assessment methods. To do so, we review the efficiency and efficacy of health impact assessment methods applied to road traffic air pollution. Afterwards, we construct a methodology to estimate health impacts and economic savings from variations in road traffic levels, based on a full-chain approach.

Our findings show that an ELEZ as implemented in Barcelona is effective and efficient. NO$_2$ concentrations decrease below the required standard. Mortality and acute morbidity are reduced. Overall, the policy will yield savings of 40,672,430.06 euros.

This study is relevant because it provides evidence on how emergency low emission zones increase social welfare. It provides a methodology to quantify such benefits and delivers a full cost-benefit analysis. Such methodology can also be applied to any policies that target NO$_2$ road traffic emissions.

This paper is organized as follows. In section 2, we review road traffic air pollution health impact assessment methods and its use in the existing literature. Section 3 presents our methodology and its application to the ZBE policy. Under section 4, we present our results. Section 5 summarizes and introduces further considerations.

2. Road Traffic Air Pollution Health Impact Assessment Methods
Air pollution health impact assessment methods have in common the use of a full-chain approach to assess the effects of...
of policies on indicators of health. In general terms, a full-chain approach tries to reproduce the steps taken by polluting agents from their release into the atmosphere until their assimilation by the human body, when they may produce a certain health impact (see figure 1). This health impact can be economically valued through various approaches. As a result, air pollution health impact assessment methods provide an estimate for variations in emissions, concentration, exposure, health impacts and economic benefits or losses produced after a change in the area of interest.

![Figure 1. Full-Chain Approach](source: author's production)

After 25 years of research, the scientific community has agreed on the use of air pollution health impact assessment methods as the criteria to assess the impact of changes in air pollution concentration on the environment and in life quality (Grau, Deu and Künzli, 2009). Environmental Public agencies in the United States (EPA), European Union (EEA) and the UK (DEFRA and COMEAP) have developed a substantial number of methodologies and data to generate and contrast air pollution policy now used by governmental bodies at the national, regional and local level and in academia. However, these methods differ substantially in various steps of the impact chain, such as the scope of geographic area being analyzed; the choice of pollutants whose effects are being quantified; the tools used to estimate emissions, concentration, and exposures of pollutants; the estimation of the effects exposures have on health; the health effects studied and the valuation methods used to quantify health impacts. Obviously, these choices depend on data availability and cost constraints, which in the end determine the degree of sophistication each method attains.

In the following sections, we provide an analysis of the most relevant methods within each step of the full-chain.

### 2.1 Emissions

For the ambit of air pollution impact assessment methods, emissions are polluting molecules of a gas or microscopic particles. NO₂, NOₓ, PM₁₀, PM₂.₅, and O₃ drive the majority of the existing literature regarding air pollution (for all, see Department of Environment Food & Rural Affairs, 2015; Kampa and Castanas, 2008; Kim, Kabir and Kabir, 2015; Mabahwi, Leh and Omar, 2014). For road traffic air pollution, the relevant agents are NO₂ and PM₁₀, which are released in the plume of driving vehicles.

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2 As further examples, see ExternE for US, I-PA approach for UK (Department for Environment Food & Rural Affairs, 2015) and INTRARESE, CAFE and IEHIA projects for EU (Environment Agency of Austria, 2012; European Environment Agency & Cooperative programme for monitoring and evaluation of the long-range transmission of air pollutants in Europe, 2007; IEHIAS, n.d.; IEHIAS & INTARESE project, 2007).

3 The UK has developed substantial research regarding the use of different methodologies according to this criteria, with especial emphasis on the damage cost and abatement cost methodologies. For more information, see (Department for Environment Food & Rural Affairs, 2015; Department for Environment Food and Rural Affairs (DEFRA), 2013).
Emissions from road traffic depend on the number of active vehicles, the distance driven, and the efficiency of each engine within an area and time frame. There are two main options to quantify emissions:

(i) **Empirical methods** identify all vehicles driving within an area and make actual measurements of the content of their plume when driving through strategic points during a period of time. The assessment of the number of particles released is often done by remote sensing devices that read the plate of the vehicle and measure its speed, acceleration, and emissions using absorption spectroscopic techniques (see Puerto, Castells, Bigas and Lao, n.d. for an example).

(ii) **Estimates** allow for the quantification of emissions by using more or less sophisticated formulas. The standard formula is (IEHIA, n.d.; Iñiguez et al., 2011):

\[
E_t = e_{cool} + e_{warm} + e_{cov}, \quad \text{with } e_{warm} = n_j \times m \times E(v); (1)
\]

and where:
- \(e_{warm}\) emissions when the engine is warm,
- \(e_{cool}\) emissions when the engine is still cold,
- \(e_{cov}\) emissions from evaporation,
- \(n\) number of cars within the same category, most of the times according to Euro directives,
- \(m\) km driven per day,
- \(E(v)\) Emission rate of the vehicle according to its speed.

Both \(e_{cov}\) and \(e_{cool}\) are not considered in most studies, as their impact is negligible. To estimate \(E(v)\), the scientific community has developed emission factors for certain types of engine (see Iñiguez et al., 2011 for an example of the use of emission factors).

Compared to estimates, empirical methods are highly precise. However, they have important cost-efficiency drawbacks and therefore are not recommended for rough estimations or for the analysis of policies with low expected impacts.

2.2 Concentration

Estimating the changes in concentration due to changes in emissions is possibly the most challenging part of health impact analysis methodology. This is due to the number of factors that affect how particles are dispersed in the environment, making these relations strongly-nonlinear and rather complex. For instance, at the local scale, the effect of a certain emission in a street is mostly a function of the configuration of the traffic network and meteorological conditions (IEHIA & INTARESE project, 2007). Illustratively, pollution episodes in Barcelona are strongly correlated to weather conditions with very light wind or anticyclonic periods (Ajuntament de Barcelona, Cristina Castells).

To estimate variations in ambient concentrations due to external inputs, such as the amount of road traffic, two general approaches can be recognized:

(i) **Atmospheric dispersion models**, which use mathematical and numerical techniques to simulate the physical and chemical processes involved in the dispersion of pollutants. The most widely used are Lagrangian models, namely Gaussian, and Eulerian models.

(ii) **Statistical models**, which ignore the intervening processes and focus on representing the relationship between the source and concentrations at a receptor using empirically-informed formulae or statistical functions. The most reliable models within this category are geostatistical interpolation models (GIM) and land use regression models (LUR), as they take into account geographical and meteorological conditions (see Beelen et al., 2013 for an example of LUR models).

The major problem of statistical models compared to dispersion models is that they rely on available monitoring networks to assign an overall concentration to all subjects living in a certain area. Therefore, they oversee the relevant variations on concentrations within those areas (Lee et al., 2014). Moreover, atmospheric dispersion models tend to be more reliable for estimations of short-term (daily) variations in concentration than statistical models due to data constraints. Consequently, when possible, air quality models are to be favored for impact assessment, because they provide more reliable predictions (IEHIA, n.d.).
Nevertheless, both types of models are costly to develop and require certain expertise to be used. For this reason, environmental agencies have promoted the development of screening models that incorporate such models in user-friendly platforms.

### 2.3 Exposure

Paustenbach represented exposure as "the product of concentration, time and duration or rate of transport of the toxicant". This is the current general approach, with the formula

\[ Ex = \frac{1}{T} \sum_{i=1}^{K} C_i t_i \]  

(2)

and where,
- \( K \) is the number of different environments (i) in which an individual is exposed to the pollutant,
- \( C \) is the concentration of a pollutant in a certain environment,
- \( t \) is the time spent by individuals in a certain environment, and
- \( T \) is the total amount of time of interest.

General practice for studies in urban environments assigns a value of 1 to ambient exposure (see Mueller et al., 2016; Pérez Grau et al., 2009), since if concentration has been properly quantified and only urban ambient pollution is regarded concentration equals exposure.

### 2.4. Health impacts

The standard estimation of health impacts from air pollution exposure values requires three steps: (I) selecting health impact measures, (II) estimating its relative risk, and (III) estimating expected cases.

#### 2.4.1 Selecting health impact measures

The choice of relevant health impacts depends on the nature of the policy or change analyzed. The time period over which the exposure is changed determines whether the policy will only produce short-term or acute effects, or long-term or chronic effects should also be accounted for. A project can quantify mortality, morbidity or disease, or both. Once relevant impacts are identified, they can be measured in several ways. This choice is also case-specific and should depend on available data and the nature of the health impact quantified. The most common health measures in air pollution health impact assessment studies are rough increases in mortality and/or morbidity cases and healthy life expectancy.

#### 2.4.2 Estimating its relative risk

For the scope of health impact assessment methods, the relative risk is the increase in the likelihood to develop a health effect after a variation in exposure. It is estimated as

\[ RR = \exp (\beta \Delta Ex) \]  

(3)

where
- \( \beta \) captures the causal relationship between exposure to a certain pollutant and a particular health impact, and
- \( Ex \) is exposure.

\( \beta \) is estimated by means of the so-called Exposure-response functions (also ERF or CRF). Each ERF is specific for a pollutant and a health impact. Studies usually extract ERFs from peer-reviewed literature, as their derivation requires meta-data analysis and is thus very costly.

#### 2.4.3 Estimating expected cases

Expected cases are estimated by multiplying mortality and/or morbidity rates in the reference area by the population attributable fraction (PAF) obtained. The PAF is the proportional reduction in mortality that would occur if exposure to
the risk factor (i.e. pollution) was reduced or increased to an alternative scenario (World Health Organization 2015a). Its standard formula is

\[
\text{PAF}_{\text{pop}} = \frac{P_p(RR-1)}{P_p(RR-1)+1} \quad (4)
\]

where,
- \( P_p \) is fraction of the population exposed to air pollution, and is often assumed as 1, and
- \( RR \) is the relative risk.

If results are expressed in terms of healthy life expectancy they must be transformed according to standard life-table methods.

### 2.5 Economic valuation

In our view, monetizing health impacts is the most controverted part of full-chain models for obvious reasons. It is well-known that valuations of the burden of life and illness can vary substantially among individuals, and therefore it is difficult to provide a well-grounded estimation of those values. In the literature, two general approaches prevail:

1. **the Cost of illness approach (COI)** estimates the material costs related to mortality and morbidity. Valuations often consider medical costs, loss of income and productivity.
2. **the Willingness to pay approach (WTP)** measures how much money an individual would be willing to pay to avoid a particular health state. Research under this approach has focused on quantifying the cost of mortality. Among the valuations developed, the Value of Statistical Life (VoSL) seems to be the most accepted.\(^4\)

An advantage of COI valuations compared to WTP is that they consider social costs. On the other hand, WTP valuations include individual preferences and therefore certain indefinable costs (e.g. pain, quality of life). Furthermore, WTP values also appear to be fairly stable in Western countries (De Hollander, 2004), although they have shown to be dependent on income.

Both COI and WTP valuations can be estimated through statistical analysis or taken from existing literature. The main drawback of statistical analysis is that for most studies it may be too costly. As with emissions data, public authorities may have the higher incentives to perform such measurements.

### 3. Our Approach

We have estimated decreases in mortality, respiratory diseases and cardiovascular diseases attributable to decrease in emissions of NO\(_2\), and its corresponding economic savings for the 2-year period from 1st of December of 2017 to 2020, using a full-chain approach. Details on methods used are specified in the following sections.

#### 3.1 The ZBE policy

As previously stated, the ZBE policy consists of access restrictions to the Barcelona urban area. Restrictions are applied to highly-polluting vehicles during days of extreme pollution called pollution episodes. The urban scope is the area colored in dark blue in figure 2.

Barcelona has to comply with the air quality standards set by the European Union.\(^5\) Under current EU regulation, the average NO\(_2\) concentration for a year in Barcelona shall not surpass 40 \(\mu g/m^3\). Daily, reported concentrations cannot be above 200 \(\mu g/m^3\) more than 18 times. In Barcelona, the EU limit was surpassed 4 times in 2016. A pollution episode is registered and the ZBE restrictions are activated every time two or more measuring stations in Barcelona report NO\(_2\) concentrations above 200 \(\mu g/m^3\).

The policy will be implemented in two stages, from 1\(^{st}\) of December of 2017 to July 2018, and from then on to the 31\(^{st}\) of

4. For further information on VoSL, see Lindhjem, Navrud, & Braathen, 2016.
December of 2019. After that, restrictions are intended to be permanent. In the first stage, vehicles affected will be gasoil powered cars and registered before 2006 (Euro 1, 2 and 3), gasoline-powered cars registered before 2000 (Euro 1 and 2), and pre-Euro 1 vans. In the second stage, all Euro 1, 2 and 3 vans will also be banned. Vehicles exempted from the ban are motorbikes, trucks, buses and public service vehicles.

To ease the identification of the restricted vehicles, all those not affected by the ZBE policy have been labeled according to their emissions. For this reason, in this paper we refer to restricted vehicles as unlabeled vehicles. Compliance is controlled by the local police and sanctions imposed are fines, which can be accumulated by violators with no limit.

### 3.2 Geographical scope

This study focuses on the areas of Barcelona affected by the ZBE policy. The city, which is located on the northeastern coast of Spain, has 1,608,746 inhabitants living in an area of 101 Km$^2$ (Ajuntament de Barcelona, 2016). Barcelona is divided into 10 districts, that are in turn divided into quarters. Quarters of Zona Franca, Vallvidrera, Tibidabo and Les Planes, are excluded from the policy and thus not considered.

Research undertaken by local authorities shows that road traffic emissions are responsible for 64.6% of the NO$_2$ and 53% of PM$_{10}$ in the city. In this study we focus on NO$_2$ variations, as ZBE restrictions depend on NO$_2$ levels and, to our knowledge, experts have not found a clear relation between concentration levels of NO$_2$ and PM$_{10}$.

Roads affected by the policy include 98.16% of the existing network and absorb 80.03% of road traffic. Notoriously, Rondes, which are excluded from the ZBE area, absorb 20.73% of traffic within 1.89% of the existing network (Agència d'ecologia Urbana de Barcelona, 2013).6

The majority of private vehicle traffic is due to commuters coming from surrounding areas of the city (57.5% compared to 42.5% of intraurban traffic). Roughly 80% of car trips are individual (Agència d'ecologia Urbana de Barcelona, 2013). Compared to 2009, the average age of the vehicle fleet has increased, and a big amount of it is powered by gasoil, which is known to be more polluting than gasoline. As a result, road traffic emissions have increased.

Regarding meteorology, Barcelona has a Mediterranean climate, with an average temperature of 18oC and high levels of humidity. These features favor air circulation and therefore promote the dispersion of pollutants. As a result, average levels of pollution hardly ever surpass the yearly EU limit of 40 μg/m$^3$, in opposition to other nearby cities such as Madrid. However, low precipitation rates, seasonal African winds and anticyclonic periods interact with the narrow design of traffic network (see Figure 3), favoring pollution episodes. Illustratively, the measuring station’s network reports that the average yearly concentration of NO$_2$ has stabilized below 40 μg/m$^3$ in the last five years, while the daily limit of 200 μg/m$^3$ is still surpassed in some areas. Choosing a worst case scenario, we have assumed that 3 pollution episodes will occur per year and last for a day.

### 3.3 Emissions

Emissions per day in the Barcelona urban area have been estimated using a hybrid method. The vehicle fleet and its emissions were characterized using empirical methods. This data was used as an input for the standard function (1) together with additional data. For further detail, see Appendix A and Section 4.

The dataset regarding the characterization of the vehicle fleet was obtained from a report provided by Agència de l’Energia de Barcelona, a public institution depending on the Barcelona City hall (Àrea Metropolitana de Barcelona, 2017). The report provides the classification of vehicles driving within the Barcelona metropolitan urban area on weekdays from March to May 2017 according to several features, such as the type of vehicle, geographical area, power source, euro category and, most relevantly, DGT label. Additionally, it provides average emissions measured for vehicles within the aforementioned categories. We used the dataset to derive the number of vehicles in Barcelona and classify them according to their power source and type. We also obtained the average emissions for unlabeled vehicles. For further information, see Table A of Appendix A.

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6 Measured in drivable kilometers.
Data on average kilometers driven per day per type of vehicle was obtained from *Pla de Mobilitat Urbana de Barcelona 2013-2018* (Agència d'ecologia Urbana de Barcelona, 2013).

### 3.4 Concentration

Concentrations were calculated using SCREENVIEW, a software platform running the screening model SCREEN3. The software provides estimates of "worst-case" 1-hour concentrations for a single source in an area with the inputs shown in Figure 4,

![SCREENview inputs](image)

*Source: Author's Production, 2017.*

Source release height was estimated at 0.3 m. The emission rate was estimated dividing the aforementioned emissions by the seconds in a day and the area of Barcelona. The area of Barcelona affected by the ZBE policy was assimilated to a rectangle with an exposure map crafted by Consorci Sanitari de Barcelona in 2017 (see Appendix B). Meteorology is determined according to Pasquill-Gifford Stability Classes, which represent six levels of atmospheric stability and the weather conditions related to them. According to insolation and wind speed in Barcelona, the assigned classes were B-C. We computed the average value of both weather conditions, detailed in Appendix B.

### 3.5 Exposure

As this study only focuses on urban ambient concentration in Barcelona (more specifically, in the areas of it affected by the ZBE policy), exposure equals concentration. As mentioned in section 2.3, this is consistent with general practice for studies in urban environments (see Cesaroni et al., 2012; Mueller et al., 2016; Pérez Grau et al., 2009).

### 3.6 Health impacts

After analyzing peer-reviewed literature and best available data we selected three health impacts:

(i) All-cause mortality,  
(ii) Cardiovascular illnesses, and  
(iii) Respiratory illnesses.

Mabahwi et al., 2014 provides evidence on the relation between NO$_2$ levels and selected health impacts. Only acute effects (short-term) are considered, as the ZBE policy is not expected to generate long-time reductions in exposure.

To estimate the relative risk, we used the Exposure-response function dataset created as part of the EU-funded INTARESE project. The dataset is provided in Appendix C. The project derived a set of 'core' ERFs for a range of environmental stressors and health outcomes, mainly targeted at issues covered by the seven case studies in the INTARESE project. This dataset is considered best available data for three main reasons: (1) it includes all of the variables, and thus minimizes double counting, (2) it is one of the more up-to-date existing datasets (from 2010), and (3) it is derived from meta-analysis,
and therefore it is more precise.\textsuperscript{7}

In order to calculate the RR exposure difference used the formula developed by Mueller et al. This is adaptation standard RR formula and was derived the air pollution study in Barcelona they conducted in late 2016, and therefore it was considered the best available method. The formula is:

$$\text{RR Ex}_{\text{difference}} = \exp(((\ln(\text{RR}))/\text{ex}_{\text{before}}) \times (\text{ex}_{\text{after}}))$$  \hspace{1cm} (5)

where,

- \text{RR} is the relative risk,
- \text{Ex}_{\text{before}} is the exposure before the implementation of the ZBE policy, and
- \text{Ex}_{\text{after}} is the exposure after the implementation of the ZBE policy.

Afterwards, we estimated attributable cases as described in section 2.4. We estimated PAFs for each district of Barcelona and derived mortality, respiratory hospital admissions, and cardiovascular hospital admissions daily rates for the whole city. Data for mortality was obtained from \textit{Agència de Salut Pública de Barcelona}. Data for respiratory and cardiovascular hospital admissions was obtained from Pérez et al., 2009. As a result of these calculations, we obtained the number of health events that are reduced per day and for the whole period in which the policy is implemented.\textsuperscript{8} RR, PAFs, and mortality and morbidity rates are shown in Appendix D.

### 3.7 Economic savings from health impacts

Valuations for the selected health impacts were extracted from peer-reviewed literature. We selected the most specific valuations available for Barcelona. The economic valuation for mortality used is the WHO Regional Office for Europe VoSL approach (3,202,968 euros for Spain in 2012). As we are only accounting for acute health impacts, we used the cost of a hospital admission in Barcelona (2,100 euros in 2006), as reported by Pérez et al., 2009 for hospital admissions. Values were transformed to 2017 prices using the IPC annual increase.\textsuperscript{9} Global savings resulting from the implementation of the policy were computed by bringing 2017 values forward using a standard future value formula with compound interest. The interest rate chosen was the TEDR for households as of September 2017.\textsuperscript{10}

### 4. Results

Recall that we estimated the changes in NO\textsubscript{2} emissions and concentration, health impacts and social savings that arise from the implementation of the ZBE policy in Barcelona (see section 3 above). Table 1 summarizes our main results and includes some of the information that is after developed in Tables 2, 3, 4 and in the Appendixes. Table 2 describes the change in emissions, Table 3 shows health impacts and Table 4 displays the savings generated from implementing the policy.

#### Table 1. Summary of results.

<table>
<thead>
<tr>
<th>Vehicles affected</th>
<th>Emissions per day (g)</th>
<th>Concentration (µg/m\textsuperscript{3})</th>
<th>(\Delta)Mortality (cases)</th>
<th>(\Delta)Resp. dis. (cases)</th>
<th>(\Delta)Cardio. dis. (cases)</th>
<th>Savings (€)</th>
<th>Savings per capita (€)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Steady state</td>
<td>26826675.8</td>
<td>212.25</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>Stage 1</td>
<td>23,304,019.19</td>
<td>184.4</td>
<td>-3.52</td>
<td>-1.29</td>
<td>-0.58</td>
<td>11,388,280.42</td>
<td>7.079</td>
</tr>
<tr>
<td>Stage 2</td>
<td>23,274,983.51</td>
<td>183.7</td>
<td>-9</td>
<td>-3.28</td>
<td>-1.49</td>
<td>29284149.64</td>
<td>18.20</td>
</tr>
<tr>
<td>Total</td>
<td>23,274,983.51</td>
<td>183.7</td>
<td>-12.52</td>
<td>-4.57</td>
<td>-2.07</td>
<td>40,672,430.06</td>
<td>25.28</td>
</tr>
<tr>
<td>(\Delta) from steady state</td>
<td>-13.24%</td>
<td>-13.45%</td>
<td>-4.84%</td>
<td>-0.77%</td>
<td>-0.35%</td>
<td>40,672,430.06</td>
<td>25.28</td>
</tr>
</tbody>
</table>

\textsuperscript{7} Nevertheless, it should be noted the dataset does not include any later developments.

\textsuperscript{8} From 1/12/2017 to 31/12/2019.

\textsuperscript{9} IPC variation used was 1.6% (October 2017).

\textsuperscript{10} TEDR stands for “Tipo efectivo definición restringida”, and is equivalent to TAE (“Tasa Anual Equivalente”) excluding bank fees. The value used was 1.3%. It was obtained from Banco de España as the 2017 rate.
Result 1. The ZBE policy affects 12.38% of vehicles in the first stage and 15.89% of vehicles in the second stage. We have identified 10,020 polluting vehicles, meaning 15.89% of the total vehicle fleet. This finding implies that restrictions overall will affect 12,125 individuals, as the occupation rate per vehicle in Barcelona is 1.21 (Agència d'ecologia Urbana de Barcelona, 2013). Private cars are the most affected by the policy, capturing 81.13% of all restrictions. Notoriously, results show that there are no vans affected in the first stage, while in the second stage restrictions extend to 2,213,44, that represent 3.51% of the total vehicle fleet.

Table 2. Vehicles affected and emissions.

<table>
<thead>
<tr>
<th>Type of vehicle</th>
<th>Active vehicles</th>
<th>Emissions (grams)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Absolute</td>
<td>%</td>
</tr>
<tr>
<td>Unlabeled private cars</td>
<td>7,624.07</td>
<td>12.09</td>
</tr>
<tr>
<td>diesel</td>
<td>5,068.18</td>
<td>8.04</td>
</tr>
<tr>
<td>gasoline</td>
<td>2,555.90</td>
<td>4.05</td>
</tr>
<tr>
<td>Unlabeled vans</td>
<td>2,213.44</td>
<td>3.51</td>
</tr>
<tr>
<td>diesel</td>
<td>2,160.30</td>
<td>3.43</td>
</tr>
<tr>
<td>gasoline</td>
<td>53.14</td>
<td>0.08</td>
</tr>
<tr>
<td>Vans older than Euro 1</td>
<td>0.00</td>
<td>0.00</td>
</tr>
<tr>
<td>Unlabeled taxis</td>
<td>182.88</td>
<td>0.29</td>
</tr>
<tr>
<td>diesel</td>
<td>175.96</td>
<td>0.28</td>
</tr>
<tr>
<td>gasoline</td>
<td>6.92</td>
<td>0.01</td>
</tr>
<tr>
<td>Pollutants (first period)¹¹</td>
<td>7,806.95</td>
<td>12.38</td>
</tr>
<tr>
<td>Pollutants (second period)¹²</td>
<td>10,020.39</td>
<td>15.89</td>
</tr>
<tr>
<td>Non pollutants</td>
<td>53,040.61</td>
<td>84.11</td>
</tr>
<tr>
<td>Total vehicles</td>
<td>63,061.00</td>
<td>100.00</td>
</tr>
</tbody>
</table>

Result 2. With the ZBE policy, emissions are reduced around 13.2% per day. As the third and fourth column of Table 3 show, emissions of NO₂ per day are reduced in 3,522,656.61 grams of NO₂ during the first stage, and in 3,551,692.29 grams during the second stage. Therefore, when excluding highly polluting vehicles, traffic emissions decrease by 13.24% per day.

Result 3. The policy succeeds in decreasing NO₂ concentration below 200 µg/m³. According to the third column of Table 1, NO₂ concentration during pollution episodes decreases 13.47% with the ZBE policy. Recall that to estimate concentration we used SCREEN3, a screening model that relies on a Gaussian dispersion model. We found that, without the policy, NO₂ levels are of 212.25 µg/m³ during pollution events. During the first stage, NO₂ concentrations decrease to 184.4 µg/m³, and during the second stage, concentration drops to 183.7 µg/m³. Therefore, NO₂ concentration is already reduced below the daily limit of 200 µg/m³ during the first stage of the policy. These results show that pollution episodes will not last for more than a day with the ZBE Policy.

Result 4. Daily mortality is reduced by 4.84%, acute respiratory hospitalizations decrease in 0.77% and acute cardiovascular hospitalizations drop 0.35%.

¹¹ Recall that first period comprises 01/12/2017-31/06/2017 and includes unlabeled taxis private cars and pre-Euro 1 vans.
¹² Recall that second period comprises 01/06/2017-31/12/2019 and includes unlabeled vans, taxis and private cars.
Table 3 describes the obtained number of cases and their respective weights. The ZBE restrictions imply 2.01 casualties less per day, 0.73 hospitalizations less due to acute respiratory diseases and 0.33 hospitalizations less due to acute cardiovascular diseases. In other words, after being implemented for two years, the ZBE policy is expected to avoid 12.52 casualties, 4.57 acute respiratory episodes, and 2.07 acute cardiovascular attacks.

The largest change displayed in Table 3 occurs in mortality. Remarkably, 4.84% of preventable casualties in a day are caused by the reported difference in NO$_2$ concentration. This is consistent with the findings of Mueller et. Al (2016). Of course, this is overall a minor fraction of yearly mortality, which is expected to decrease by 0.04% due to the ZBE policy (see Column 3 of Table 3). It is to be noted that this results cannot be fully translated to a permanent restrictions scenario, as restrictions would affect NO$_2$ concentration in the steady state, and therefore policy savings would be lower. However, they provide valuable insights on the potential effects that permanent restrictions could have on mortality and morbidity in Barcelona.

Table 3. Attributable Cases per Health Event.

<table>
<thead>
<tr>
<th></th>
<th>Mortality</th>
<th>Out of all cases (%)</th>
<th>Resp. disease</th>
<th>Out of all cases (%)</th>
<th>Card. disease</th>
<th>Out of all cases (%)</th>
<th>Total population</th>
</tr>
</thead>
<tbody>
<tr>
<td>Per day</td>
<td>2.01</td>
<td>4.84%</td>
<td>0.73</td>
<td>0.77%</td>
<td>0.33</td>
<td>0.35%</td>
<td>1,608,746</td>
</tr>
<tr>
<td>From 1/01/2017 to 31/12/2019</td>
<td>12.52</td>
<td>0.04%</td>
<td>4.57</td>
<td>0.006%</td>
<td>2.07</td>
<td>0.0028%</td>
<td></td>
</tr>
</tbody>
</table>

Source: author’s computation, 2017.

Result 5. Overall, the policy generates 40,672,430.06 euros on social savings, meaning 25.28 euros per capita.

In section 3 we estimated the savings generated by the ZBE policy. We display our findings in Table 4. The largest savings are due to the decrease in mortality, that alone amounts to 40,658,250.47. In turn, respiratory diseases and cardiovascular diseases only account for 0.035% of total savings. Each day that restrictions are put in place generates savings of 6,507,588.81 euros. Taking into account that we assumed a worst-case scenario for our calculations, savings could be cut in half with more favorable conditions.

Table 4. Savings Generated by the ZBE Policy.

<table>
<thead>
<tr>
<th>District</th>
<th>Population</th>
<th>Mortality (€)</th>
<th>Respiratory dis. (€)</th>
<th>Cardiovascular dis. (€)</th>
<th>Total (€)</th>
<th>Per day (€)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Total</td>
<td>1,608,746</td>
<td>40,658,250.47</td>
<td>9,760.22</td>
<td>4,419.37</td>
<td>40,672,430.06</td>
<td>6,507,588.81</td>
</tr>
</tbody>
</table>

Source: author’s computation, 2017.

A deep analysis of the costs of this policy was out of the scope of this paper. However, a back-of-the-envelope calculation suggests that net benefits are positive. The ZBE policy has two main costs: implementation costs and the individual cost for each citizen that cannot use his vehicle during a pollution episode. The official budget for the policy is 654,500 euros (Consell de Plenari Ajuntament de Barcelona, 2016). Let us assume that all of it has been spent. Knowing that restrictions apply to 10,020.39 vehicles (see Table 2) and that the occupancy ratio for private vehicles in Barcelona is 1.21, we can estimate that the policy affects 12,124.67 individuals, forced to find an alternative mean of transport during pollution episodes. It turns that, for the policy not to be beneficial, the cost of that round trip should be higher than 638.98 euros. What is more, even assuming a worst-case scenario in which a whole family was affected, costs per person should be higher than 528 euros. Needless to say, it is difficult to imagine a situation that would cause such costs.

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13 See Appendix D for further information regarding selected mortality and morbidity rates in Barcelona.
14 Overall savings-budget=net savings/days of pollution episode=savings per day/individuals affected= individual savings per day.
5. Summary and Conclusions

In this paper, we analyzed the effects that ELEZ have on air quality and citizens. To do so, we developed a methodology using road traffic air pollution health impact assessment methods. Afterwards, we applied this methodology to the ZBE policy in Barcelona. We estimated the impacts of the expected traffic restrictions on NO\textsubscript{2} levels from 2018 to 2020 in the city. We calculated the decrease in casualties, acute respiratory episodes and acute cardiovascular episodes. Finally, we computed the social savings that arise from the policy using WTP and COI methods.

Our analysis provides 5 main results:

(i) The ZBE policy affects 12.38% of vehicles in the first stage and 15.89% of vehicles in the second stage,
(ii) With the ZBE policy, emissions are reduced by around 13.2% per day,
(iii) The policy succeeds in decreasing NO\textsubscript{2} concentration below 200 µg/m\textsuperscript{3},
(iv) Daily mortality is reduced by 4.84%, acute respiratory hospitalizations decrease in 0.77% and acute cardiovascular hospitalizations drop 0.35%, and
(v) Overall, the policy generates 40,672,430.06 euros on social savings, meaning 25.28 euros per capita savings.

However, these results present some limitations. Our findings are quite sensitive to the number of pollution episodes per year. Indeed, every day in which restrictions are put in place generates savings of 6,507,588.81 euros. As we noted, the number of pollution episodes per year fluctuates. Therefore, taking into account that we assumed a worst-case scenario for our calculations, savings could be cut in half with unfavorable conditions for NO\textsubscript{2} dispersion. It should also be noted that this policy is implemented simultaneously with many others.\textsuperscript{15} Thus, the ZBE policy may end up having smaller effects if other measures succeed in decreasing NO\textsubscript{2} average concentration. On the other hand, we only quantified the effect of NO\textsubscript{2} emissions, as ZBE restrictions depend on NO\textsubscript{2} levels and, to our knowledge, experts have not found a clear relation between concentration levels of NO\textsubscript{2} and PM\textsubscript{10}. As road traffic is estimated to be responsible for 53% of PM\textsubscript{10}, it is likely that the effects on air quality overall are higher than estimated. Finally, we acknowledge that savings from acute cardiovascular and respiratory episodes may be underestimated, as the valuation measure used does not account for the burden caused to the individual (e.g. pain, stress).

Nevertheless, the main strength of this study is that it provides evidence to support that, even in a worst-case scenario, an ELEZ is likely to yield substantial benefits for citizens. The detailed emissions data compared to previous studies strengthens the internal validity of the analysis. Additionally, it provides valuable insights on the potential effects that implementing permanent restrictions on polluting vehicles could have on mortality and morbidity. Last, but not least, the methodology we developed can be applied to any policies that target NO\textsubscript{2} emissions and is thus scalable to other settings and policies.

With this study, we do not intend to prove that ELEZ are the most efficient policy to counteract pollution episodes. Doing so would require comparing the cost and benefits of such policy with those of other measures. Furthermore, the existing literature regarding cost-assessment of policies that target road traffic air pollution is fairly limited. As a consequence, a cost-assessment methodology comparable to road traffic air pollution health impact assessment methods has not been accepted. This could be a natural path for future research. Furthermore, in this study, we considered that emissions did not depend on exogenous variables, such as driving patterns. It would be interesting to assess whether and how such factors affect road traffic emissions.

On an end note, it is likely that the ELEZ allocates costs on people with lower incomes who tend to own older, more polluting, vehicles. The larger question is whether a policy targeting pollution should also look to be redistributive, or those effects should be offset with specific redistributive measures.

\textsuperscript{15} The city hall alone has devised 8 lines of action in order to end with pollution from 2017 on, which add up to the (already mentioned) existing initiatives from the local to the supranational level.
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Disappearing Jobs and Displaced Workers: 
Does Education Matter?

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ABSTRACT
This paper exploits state and time variations in the U.S. Current Population Survey for the years 1983-2016 to examine the extent to which job polarization can account for the widening gap in labour force participation rates by education. The use of the Bartik shift-share instrument is a new method in the US literature used to isolate exogenous shocks to labour demand for middle-skill workers. Estimation by 2SLS suggests that the decline in the employment share of middle-skill occupations has had an economically and statistically significant effect on the labour force participation differential by education. In fact, the econometric evidence indicates that much of the observed divergence in participation rates since 1983 can be accounted for by job polarization. On the other hand, growth in alternative job sectors could only marginally compensate for those effects.

¹ I thank Mario Sanclemente Villegas for his superb assistance during the research process. I am greatly indebted to Thijs van Rens for his excellent help with refining the econometric model and particularly the construction of the Bartik shift share instrument. I am grateful to Gianna Boero for her continued guidance to the entire EC331 class as well as her encouragements in private conversations.

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1. Introduction
Automation, globalisation and female emancipation radically transformed the labour market in the U.S. and worldwide. With a focus on prime-age men (aged 25-54) this paper examines two of the more striking trends during 1983-2016 that have disproportionately affected the less educated: first, the polarization of job opportunities away from middle-skill towards low- and high-skill occupations, and second, the widening gap in labour force participation by education.

1.1 Job Polarization
Recent insights by Autor et al. (2006, 2010), Cortes et al. (2014), Goos et al. (2007) and Acemoglu et al. (2011) highlight that certain demographic groups experienced a dramatic fall in the demand for their labour. Notably, unskilled men previously employed in routine-based jobs such as manufacturing workers, mechanics and information clerks - the so-called middle-skill occupations - saw their jobs disappear.

Pioneering research by Autor et al. (2003), Autor et al. (2010) and Goos et al. (2007) sheds light on the underlying factors behind this widely observed reduction in job opportunities. As technology gets more advanced, middle-skill occupations characterised by procedural, rule-based activities could be coded into computer programs (Autor et al. 2010); and as international economies get ever more integrated, tasks could be offshored relatively effortlessly (see Autor et al. 2010, Goos, Manning and Salomons, 2010). From the 1980s onwards, those changes in labour market fundamentals have been manifested in contracting opportunities in middle-wage, middle-skill white and blue-collar jobs primarily occupied by less-educated workers, coupled with expanding opportunities in both high-skill, high-wage and low-skill, low-wage jobs (see Autor et al., 2010, 2006; Goos and Manning, 2007; Goos, Manning and Salomons, 2009; Acemoglu and Autor, 2011).

1.2 Labour Force Participation
Concurrently, the share of prime-age American men that either work or actively seek employment has declined drastically. Participation fell to 88 percent in 2016 from a high in 1954 of 98 percent (Council of Economic Advisers, 2016). Most troublesome is the widening gap by education, with less-educated males experiencing a particularly steep fall in labour market activity. Most strikingly, the fall in participation for the less educated accelerated in the late 1980s, when the effects of job polarization were gradually materialising.

1.3 Putting the trends together
A large body of literature has emerged discussing the main drivers of declining participation. Some authors emphasise supply-side factors such as the increasing opportunity cost of work (Winship, 2017) and deteriorating health conditions (Krueger, 2016). This paper challenges this view and argues for a primarily demand-driven explanation, adding to the works of Juhn & Potter (2006), Autor et al. (2010), and the Council of Economic Advisers (2016). Evidence from this paper lends concrete econometric support to the notion that declining job opportunities were a substantial driver of diverging participation rates by education.

The theoretical model behind these findings follows the marginal cost and marginal benefit considerations proposed by Becker (1965). Faced with falling job opportunities in the middle-skill sector, many of the less educated prime-age men previously employed in those disappearing occupations fail to acquire the requisite skills to transition into high-skill employment or refuse to accept a lower wage offer for low-skill jobs. As those less educated men see their marginal benefit from labour market activity fall, many temporarily or permanently drop out of the labour force, whilst the highly educated remain largely unaffected. Workers who switch to a job in growing low-skill occupations such as retail or personal services will most likely displace other less educated men otherwise employed in lower skill jobs. Hence, job polarization is thought to have exacerbated the labour force participation differential by education for prime-age males over the past forty years.

1.4 Evidence from the Literature
Exploratory evidence in the literature (see Aaronson et al., 2014; Autor et al., 2010; Tüzemen, 2018 and Cortes et al., 2014) supports the theoretical prediction of a strong impact of job polarization on participation rates for less educated males in the U.S. However, recent findings from Europe covering 1996-2013 (Verdugo et al., 2017) call into question the
The universality of such relationship. Using an IV approach to disentangle the exogenous demand-driven changes in middle-skill employment, Verdugo et al. (2017) find no significant effect on the participation of prime-age men with less than a high-school education, the group with the largest decline in participation and job opportunities. That would imply two distinct labour market responses to falling job opportunities in Europe and the U.S. and suggest that other factors (i.e. regional or institutional) may influence participation decisions also.

This paper contributes in several ways to the existent literature. Foremost, it refines the existent body of work in the U.S. by making a clear distinction between demand and supply-driven responses using a Bartik shift share instrument. To the author's best knowledge, no paper has yet attempted to do so for U.S. data. Further, previous evidence on the relationship between job opportunities and participation has largely been exploratory and descriptive in nature. Thus, it is one of this paper’s main objectives to determine quantitatively how much of the divergence in participation by education can be explained by the disproportionate decline in the labour demand for less educated men. This is also novel in the literature, which has only indirectly examined the participation wedge.

This paper supports the demand-side view that the polarization of job opportunities for prime-age men had a substantial, economically and statistically significant effect on – and can, in fact, explain much of – the participation gap by education. Results indicate that growth in the alternative occupations, the low-skill jobs, marginally compensated for the decline in middle-skill occupations. Finally, this paper bridges the gap between contrasting results on the European and U.S. level in the literature. Confirming the direction and significance of the effects reported by previous work on the U.S., this paper indicates varied experiences in Europe and the U.S. and suggests that regional factors and policies may play a role in transmitting global shocks to labour demand. In a wider context, this paper illustrates a vital mechanism through which many American men fell behind their more highly educated countrymen and highlights important potential drivers behind the recent rise in populist movements.

2. The Data

This paper draws on the male prime-age subsample from the IPUMS U.S. Current Population Survey (Flood et al., 2017) for the years 1983-2016 for 50 states, excluding the District of Columbia. The time period has been chosen to approximately coincide with the onset of the observed polarization of the labour market. In this framework, prime-age men constitute the demographic group of interest to abstract from other, potentially distorting factors influencing the participation decision (i.e. retirement, educational uptake, female emancipation, etc.). Moreover, prime-age men have historically had the greatest labour force attachment and the highest employment shares in middle-skill occupations of all demographic groups. This naturally makes them an interesting subject for analysis.

The dataset was chosen for several reasons. As the primary source of U.S. labour force statistics with a sample size of 60,000 households per month, it contains not only relevant information on an individual’s labour force status, but rich demographic and occupational data as well. This allows for a detailed examination of changes in the demand for specific occupations. Further, as a stratified, independent state sample, CPS data can be used to reliably calculate state-year shares using appropriate weighting. This enables the state-year level repeated cross-section analysis on which this paper’s methodology is predicated.

As a caveat, repeated redesigns of the survey slightly complicate the comparability over time for some variables and the sample size might not be sufficient to produce reliable estimates for small sub-groups within states. The methodology below has been adopted to either circumvent or correct for those limitations.

2.1 Occupations

At the core of this paper lies the definition of middle-skill jobs and overall job opportunities to the less educated. The following outlines two classifications of those occupations.

---

3 E.g. participation rates are adjusted for the 1994 CPS Redesign following Polivka & Miller (1998)
2.1.1 By Skill Content (Autor et al. 2010 and Acemoglu et al. 2011)

- High-Skill: Managerial; Professional; Technician
- Middle-Skill: Office Administration; Precision Production; Craft & Repair; Operators
- Low-Skill: Low Skill Services; Protective Services; Extraction; Transportation

As illustrated in Table 1, the share of middle-skill jobs in the U.S. economy fell dramatically, whilst low and high-skill occupations saw increases in their employment share.

<table>
<thead>
<tr>
<th>Year</th>
<th>Low-Skill</th>
<th>Middle-Skill</th>
<th>Low &amp; Middle-Skill</th>
<th>High-Skill</th>
</tr>
</thead>
<tbody>
<tr>
<td>1983</td>
<td>0.155</td>
<td>0.518</td>
<td>0.673</td>
<td>0.327</td>
</tr>
<tr>
<td>1990</td>
<td>0.159</td>
<td>0.517</td>
<td>0.675</td>
<td>0.325</td>
</tr>
<tr>
<td>2000</td>
<td>0.161</td>
<td>0.489</td>
<td>0.650</td>
<td>0.350</td>
</tr>
<tr>
<td>2010</td>
<td>0.194</td>
<td>0.451</td>
<td>0.648</td>
<td>0.352</td>
</tr>
<tr>
<td>2016</td>
<td>0.187</td>
<td>0.437</td>
<td>0.624</td>
<td>0.376</td>
</tr>
<tr>
<td>Total Change</td>
<td>+3.2ppt</td>
<td>-8.1ppt</td>
<td>-4.9ppt</td>
<td>+4.9ppt</td>
</tr>
</tbody>
</table>

2.1.2 By Task Content (Cortes et al., 2014)

- Routine Manual: Precision Production; Craft & Repair; Operators, Labourers
- Routine Cognitive: Sales; Office Administration; Other Clerical Occupations
- Non-Routine Manual: Private Household; Protective Services; Other Service Jobs
- Non-Routine Cognitive: Managerial; Professional; Technician

Like middle-skill jobs, routine manual jobs experienced the most drastic decline (see Table 2).

<table>
<thead>
<tr>
<th>Year</th>
<th>Routine Manual</th>
<th>Routine Cognitive</th>
<th>Non-Routine Manual</th>
<th>Non-Routine Cognitive</th>
</tr>
</thead>
<tbody>
<tr>
<td>1983</td>
<td>0.429</td>
<td>0.167</td>
<td>0.079</td>
<td>0.325</td>
</tr>
<tr>
<td>1990</td>
<td>0.423</td>
<td>0.169</td>
<td>0.085</td>
<td>0.323</td>
</tr>
<tr>
<td>2000</td>
<td>0.401</td>
<td>0.163</td>
<td>0.089</td>
<td>0.348</td>
</tr>
<tr>
<td>2010</td>
<td>0.360</td>
<td>0.171</td>
<td>0.119</td>
<td>0.350</td>
</tr>
<tr>
<td>2016</td>
<td>0.348</td>
<td>0.164</td>
<td>0.115</td>
<td>0.373</td>
</tr>
<tr>
<td>Total Change</td>
<td>-8.1ppt</td>
<td>-0.3ppt</td>
<td>+3.6ppt</td>
<td>+4.8ppt</td>
</tr>
</tbody>
</table>

The two occupational groupings are closely related, which is reflected in the data, yielding a strong correlation between the share of middle-skill jobs and the share of routine-manual occupations (ρ=91.66%). A similar calculation for the non-routine manual and the low-skill occupational shares also shows a close relationship (ρ=83.28%).

In further analysis, this paper defines the total share of job opportunities available to the less educated sub-population as the sum of middle- and low-skill employment shares, or, likewise, as the sum of routine manual and non-routine manual employment shares. A simple calculation of the correlation between these two shares of aggregate job opportunities highlights that the two definitions are very closely related (ρ=95.07%). Routine manual and non-routine manual occupations combined proxy the variation in low and middle-skill job shares fairly well. It follows that the total job opportunities of less educated men can be modelled in two ways: declining routine-based and growing service-based jobs.
3. Methodology
To understand how long-run changes in demand for middle-skill occupations widened the participation gap by education, this paper adopts a modified version of the methodology used in Verdugo et al. (2017) and exploits variations in key variables across states and time in a state-year panel setting. The baseline model takes the following fundamental forms:

\[
\text{GAPPR}_{st} = \alpha_s + \alpha_t + \phi \times \text{year} + \beta \times \text{mskill}_{st} + X' \theta + \varepsilon_{st}
\]

\[
\text{GAPPR}_{st} = \alpha_s + \alpha_t + \phi \times \text{year} + \beta \times \text{mskill}_{st} + X' \theta + \varepsilon_{st}
\]

where the dependent variable \( \text{GAPPR}_{st} \) is the state-level difference in participation by education, \( \text{mskill}_{st} \) is the employment share in middle-skill jobs, \( \text{Lmskill}_{st} \) is the aggregate share of low- and middle-skill jobs, and \( \varepsilon_{st} \) is the residual error term. Covariates in \( X_{st} \) include shares by state and year of demographic groups by age, education, and ethnicity\(^4\), and capture the effect of changing demographics. The model also contains state-fixed effects \( \alpha_s \), time-fixed effects \( \alpha_t \) and regional-specific\(^5\) deterministic trends \( \phi \) to flexibly account for shocks in participation rates over time and across states.

Using an alternative definition to test the impact derived from technological shocks manifested in the decline of routine manual occupations, the following models are estimated:

\[
\text{GAPPR}_{st} = \lambda_s + \lambda_t + \delta \times \text{year} + \gamma \times \text{routine\_man}_{st} + X' \phi + \eta_{st}
\]

\[
\text{GAPPR}_{st} = \lambda_s + \lambda_t + \delta \times \text{year} + \gamma \times \text{routine\_man}_{st} + X' \phi + \eta_{st}
\]

where \((\text{non})\text{routine\_man}_{st}\) is the share of (non-)routine manual occupations.

3.1 Identification
The primary empirical challenge is the identification of exogenous changes to labour demand. Models (1) to (4) will yield biased estimates since they fail to disaggregate labour market shocks into its demand and supply driven components. Changes to the institutional setting may induce disproportionate labour supply responses that a naive estimation by OLS would capture as a demand effect, given that only the general equilibrium is observed. To isolate the exogenous variation in the demand for the respective occupational employment shares this paper uses a variant of the Bartik shift share instrument (Bartik 1991). The instrument exploits variations in the initial structure of local labour markets in 1983 across states. The initial occupational composition of local markets has made states more or less vulnerable to the long-run trends of globalisation and automation that have polarized the labour market since the 1980s. As these shocks largely occurred at the national and global level, variation from these sources should be exogenous to the unobserved local state-level factors that simultaneously determine the participation rates and employment changes across states. Such a Bartik instrument is constructed as follows:

\[
L^d_{st} = \sum_{k} \text{Share}_{k,83} \times \delta_{nk,83}
\]

where \( \text{Share}_{k,83} \) is the initial composition of occupations \( k \) across states in 1983 and \( \delta_{nk,83} = \text{Share}_{nk,83} / \text{Share}_{k,83} \) is the national growth in the employment share for occupations \( k \) at time \( t \) using 1983 as the base year. To exploit the distinction between job polarization within and between industries documented by Tüzemen & Willis (2013), occupations are defined as industry-specific. To identify the instrument, this paper interacts individual occupations with a total of 15 industries. Since in 1983 job polarization had not yet been in full process, the initial composition of occupations should be exogenous to later changes to both employment and labour force participation. National growth rates, which are primarily driven by global trends, are likely to be orthogonal with local factors when also controlling for state- and time-fixed effects. Therefore, the shift share instrument as detailed above should isolate the exogenous variation in employment coming from national and global sources.

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4 15 demographic groups - 5 educational × 3 age - and controls for ethnic composition
5 Regional grouping follows the official definitions of the Bureau of Economic Analysis. A regional trend is used as a state-deterministic trend kills off too much variation in the first stage for 2SLS to be consistent.
4. Results

The following section establishes the main empirical implications of job polarization with regards to its displacement effect. In particular, the estimation results discussed below provide concrete econometric evidence of a positive relationship between the decline in middle-skill job opportunities and the gap in participation by education. To allow for a more in-depth analysis, I shall provide a brief exposition of a few particularities of some of the specifications shown in the tables below.

Table 3 reports this paper’s main results, including estimations by 2SLS along with their corresponding reduced form coefficients. In columns (2) and (5), the respective aggregate employment shares ‘low+middle’ and ‘manual’ are instrumented by their two composite Bartik instruments. In the analysis that follows, I will refer to this identification strategy as the ‘2 instrument Bartik’. In columns (3) and (6), only variations from middle-skill and routine manual Bartik instruments are used to predict changes in the aggregate shares. I will refer to this strategy as the ‘1 instrument Bartik’. The important distinction between the two identification methods lies in the interpretation of their results. Estimates in columns (2) and (5) show the displacement effect of the declining share of total job opportunities to the less educated as predicted by changes from both its composite employment shares (low and middle-skill), whereas estimates in columns (3) and (6) report the impact of the falling total share as explained by the declining occupations exclusively. Finally, model (7) splits up the aggregate employment share of manual occupations into their constituent shares and includes them separately.

Table 3 reports the Kleinbergen-Paap rk Wald F statistics for a test of weak instrument identification. The test statistics indicate that the constructed Bartik instruments are indeed strong predictors of observed changes in employment shares.

Table 3. Job Polarization and Participation Gap

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
<th>(7)</th>
</tr>
</thead>
<tbody>
<tr>
<td>GAPPR GAPPR GAPPR GAPPR GAPPR GAPPR GAPPR</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Panel A. Second Stage</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>middle-skill</td>
<td>-.511**</td>
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<td>low+middle</td>
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<td><strong>Panel B. Reduced Form</strong></td>
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<td>(.0605)</td>
<td></td>
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<td>routine manual</td>
<td>-.146**</td>
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<td>Bartik</td>
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<td>non-routine</td>
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<td>manual Bartik</td>
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<tr>
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<td></td>
<td>1700</td>
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<td>1700</td>
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<td>1700</td>
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<td>1700</td>
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<td>1700</td>
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<td></td>
<td>1700</td>
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</tr>
</tbody>
</table>

**Notes:** Panel A: 2SLS regressions as detailed in the text. Panel B: Corresponding reduced form OLS regressions.

Standard errors clustered by state in parentheses

* p < 0.05, ** p < 0.01, *** p < 0.001
Proceeding with our analysis, columns (1)-(3) yield concrete econometric evidence that the relative decline in job opportunities for the less educated prime-age male sub-population indeed widened the labour force participation gap by education over the sample period. The size of the coefficients imply that these effects are substantial. A one percentage point fall in the employment share of middle-skill occupations on average increases the participation wedge by approximately 0.511 percentage points whilst a one percentage point fall in the combined employment share of middle- and low-skill jobs increases the gap by approximately 0.644-0.836 percentage points. The fact that the middle-skill share decreased on the national level by 8.1 percentage points from 1983-2016 and the combined employment share for low- and middle-skill occupations fell by 4.9 percentage points over the same period implies an estimated increase in the participation gap (from a shift in labour demand) of approximately 4.14 and 3.16-3.85 percentage points respectively. Compared to the observed rise in the average participation gap by 5.3 percentage points, this constitutes a substantial demand-driven response. The moderate to small quantitative difference in the sensitivity implied by (1) and (2) - (3) provide some suggestive evidence that the rise in the number of low-skill sector occupations only marginally compensated for the pervasive drop in middle-skill occupations. The low explanatory power of the low-skill Bartik instrument as indicated by insignificant reduced form coefficients (see Panel B) further buttresses the notion that this effect was minimal. A strong correlation between the constructed instruments for individual low- and middle-skill employment shares renders any concrete inference about the precise individual effects in the task-based approach in column (7) infeasible.

The results in (4) - (7) lend additional support to the proposed hypothesis. Following the same reasoning from above, the 7.3 percentage point fall in the national routine manual employment share approximately produced a 2.62-2.96 percentage point increase in the educational divide in participation. The effect of the decline in the manual employment share is estimated at around 1.97-2.32 percentage points. Moreover, including a separate term for the growing nonroutine manual (service) sector in column (7) indicates that this channel did not compensate for the drop in job opportunities in the routine manual sector. Reconciling this finding with the insignificant reduced form coefficients suggests that the growth of alternative job opportunities most probably only marginally, if at all, softened the displacement effect of job polarization.

The analysis above yields concrete econometric evidence that job polarization has had a substantial impact on the participation gap by education of prime-age men whilst the growth in alternative job opportunities has not been sufficient to compensate for the decline in middle-skill and routine manual jobs.

4.1 Decomposition

To decompose the effects on the participation gap, I run a series of regressions of the form:

\[
Y_{it} = \alpha_i + \alpha_t + \phi \text{year} + \beta \text{jobtype}_{it} + X'_{it} \theta + \epsilon_{it}
\]

where \(Y = \text{GAPPR}, \text{LFPR}^{LE}, \text{and LFPR}^{HE}\) and \text{jobtype} is the respective occupational employment share as defined above. The results in Panel A and B in Table 4 suggest that employment changes in middle-skill and low-skill occupations, in aggregation and separately, affected the participation rates of the less educated and substantially widened the participation gap. It is worth noting that whilst not all coefficients in column (2) are significant at the 5% significant level, only a few coefficients are not significant, indicating that a genuine relationship is likely to exist.

Dynamics are similar for the manual occupations reported in Panels C, D and E. The rise in the participation gap appears to have worked through a variety of channels. Most notably, the drastic decline in job opportunities for routine manual occupations lowered the labour force participation rate for less educated men, whereas it had a significantly smaller effect on the participation of highly educated men. This evidence is in line with this paper’s hypothesis that less educated men who have been affected disproportionately by polarised job opportunities will have disproportionate labour market responses to that decline.

\[
\text{GAPPR} \, \text{is the gap in participation by education, and LFPR}^{LE}, \, \text{and LFPR}^{HE} \, \text{the labour force participation rates for less educated and highly educated men, respectively.}
\]
Table 4. Decomposition of the Participation Gap

<table>
<thead>
<tr>
<th>Panel</th>
<th>Individual Share</th>
<th>Aggregate Share</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1) GAPPR</td>
<td>(2) LFPR\textsuperscript{LE}</td>
</tr>
<tr>
<td>Panel A</td>
<td></td>
<td></td>
</tr>
<tr>
<td>middle-skill</td>
<td>-0.511\textsuperscript{**}</td>
<td>0.464\textsuperscript{†}</td>
</tr>
<tr>
<td></td>
<td>(0.186)</td>
<td>(0.237)</td>
</tr>
<tr>
<td>Panel B</td>
<td></td>
<td></td>
</tr>
<tr>
<td>low+middle (2 instrument variant)</td>
<td>-0.644\textsuperscript{**}</td>
<td>0.534\textsuperscript{†}</td>
</tr>
<tr>
<td></td>
<td>(0.218)</td>
<td>(0.296)</td>
</tr>
<tr>
<td>low+middle (1 instrument variant)</td>
<td>-0.836\textsuperscript{**}</td>
<td>0.76\textsuperscript{*}</td>
</tr>
<tr>
<td></td>
<td>(0.27)</td>
<td>(0.345)</td>
</tr>
<tr>
<td>Panel C</td>
<td>routine manual</td>
<td></td>
</tr>
<tr>
<td></td>
<td>-0.324\textsuperscript{**}</td>
<td>0.369\textsuperscript{*}</td>
</tr>
<tr>
<td></td>
<td>(0.12)</td>
<td>(0.164)</td>
</tr>
<tr>
<td>Panel D</td>
<td>manual</td>
<td></td>
</tr>
<tr>
<td></td>
<td>-0.437\textsuperscript{**}</td>
<td>0.594\textsuperscript{**}</td>
</tr>
<tr>
<td></td>
<td>(0.169)</td>
<td>(0.223)</td>
</tr>
<tr>
<td></td>
<td>-0.515\textsuperscript{**}</td>
<td>0.585\textsuperscript{**}</td>
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<tr>
<td></td>
<td>(0.207)</td>
<td>(0.254)</td>
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</table>

Panel E. Total Opportunities Separately

<table>
<thead>
<tr>
<th></th>
<th>routine manual</th>
<th>non-routine</th>
<th>manual</th>
</tr>
</thead>
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<tr>
<td></td>
<td>-0.366\textsuperscript{*}</td>
<td>-0.113 (0.263)</td>
<td>-0.52\textsuperscript{*}</td>
</tr>
<tr>
<td></td>
<td>(0.166)</td>
<td>(0.298)</td>
<td>(0.223)</td>
</tr>
<tr>
<td></td>
<td>0.603\textsuperscript{**}</td>
<td>0.634\textsuperscript{*}</td>
<td>0.52\textsuperscript{*}</td>
</tr>
<tr>
<td></td>
<td>(0.209)</td>
<td>(0.298)</td>
<td>(0.223)</td>
</tr>
<tr>
<td></td>
<td>0.237\textsuperscript{†}</td>
<td>0.52\textsuperscript{*}</td>
<td>0.52\textsuperscript{*}</td>
</tr>
<tr>
<td></td>
<td>(0.14)</td>
<td>(0.14)</td>
<td>(0.14)</td>
</tr>
<tr>
<td>N</td>
<td>1700</td>
<td>1700</td>
<td>1700</td>
</tr>
</tbody>
</table>

Notes: 2SLS regressions include state and time-fixed effects, a region-specific trend and demographic controls for 1983-2016. Standard errors clustered by state in parentheses.

4.2 Financial Crisis and Its Aftermath
Motivated by a seminal paper by Jaimovich & Siu (2012), who document a dramatic acceleration of job polarization during recessions, it seems natural to test whether the dynamics of participation have changed correspondingly. I therefore add to my specification the interaction of the jobtype variables from (6), with an indicator taking the value one for observations post 2007 and zero otherwise. This estimation examines whether the effect of job polarization on the participation gap has been constant during and after the crisis.

Evidence for a structural break in the relationship between job opportunities and the participation gap is moderate. For most occupational classifications aside from the total share of job opportunities defined by skill content or task content (valid only for the 1 instrument variant Bartik), the post 2007 experience has not been significantly different from the pre-crisis period.

Given those mixed results, it is insightful to closer examine those regressions yielding a significant structural break. The results shown in Table 5 indicate that the displacement effect of job polarization may have changed in a rather intriguing fashion in the wake of the Great Recession; they suggest that post 2007, the displacement effect fell in magnitude. That finding could potentially reflect the fact that workers laid off during recessions have a greater attachment to the labour market than those laid off due to structural causes, as during recessions, workers may be more inclined to view changes in their job opportunities as a temporary feature of the business cycle and therefore be less (though still highly) respon-
sive to changes to their labour demand. Alternatively, workers displaced by the Great Recession might have been more inclined to re-join the labour force as economic conditions improve. This result is not robust to all occupational groupings and is to be taken solely as indicative.

Table 5. Additional Post 2007 Effect

<table>
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<tr>
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<th>(2) GAPPR</th>
<th>(3) GAPPR</th>
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<td>low-middle</td>
<td>-.879**</td>
<td>-.711**</td>
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<tr>
<td></td>
<td>(.275)</td>
<td>(.229)</td>
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<tr>
<td>(low-middle-skill)*post07</td>
<td>.203*</td>
<td>.143**</td>
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<td>(.0814)</td>
<td>(.0535)</td>
<td></td>
</tr>
<tr>
<td>manual</td>
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<td>-.576*</td>
<td></td>
</tr>
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<td>(.226)</td>
<td></td>
</tr>
<tr>
<td>manual*post07</td>
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<td>.0752*</td>
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<td></td>
<td></td>
<td></td>
<td>(.0381)</td>
</tr>
<tr>
<td>N</td>
<td>1700</td>
<td>1700</td>
<td>1700</td>
</tr>
</tbody>
</table>

Notes: Column (1) reports results using the 1 instrument variant Bartik; column (2) the 2 instrument variant Bartik and (3) the 1 instrument variant Bartik. All models include state-fixed effects, time-fixed effects, a region-specific deterministic trend and demographic controls for years 1983-2016. Standard errors clustered by state in parentheses

* p < 0.05, ** p < 0.01, *** p < 0.001

4.3 2SLS vs. OLS

This section draws attention to a final, yet perhaps surprising, finding of the above analysis. Comparing 2SLS with standard OLS coefficients indicates that the OLS coefficients are substantially biased downwards (in their absolute value). When defining occupations by task content – for example, routine manual jobs – OLS seems less biased, though substantial nonetheless. That could imply that exogenous variations from global demand sources such as automation and globalisation predict much larger effects than a naive analysis by OLS. That, in turn, could indicate the presence of various local factors that counteracted those disruptive global processes. Alternatively, it could also point to larger measurement errors of occupational codes by state level which bias the results by OLS downwards. Further research is necessary to explore this issue in greater detail.

5. Robustness Checks

In the section above, I have shown that the effect of job polarization on the participation gap by education is not contingent upon a specific classification of occupational grouping. This section in addition considers the following robustness checks:

- Estimating the models detailed in the main text with different deterministic region-specific trends, using the alternative definition provided by the CPS (see Table 6).
**Table 6. Robustness: 2SLS Results from Different Regional Grouping**

<table>
<thead>
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<th>(3)</th>
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<td>GAPPR</td>
<td>GAPPR</td>
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<tr>
<td><strong>Panel A. By Skill Content</strong></td>
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<tr>
<td>middle-skill</td>
<td>-.536***</td>
<td></td>
<td></td>
<td>-</td>
</tr>
<tr>
<td></td>
<td>(.145)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>low+middle (1 instrument variant)</td>
<td>-.86**</td>
<td></td>
<td>-</td>
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<td>(.269)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>low+middle (2 instrument variant)</td>
<td>-.849**</td>
<td></td>
<td>-</td>
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<td>1700</td>
<td>1700</td>
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<tr>
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<td>CPS</td>
<td>CPS</td>
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</table>

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<th>(6)</th>
<th>(7)</th>
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<td>GAPPR</td>
<td>GAPPR</td>
<td>GAPPR</td>
<td>GAPPR</td>
</tr>
</tbody>
</table>

| **Panel B. By Task Content** |  |  |  |     |
| routine manual | -.321** |      | -.258* |
|                | (.104)  |  | (.126) |
| non-routine manual |      | .204 |     |
|                |  | (.186) |  |
| manual (1 instrument variant) | -.466* |      |     |
|              | (.171)  |  |     |
| manual (2 instrument variant) |      | -.275* |     |
|              |  | (.134)  |     |
| Region defined by | CPS | CPS | CPS | CPS |
| N | 1700  | 1700 | 1700 | 1700 |

**Notes:** The table reports results from the same baseline regression models as in Table 3. The definition for the regional grouping for the region-specific deterministic trend in the model is taken from the original CPS data. The results are fairly robust to this change.

Standard errors clustered by state in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

- In an exclusion test, I iteratively exclude a different BEA economic region from the sample to check whether results are driven by outlier states (see Table 7).
These simple robustness checks indicate that the key results derived in the main text do not depend on specific definitions, nor are they driven by outliers in a major way.

6. Concluding Remarks

This paper has presented concrete econometric evidence that the polarization of job opportunities of prime-age men since the 1980s had a substantial, statistically significant economic effect on the labour force participation gap by education. Job polarization has been found to account for much of the observed divergence in participation rates since 1983. Having produced tractable results derived from rigorous econometric analysis treating the endogeneity of labour demand on the state level, this paper adds to the existent literature, which so far has provided a descriptive and exploratory analysis of the interplay between job polarization and labour supply trends.

This paper further suggests that the growing job alternatives in the low-skill and non-routine manual occupations most probably marginally compensated for the dramatic fall in the employment share of middle-skill jobs. Those findings have substantial implications for policymakers striving to reduce the participation wedge between educational groups. It appears that a great portion of the growing divergence in participation rates is driven by long-term structural factors with international origin, which will not reverse with economic growth. Any successful policy must first realise a root cause of the disproportionate labour supply sensitivity arises from the less educated.

This paper confirms the direction and significance of the effects presented by previous work on the U.S. A comparison with evidence from Europe raises some doubt about the universality of the presented relationship. Regional and cultural factors, economic and social policy, and a country’s institutional framework may play an important role as well. For instance, it is possible that a more generous European welfare system incentivises unemployment over nonparticipation, softening the impact.

Finally, the above analysis raises questions about the direction of the bias of the results obtained by OLS. Local demand or supply factors appear to have softened the impact of exogenous shocks to local labour demand from global sources. Alternatively, OLS results could also be driven towards zero by measurement error. Future research is necessary to explore the forces behind this somewhat surprising result.
References


Winship, S. (2017). What’s behind declining male labor force participation: Fewer good jobs or fewer men seeking them? *Mercatus Center at George Mason University*
Returns to Education on Poverty Reduction & Labor Market Outcomes

WESSAM KANES

Georgetown University, Qatar

ABSTRACT

Using CHNS data from 1989 to 2011, probit and quantile regression models are carried out to estimate the relationship between education, poverty and the non-farm labor sector at the household level within the context of rural China. Typically, returns to education are estimated at the means without a deeper examination of the heterogeneity within educational outcomes conditional on a household income distribution. This paper demonstrates that households at the lower end of the distribution experience higher gains from education as opposed to those situated in the upper quantiles. Furthermore, by estimating the effect of education on non-farm labor participation, this paper observes non-farm labor as a mechanism that transforms the returns to education into higher marginal gains in the labor market. With regards to poverty, a probit estimation model demonstrates that across working ages, higher educational attainment and more schooling significantly decrease the probability of a household being poor. To deal with the endogeneity issues present in most education models, this paper instruments for an individual’s educational attainment using their peer schooling average that is determined by the mean education of the individual’s commune, age and gender cohort.

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2 I would like to thank Dr. Daniel Westbrook for his guidance and support as well as Professor Jack Rossbach and Professor Sulagna Mookerjee for their help throughout the year. This research uses data from China Health and Nutrition Survey (CHNS). I thank the National Institute of Nutrition and Food Safety, China Center for Disease Control and Prevention, Carolina Population Center, the University of North Carolina at Chapel Hill, the NIH (R01-HD30880, DK056350, and R01-HD38700) and the Fogarty International Center, NIH for financial support for the CHNS data collection and analysis files from 1989 to 2006 and both parties plus the China-Japan Friendship Hospital, Ministry of Health for support for CHNS 2009 and future surveys.
1. Introduction

Confucius said: “Education breeds confidence. Confidence breeds hope. Hope breeds peace.” His statement captures the essence of educational reforms in hopes of alleviating poverty and increasing economic growth and social welfare. Emerging economic literature in the 90s until today claims that education is the cornerstone to development and sustained poverty reduction. This avenue of research is important to pursue for numerous reasons including the generational effects of education. In the same way that poverty easily perpetuates itself through generations, education can act in a similar way, albeit producing welfare increasing outcomes. Parents’ schooling not only impacts the household’s attitudes towards education, but also their ability to support their children whilst they are in school, making it a highly pertinent ingredient for development.

Estimating the returns to education has been and continues to be a challenge to economists because of the endogenous nature of an individual or household’s education outcomes. Unobservable characteristics such as ability are highly correlated with schooling outcomes. This creates the need to use an IV (instrumental variable) for education. Song and Card are valuable pieces of literature because of the way in which they address the need to instrument (Song, 2012; Card, 2001). Song used primary, middle and high school age cutoffs to instrument the completion of compulsory education, arguing that the policy was more strictly enforced for ages at the beginning of each schooling level (2012). Thus, the selection based on age would be exogenous and not correlated with ability and other unobservable characteristics. Song concluded that urban areas benefit more from the completion of compulsory schooling because of better quality schools versus rural areas (2012). This reaffirms that education and poverty manifest themselves differently in urban and rural contexts. Thus, it is more appropriate to evaluate urban and rural poverty separately which rationalizes the limiting of this thesis to studying solely the rural context.

My main contribution through this paper is the use of household living data provided by the CHNS (China Health and Nutrition Survey) for nine waves between 1989 and 2011 to estimate the relationship between education, poverty and the non-farm labor sector at the household level. While the majority of literature estimates returns to education at the means, I will be using quantile regressions to examine the heterogeneity in responses to education depending on the conditional household income distribution. Another contribution is observing the returns to education through the household’s participation in nonfarm labor.

In terms of empirical work, this thesis also uses a probit random-effects regression model to estimate the impact of years of schooling on the probability of being poor and the probability of one engaging in nonfarm labor. The results demonstrate that household heads with higher education are less likely to be poor throughout the ages 20 to 60. However, completion of primary school alone has little effect on poverty when compared to that of completing middle and high school. For the non-farm probit, individuals with no educational attainment start off at age 20 with a ten percent chance of having a nonfarm job. This contrasts starkly with individuals with a university degree, who start off with an 80% chance for nonfarm work that increases to 100% at around 50 years of age.

In an attempt to exploit the longitudinal nature of the data, a fixed effects model is used to determine the impact of schooling on household income per capita. A discussion of fixed effects and schooling will later reveal the limitations of using this approach as a solution to the endogeneity problem. To capture the heterogeneous response of education on household income per capita, a quantile regression model will break down the effects of schooling on household income with regards to different income quintiles. My results support the potential of education as a poverty alleviator because those at the bottom quantiles benefited more from educational attainment than those at the top.

Finally, with the use of an instrument similar to the one used in Westbrook and Hou, both the household head’s educational attainment and years of education will be instrumented for using an age, gender, commune cohort schooling average (2015). This paper will demonstrate the instrument’s relevance as well as identify the threats posed to its validity, since data limitations only allow for the creation of one instrument, thereby making it impossible to conduct endogeneity tests.

Throughout this paper, the age, gender, commune cohort schooling average will also be referred to as the peer schooling average.
Section two presents the empirical strategy while section three describes the data. Section four displays and elaborates on results, while section five concludes with a discussion on future work that can be done on the topic.

2. Empirical Specification

The empirical specification was developed to test the relationships and expected outcomes hypothesized in the previous section. The first part examines the impact of education on poverty reduction while the second focus on the relationship between education and non-farm labor.

2.1 Independent Variable of Interest

The main independent variable throughout the empirical strategy of this paper is a measure of education, denoted as $A$, either expressed as the household head’s highest level of educational attainment or their years of schooling. The reason this variable is expressed both as categories and as years is to facilitate the ability to instrument using the peer schooling averages.

Since my dependent variables are aggregated to the household level, the empirical method uses the educational attainment and years of schooling of the household head. Household heads are self-identified in the survey and are an appropriate choice of unit of observation since they are the individuals with the highest likelihood of having the most influence on household welfare. Another option would have been to use the top earner in the household, however, this could have been a potential source of bias since the top earner would be likely to more frequently change over the nine waves of the survey. Additionally, the education of household heads is more likely to be predetermined and thus not as correlated to the underlying economic environment and other unobservables that would bias the estimators in the model.

2.2 Education on Poverty

Poverty, in this paper, is determined using official rural poverty lines published yearly by the SSB (Statistical Bureau of China). Although Park and Wang discuss the discrepancies between poverty line calculation methods employed by the SSB, the official rural poverty lines are used throughout this study over international measures such as the World Bank’s $1.15 since they are better suited to the context of rural China (2001).

Based on the official rural poverty lines, I use a probit estimation technique to gauge the effect of different levels of education towards determining the poverty status of the household. The poverty status is calculated based on household income per capita at the household level. The model is calculated using individuals between the working ages of 16 (minimum legal working age in China) and 55 or 60 (minimum retirement ages in China for females and males respectively), using the following equation:

$$ Pr[P_{ict} = 1|A, Xs] = \phi[\beta_0 + \sum_{s=1}^{S} \gamma_s A_{ict} + \alpha_c + \delta_t + X'_{ict}\beta + u_{ict}] $$

where $P_{ict}$ is a binary dependent variable indicating whether or not the household head $i$ in province $c$ and year $t$ is poor and thus will equal 0 if the household (as determined by the poverty status of the household head) is not poor and 1 if it is; $\beta_0$ is a constant; $\gamma_s$ is the main parameter of interest estimating the separate effect of each level of educational completion, $s$, in determining poverty status; $s$ starts at one because the base level for estimation is $s = 0$. $A_{ict}$ is the categorical variable indicating the highest level of educational attainment of the household head $i$ in province $c$ and year $t$. $\alpha_c$ and $\delta_t$ are province and time effects. Province fixed effects are used to control for the differences in economic potential across different provinces such as the variation of money allocation to education or households that are located in provinces closer to a coastline. The time fixed-effects are used to account for inflation and other time-specific characteristics that may influence educational attainment or poverty. $X'_{i}$ is a row vector of control variables containing the ages and genders of the household head, along with household

Park and Wang (2001) explain that for the years 1984-1997, the poverty lines were calculated based on estimated necessary food expenditures and non-food expenditures using data from a 1984 national rural sample survey. The value of the food consumption basket was calculated using both planned and market prices. Over the years, the SSB adjusted for inflation but rarely documented the changes being made to their poverty line determination methods.

Retirement ages taken from Trading Economics data.
size and stratum. Age is entered into the model linearly then squared to capture the evolution of earnings over individual
i’s life cycle. Including a gender indicator controls for the differences in income between males and females; Gender
t, is equal to 1 if the individual is female and 0 if male. Although the sample comprises only households in rural China, the
stratum variable indicates whether the household is located in a village as opposed to a town. Including this controls for
the household’s market access in the sense that within rural areas, some households may be located in smaller villages
with less trade whereas others are closer to more dynamic locations with more traffic and better economic potential. The
final component of equation (1) is \( u_{ict} \) which is interpreted as the heteroskedastic random error term assumed to be
zero under the zero-conditional mean assumption.

Although the overall aim of this paper is to estimate the direct effect of education on poverty reduction and indirect
effect of it through the mechanism of non-farm labor, the non-farm labor indicator was omitted from the model since its
exclusion produces an estimator with omitted variable bias that capture both the impact of education on income alone
and the impact of education on income through its effect on non-farm labor.

Since poverty is derived from household income, a fixed effects panel regression is used to determine the effect that the
household head’s educational attainment level has on determining household income per capita. This model is useful
since it exploits the option of fixed effects with panel data that, in theory at least, is meant to address endogeneity. The
dependent variable, \( y_{ict} \), is the log of household income per capita and the rest of the equation is a replica of model (1).

\[
\log(y_{ict}) = \beta_0 + \sum_{s=1}^{S} \gamma_s A_{ict} + \alpha_c + \delta_t + X'_{ict}\beta + u_{ict} \tag{2}
\]

To examine the heterogeneity in the impact of education on household income per capita for different income quantiles
of the sample, a quantile regression is used to produce estimates for the 20th, 40th, 60th and 80th quantiles:

\[
Q_{\theta} [y_{ict}|x_{ict}] = \rho_\theta + \sum_{s=1}^{S} \gamma_{s\theta} A_{ict} + \alpha_c + \delta_t + X'_{ict}\beta + u_{ict} \tag{3}
\]

where \( \theta \) indicates the quantile level, \( \rho_\theta \) is the constant generated at each quantile, and \( \gamma_{s\theta} \) is the coefficient of each
educational attainment level for the household head at each quantile. The rest of the model specification is similar to
equation (1).

To examine the heterogeneous impacts of education more closely, the data is treated as a repeated cross-section, and the
above quantile model is run separately for 1989, 2000, and 2011 using equation 3 with the years of education instead of
educational attainment and no time fixed effects.

The quantile coefficients for the years 1989, 2000, and 2011 are later compared to ordinary least squares estimates
generated by equation (4) below to demonstrate the importance of examining the heterogeneity in educational impact
across income quantiles as oppose to only estimating at the means:

\[
y_{ict} = \beta_0 + \beta_1 A_{ict} + \alpha_c + X'_{ict}\beta + u_{ict} \tag{4}
\]

where \( \beta_0 \) is a constant and \( \beta_1 \) estimates the percentage change in logged income per capita, \( y_{ict} \), with every additional year
the household head has of schooling \( A_{ict} \).

2.3 Education on Nonfarm

The next part of the empirical strategy is designed to examine the effects of education on the likelihood of the household
head participating in nonfarm labor as their primary occupation. The probit estimation technique is used for the
following model to estimate the effects of education on the likelihood of the household head having a nonfarm primary
occupation:

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6 Hausman test conducted using fixed effects and random effects regressions of equation (2). P-value = 0 conclud-
ed the use of fixed effects to be most appropriate.
where \( NF_{ict} \) is a limited dependent variable made to indicate whether household head \( i \)'s primary occupation is farm (if equal to 0) or nonfarm (if equal to 1). The rest of the model specifications are similar to equation (1) except for the addition of household income and a control for different types of farm business that the household head participates in. Following the findings of Vasco and Tamayo, household wealth and land ownership are important determinants of whether or not the household primarily participates in nonfarm labor (2017). Since the data set did not provide information on land ownership, controlling for the type of farm business takes into account whether the household works on no farm, a collective farm, a household farm, or both a collective and household farm.

2.4 Instrumentation

Economic papers on estimating returns to education often, if not always, include some way to account for the endogeneity of the education outcomes. There are unobservable characteristics such as ability that correlated with schooling outcomes. Since they are unobservable, their omission in the model produces an upwards bias on the estimated returns to education. Thus, an instrument that is not correlated with one's ability but correlated with schooling is used to correct for this omitted bias.

Due to the lack of geographic and topographic data, the only viable instrument that could be used in this study is the peer schooling average. The peer schooling average is calculated by taking the mean educational attainment and years of schooling of a cohort of individuals that fall into a specific working age range within each commune and gender. Each individual’s own educational outcome is subtracted from that mean so that average schooling of their cohort is uncorrelated with their individual ability.

When assessing an instrument, both relevance and exogeneity must be discussed. The results section will demonstrate instrument relevance, since the peer schooling average is statistically significant and highly correlated with the actual educational outcome variable, whether it be expressed in terms of years of schooling or educational attainment. The validity of the instrument is impossible to prove, given the lack of other instruments that can be used to run an over identification test. Intuitively, a threat to the validity of this instrument is local economic potential. There are variations in educational outcomes across location due to each location’s ability to finance schooling and implement policy. Local farming conditions could also affect educational outcomes since children would be less likely to go to school if they are needed during the harvest season. Furthermore, there could be characteristics about the location that affect both one’s schooling and their family’s income.

Throughout this paper I use the peer schooling average to instrument for educational outcomes when looking at the income and probit models. To estimate the impact of the peer schooling average on household income per capita, I use the 2SLS (two-stage least squares) method for the 1989, 2000 and 2011 cross-sections of the data in the following model:

\[
Pr[NF_{ict} = 1|A_{ic}, X_{ic}] = \phi[\beta_0 + \sum_{s=1}^{S} \gamma_s A_{ict} + \alpha_c + \delta_t + X'_{ict}\beta + u_{ict}] \tag{5}
\]

where equation (6b) denotes the reduced form of (6a). \( y_{ic} \) is household income per capita and \( \beta_1 \) estimates the returns to the household head’s years of schooling using the same province fixed effects and controls as in equation (4). In the reduced form, \( \gamma_1 \) estimates the correlation between the household head’s peer schooling year average instrument, \( IV_{ic} \), and the actual years of schooling \( A_{ic} \).

The peer schooling average instrument is also used in the poverty and non-farm probability models, executed by Stata’s ivprobit program that uses maximum likelihood estimation to fit probit models when one or more of the regressors are endogenous. One of the main reasons the education variable is also expressed in terms of years is due to the fact that categorical variables cannot be accounted for when the outcome variable is dichotomous, as it is with the poverty
or nonfarm models. In order to compare the probit results with ivprobit instrument results, equations (1) and (5) are re-written using the household heads' years of schooling as the main independent variable instead of the categorical educational attainment. The impact of years of schooling on poverty is denoted by the $\beta_1$ in equations 7 and 8:

$$Pr[P_{ict} = 1 | A, Xs] = \phi[\beta_0 + \beta_1 A_{ict} + \alpha_c + \delta_t + X_{ict}'\beta + u_{ict}]$$ (7)

$$Pr[NF_{ict} = 1 | A, Xs] = \phi[\beta_0 + \beta_1 A_{ict} + \alpha_c + \delta_t + X_{ict}'\beta + u_{ict}]$$ (8)

In the following section, the limitations of the data set towards finding appropriate instruments will be discussed alongside other important data features.

3. Data Description

The China Health and Nutrition Survey (CHNS) is a longitudinal data base containing information on household and individual-level education, income, and labor activities for the years 1989, 1991, 1993, 1997, 2000, 2004, 2006, 2009 and 2011. The sampling process followed a multistage random cluster design. Counties were stratified by income and four counties were selected at random for each province. Villages and townships were also selected randomly. Similarly, neighborhoods were randomly selected from cities (Carolina Population Center). This paper only focuses on rural china because poverty is a more prominent problem in rural areas and most of the Chinese education reforms targeted such areas. The CHNS data is well-suited to rural analysis, as over seventy percent of the data come from households living in rural parts of China. Another important note to make on the data set as a whole is that it is not weighted. Thus, it is not representative of all of China.

3.1 Evolution of HOUSEHOLDS Surveyed per Wave

The CHNS website covered 15,917 individuals belonging to 3,795 households in 1989. Only individuals that belonged to the original sample households in 1989 were surveyed in 1991; thus, 14,788 individuals were surveyed. Some households must have no longer participated since the number of surveyed households in 1991 was only 3,616. In 1993, all new households formed from the original sample households were added to the sample. Similarly, in 1997, all new households formed were added to the sample and additional households were added to replace those no longer participating. From 2000 and onwards, newly formed households from the original sample were added, and other households were added as well to deal with attrition.

3.2 Education Variables of Interest

As discussed in Appendix A, there are two main variables in the data set that reflect schooling. The first is a variable that indicates the highest educational attainment level for each individual. The second variable denotes the years of schooling of each individual. The years of schooling variable has more variation; however, it is not a monotonic rendering of years of schooling. This was adjusted for as described by the variable composition in Appendix A. Thus, educational attainment is used when possible; however, for purposes of instrumenting, years of schooling is used as an alternate form of expressing educational outcomes. In 1989, no schooling was high with the majority of people stopping at primary school completion. In 2011, there are far less instances of no schooling and more individuals who pursued post primary education.

Figure 1 displays two kernel density plots that demonstrates the difference in distribution of household income per capita with no schooling and with the completion of lower middle school, otherwise known as middle school. In the year 1989, figure 1(a) shows that the completion of the nine years of mandatory schooling, as implemented by the 1986 policy, does in fact increase household incomes per capita. In 2011, there is a greater difference between household income per capita with no schooling versus middle school. This shows the increasing effect of education on income from the first survey wave to the last.
3.3 Data Limitations
This data set is an interesting one to use since it is longitudinal and has also health and nutrition information in addition to the standard household survey. However, its main drawback is the anonymity of the commune and location of households. Had that information been available, topographical data could have been matched to it to create better instruments that gauged the economic potential of the area the household was located in. Location information would have also been useful in creating spatial deflators that accounted for changes in prices over time. My models try to account for this with the use of time fixed effects, but a spatial deflator would have been a better tool to use.

On the other hand, the data does have some very interesting health and nutrition information that can be used for future work or extensions. This will be elaborated on further in the final section of the paper.

4. Results
The following section will display and elaborate on the results derived after executing the empirical strategy described in section three.

4.1 Education & Poverty Reduction
Using the probit random-effects regression model described by equation (1), in Appendix I, Table I1 is a regression output that shows the statistical significance of the education coefficients for the household head. Probit results are difficult to interpret and thus the marginal probability effects are calculated to produce Figure 2.

Figure 2 depicts the margins plot using the model with the household head's characteristics. There seem to be three main strands of educational attainment, with the first being no schooling, some primary school and primary school completion, the second being lower and upper middle school, and the third technical school and university. In the first strand, it is good to see that the least amount of schooling has the highest probability of being poor across all ages. It is interesting to note that as the educational levels increase, there seems to be less of an age effect, indicating that more education leaves one less vulnerable to poverty as they get older.

Since poverty is based in household income per capita, regression (2) was used to capitalize on the longitudinal nature of the data. Using a fixed effects regression omits the features in the unit of observation that do not vary with time, such as the household head's age (this changes over time but is perfectly correlated with change in year), gender, or ability. Thus, a fixed effects panel regression is one way to tackle the endogeneity issue when estimating the returns to education.
A Hausman test comparing coefficients rendered after running a fixed effects and random effects model of equation (2) concluded that I should be using fixed effects. There is a clear lack of statistical significance on coefficients in column one of table 2. On the other hand, column 2 shows the rendering of statistically significant coefficients using random-effects. For the random-effects model, completion of lower middle school by the household head, equivalent to 9 years of compulsory schooling, increases household income by approximately 30%; for fixed-effects, household income increases by only 7.3%. Conceptually, educational attainment should not vary much through time for working age household heads. Thus, it does not make sense to use fixed effects, further supporting the case to use an instrument to address endogeneity instead.

To more closely examine the heterogeneity in the impact of education on household income, the results produced from running the quantile regression model (3) are displayed in table 3 below. The results suggest that across all waves, educational attainment has a greater impact on those at the bottom end of the household income distribution. For example, completing the compulsory 9 years of schooling would increase a household’s income by roughly 49% if they were in the lowest quintile, and roughly 30% if they were in the highest quintile. The higher responses to education at the bottom quantiles demonstrates the viability of education as a poverty alleviator. It could also be the case that education has less of an effect on higher income households because their incomes are already high. Thus, a smaller return to education would be increasing an already larger income than that of a household in the lower portion of the distribution. Despite this, although incomes of poorer households are lower, which may be why the returns to education coefficients are high, education still plays a bigger role in increasing the economic wellbeing of a poorer household than it does for a more affluent household.

Figure 3 displays margins plots derived from the quantile regression of household income per capita on the years of household head’s schooling. At each educational attainment level, the lowest quantile, the 20\textsuperscript{th}, has the highest predicted coefficients on education, thereby affirming the greater impact education has on poorer households.
The next model (4) looks at this heterogeneity in educational returns using data from the years 1989, 2000 and 2011. For this model, the educational variable is expressed in terms of years of schooling as opposed to educational attainment. A simple OLS regression is run first, followed by a quantile regression, and the coefficients are then compared both graphically in figure 6 and numerically in Appendix J, tables J1, J2 and J3.

**Figure 4.** OLS (Black Dash Line) Vs Quantile (Red Solid Line) Returns to Years of Schooling

As demonstrated by the comparison of OLS and quantile coefficients, the impact that education has changes depending on the positioning of the household on the income distribution. This change is not captured by the OLS coefficients, illustrated by the horizontal black dashed line in each sub graph of figure 4. It is interesting to note that in 1989, overall returns to education across all income quantiles were much lower than in the years 2000 and 2011. This paper hypothesizes that nonfarm labor is the mechanism through which returns to education increase. One explanation the notable change across time in figure 4 is China’s expansion of its nonfarm labor sector. Around the years 1986 - 1995, China underwent factor market liberalization that relaxed controls on factor inputs, allowing people to reallocate their productive inputs from farm to nonfarm labor. Thus, through the more feasible access to nonfarm labor over time, returns to education increase from 1989 to 2011.

The second half of my preliminary results test the effect of education on the probability of the household head having a nonfarm primary occupation. This is derived by running the probit described in equation (5), which uses the educational attainment categorical variable. Probit output coefficients are displayed in Table I2 in Appendix I.

Figure 5 was derived after plotting the margins calculated over educational attainment and age of household head from the probit results. It is interesting to see that with no schooling, or some primary and primary completion, after age 40, the household head has a much better chance at getting into the nonfarm sector as compared to when they are younger. With increasing higher levels of education there is less of an age gradient and household heads are far more likely to engage in nonfarm labor, even at earlier ages. The difference between post high school education (upper middle school as it is referred to in the data set) and formal schooling is quite significant. Figure 5 displays a large gap between technical or university degrees and the formal schooling system. This indicates that higher education (post-high school) assures nonfarm labor. It would also be useful to witness these results for different separate years. Thus, figure 6 compares the nonfarm probit margins for the years 1989 and 2011.

**Figure 5.** Marginal Probability Effects of Nonfarm Labor & Educational Attainment Model
Comparing the differences between figures 5 and 6 reinforces the importance of looking at what is happening in individual years. The immediate difference between figure 5, which uses all waves in its estimation, and figure 6, which looks at the years 1989 and 2011 separately, is the age gradient. In both 1989 and 2011, the probability of nonfarm labor decreases with age. In 1989, this can be explained by the household head retiring on the farm. In 2011, it can be explained by the fact that an educational degree matters in one's younger years since that is when they are most likely to determine their career and make the farm or nonfarm choice. Figure 6(a) demonstrates that with no schooling, individuals still had a slightly higher ability to engage in nonfarm activity than those who completed primary school. Between the years 1989 and 2011, figure 6(b) shows that there is a greater return to lower middle school and upper middle school in 2011 than there was in 1989. Overall, this implies growing returns to education over the years, which can be ascribed to the stronger presence of a non-farm labor sector.

4.2 Instrumenting using Peer Schooling Average
Using the peer schooling age average discussed in section D of the Empirical Specification, the instrument is used to compare the OLS and IV coefficients in model 7(a). Education is expressed as years of schooling and the model is run for the years 1989, 2000, 2011 separately. Additionally, a Hausman test is used to determine whether or not the IV and OLS coefficients are statistically significantly different, in which case the use of an IV is preferred.

For the years 1989 and 2000, the Hausman test p-values were 0 and 0.0001 respectively. Thus, the null hypothesis of the IV and OLS estimates not being statistically significantly different is rejected and the IV estimates are deemed viable to use. The Hausman test for the year 2011 yielded a p-value of 0.6459. In this case, we fail to reject the null and it is better to use OLS. This is surprising since 2011 is the most recent year with perhaps the least measurement error in comparison to 1989. Tables 6 and Table 7 show very large difference between the coefficients. In 1989, the estimated increase to logged household income per capita with an additional year of schooling is 16.8% using the IV and only 3.97% using OLS. According to these results, the original OLS equation underestimated the returns to education. However, the rather large difference between the two estimates does invite some skepticism and it is possible that there is measurement error.

The instrument was also used in the poverty and non-farm probits as specified by equations (8) and (9) respectively. The probit coefficients are displayed in tables I3 and I5 of Appendix I. The ivprobit outputs are shown in tables I4 and I6.
By comparing figures 6, 7, and 8, it is evident that the peer schooling instrument produces similar results to the actual education variable, thereby suggesting instrument relevance. Figures 6 and 7, with and without the instrument, demonstrate a decrease in poverty. Similarly, Figure 8, with and without the instrument, shows that the probability of nonfarm labor increases with additional years of schooling.

5. Conclusion and Future Work
In summary, this paper has provided useful results that agree with the main hypotheses made in section two. The signs on the coefficients of the education variables in each of the models confirm that education increases the probability of nonfarm labor, increases income, and decreases the likelihood of being poor. Furthermore, this paper moved away from the commonly used estimation of educational impact at the means and used quantile regressions to reveal the heterogeneous nature of educational returns. By using both versions of the education variables, years of schooling and educational attainment, it was possible to examine the returns to education in terms of one year unit increment increases, as well as in terms of qualification and completion. Although the distinction between the two is small since they both produce results that move in the same direction, using both is a way to check robustness and examine the different ways to measure education. Another important phenomenon this paper brings to light is the mechanism of nonfarm labor and the role it plays towards actualizing the returns to education in the labor market, thereby increasing
incomes and economic welfare.

Despite data limitations, this paper demonstrated the relevance of peer schooling average as an instrument for one's own education. Certain concerns pertaining to the validity of the instrument were discussed with the hopes of being able to link geographic and topographical information to this data set in the future. This would allow for further instrumenting that would in turn make it possible to run an over-identification test. Geographic and topographical instruments such as the household’s proximity to a port or trading route would also control for the local economic potential that would otherwise threaten the validity of my instrument.

Throughout this research project, I came to realize that the CHNS data set is perhaps not the most suitable to use for the specific aims of this thesis. Data with better indicators of economic potential and more options for instrumentation would have been easier to use. However, this data set does have certain features, that, if exploited, could pose the grounds for an interesting future extension to this thesis. In most cases, poverty is estimated at the household level because the determinants of poverty status are often calculated using household measures. Estimating poverty at the individual level eliminates the need to control for household characteristics that would otherwise be needed when determining poverty at the household level. The unit of the household itself varies a lot with time, making it difficult to keep track of household members’ entries and exits. Thus, individual measures are useful in the sense that their characteristics are more homogenous and more easily trackable over time. One way to measure individual level poverty is to look at food poverty – assessing whether an individual is meeting their daily caloric requirement (Ravallion, 2016). Critics of this method would argue that, in order to be considered not poor, individuals need to satisfy more than just their caloric requirement. That is why, typically, poverty lines are estimated using a basket of food and non-food goods. However, caloric intake can be used as a starting point to examining food poverty and developing individual level poverty measures. The CHNS data base has extensive longitudinal data on nutrition and health. Thus, it would be a very suitable data set to carry out this potential future work suggestion.
References


### Table I1. Probit Estimation: HH Poverty & HH Head's Educational Attainment

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>Probability of HH Poverty</th>
</tr>
</thead>
<tbody>
<tr>
<td>Edu Level = 1, Some Primary</td>
<td>-0.1255**</td>
</tr>
<tr>
<td>(0.0519)</td>
<td></td>
</tr>
<tr>
<td>Edu Level = 2, Grad Primary</td>
<td>-0.2555***</td>
</tr>
<tr>
<td>(0.0535)</td>
<td></td>
</tr>
<tr>
<td>Edu Level = 3, Lower Mid. School</td>
<td>-0.4014***</td>
</tr>
<tr>
<td>(0.0597)</td>
<td></td>
</tr>
<tr>
<td>Edu Level = 4, Upper Mid School</td>
<td>-0.4646***</td>
</tr>
<tr>
<td>(0.0766)</td>
<td></td>
</tr>
<tr>
<td>Edu Level = 5, Technical School</td>
<td>-0.9207***</td>
</tr>
<tr>
<td>(0.1659)</td>
<td></td>
</tr>
<tr>
<td>Edu Level = 6, Uni or College</td>
<td>-1.6215***</td>
</tr>
<tr>
<td>(0.3879)</td>
<td></td>
</tr>
<tr>
<td>Edu Level = 7, Masters or Higher</td>
<td>-</td>
</tr>
<tr>
<td>Age, Years (HHH)</td>
<td>-0.0587***</td>
</tr>
<tr>
<td>(0.0071)</td>
<td></td>
</tr>
<tr>
<td>Age Squared, (HHH)</td>
<td>0.0006***</td>
</tr>
<tr>
<td>(0.0001)</td>
<td></td>
</tr>
<tr>
<td>Gender, Female = 1 (HHH)</td>
<td>-0.0394</td>
</tr>
<tr>
<td>(0.0507)</td>
<td></td>
</tr>
<tr>
<td>HH Size</td>
<td>0.0460***</td>
</tr>
<tr>
<td>(0.0111)</td>
<td></td>
</tr>
<tr>
<td>Stratum (Rural Village =1)</td>
<td>0.3547***</td>
</tr>
<tr>
<td>(0.0881)</td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>0.0678</td>
</tr>
<tr>
<td>(0.2462)</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>21,893</td>
</tr>
</tbody>
</table>

*Robust standard errors in parentheses

** p<0.01, *** p<0.05, * p<0.1

---

1 White test was performed to confirm heteroskedasticity. Standard errors are clustered using community ids.
Table I2. Probit Estimation: HH Non-Farm Labor & HH Head’s Educational Attainment

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>Probability of Non-Farm Labor</th>
</tr>
</thead>
<tbody>
<tr>
<td>Edu Level = 1, Some Primary</td>
<td>0.2749**</td>
</tr>
<tr>
<td></td>
<td>(0.1095)</td>
</tr>
<tr>
<td>Edu Level = 2, Grad Primary</td>
<td>0.4392***</td>
</tr>
<tr>
<td></td>
<td>(0.1056)</td>
</tr>
<tr>
<td>Edu Level = 3, Lower Mid. School</td>
<td>0.7429***</td>
</tr>
<tr>
<td></td>
<td>(0.1243)</td>
</tr>
<tr>
<td>Edu Level = 4, Upper Mid School</td>
<td>1.0925***</td>
</tr>
<tr>
<td></td>
<td>(0.1415)</td>
</tr>
<tr>
<td>Edu Level = 5, Technical School</td>
<td>2.2483***</td>
</tr>
<tr>
<td></td>
<td>(0.2915)</td>
</tr>
<tr>
<td>Edu Level = 6, Uni or College</td>
<td>1.6580***</td>
</tr>
<tr>
<td></td>
<td>(0.3055)</td>
</tr>
<tr>
<td>Edu Level = 7, Masters or Higher</td>
<td>-</td>
</tr>
<tr>
<td>Age, Years (HHH)</td>
<td>0.0604***</td>
</tr>
<tr>
<td></td>
<td>(0.0140)</td>
</tr>
<tr>
<td>Age Squared, (HHH)</td>
<td>-0.0007***</td>
</tr>
<tr>
<td></td>
<td>(0.0001)</td>
</tr>
<tr>
<td>Gender, Female = 1 (HHH)</td>
<td>0.0062</td>
</tr>
<tr>
<td></td>
<td>(0.1099)</td>
</tr>
<tr>
<td>HH Size</td>
<td>-0.0772***</td>
</tr>
<tr>
<td></td>
<td>(0.0167)</td>
</tr>
<tr>
<td>HH Income (Yuan Nominal)</td>
<td>0.4038***</td>
</tr>
<tr>
<td></td>
<td>(0.0308)</td>
</tr>
<tr>
<td>Type of HH Business: Collective Farm = 1</td>
<td>-1.1720***</td>
</tr>
<tr>
<td></td>
<td>(0.4067)</td>
</tr>
<tr>
<td>Type of HH Business: Household Farm = 1</td>
<td>-1.5057***</td>
</tr>
<tr>
<td></td>
<td>(0.0812)</td>
</tr>
<tr>
<td>Type of HH Business: Both Collective &amp; Household = 1</td>
<td>-1.4305***</td>
</tr>
<tr>
<td></td>
<td>(0.3188)</td>
</tr>
<tr>
<td>Stratum (Rural Village =1)</td>
<td>-2.0712***</td>
</tr>
<tr>
<td></td>
<td>(0.2427)</td>
</tr>
<tr>
<td>Constant</td>
<td>-2.1508***</td>
</tr>
<tr>
<td></td>
<td>(0.5213)</td>
</tr>
<tr>
<td>Observations</td>
<td>18,048</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Table I3. Probit Estimation of Poverty using Household Head’s Years of Education

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>Probability of HH Poverty</th>
</tr>
</thead>
<tbody>
<tr>
<td>Years of Education (HHH)</td>
<td>-0.0418***</td>
</tr>
<tr>
<td>Age, Years (HHH)</td>
<td>-0.0335*</td>
</tr>
<tr>
<td>Age Squared, (HHH)</td>
<td>0.0003*</td>
</tr>
<tr>
<td>Gender, Female = 1 (HHH)</td>
<td>-0.0209</td>
</tr>
<tr>
<td>HH Size</td>
<td>0.0459*</td>
</tr>
<tr>
<td>Stratum (Rural Village =1)</td>
<td>0.2571**</td>
</tr>
<tr>
<td>Constant</td>
<td>-0.5086</td>
</tr>
<tr>
<td>Observations</td>
<td>2,329</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1

Table I4. IV Probit Estimation of Poverty using Household Head’s Years of Education

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(IV) Probability of Poor</th>
<th>Reduced Form</th>
</tr>
</thead>
<tbody>
<tr>
<td>Years of Education (HHH)</td>
<td>-0.2055***</td>
<td></td>
</tr>
<tr>
<td>Age, Years (HHH)</td>
<td>-0.0405***</td>
<td>-0.1441***</td>
</tr>
<tr>
<td>Age Squared, (HHH)</td>
<td>0.0002</td>
<td>0.0007*</td>
</tr>
<tr>
<td>Gender, Female = 1 (HHH)</td>
<td>-0.4374***</td>
<td>-1.8753***</td>
</tr>
<tr>
<td>HH Size</td>
<td>0.0307</td>
<td>-0.0008</td>
</tr>
<tr>
<td>Stratum (Rural Village =1)</td>
<td>-0.1955</td>
<td>-1.7931***</td>
</tr>
<tr>
<td>IV: Cohort Years of Education (HHH)</td>
<td>0.4886***</td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>1.8733***</td>
<td>9.9678***</td>
</tr>
<tr>
<td>Observations</td>
<td>2,329</td>
<td>2,329</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1
Table 15. Probit Estimation of Non-Farm Labor using Household Head’s Years of Education

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>Probability of Non-Farm Labor</th>
</tr>
</thead>
<tbody>
<tr>
<td>Years of Education (HHH)</td>
<td>0.1030***</td>
</tr>
<tr>
<td>Age, Years (HHH)</td>
<td>0.0598***</td>
</tr>
<tr>
<td>Age Squared, (HHH)</td>
<td>-0.0007***</td>
</tr>
<tr>
<td>Gender, Female = 1 (HHH)</td>
<td>0.0459</td>
</tr>
<tr>
<td>HH Size</td>
<td>-0.0781***</td>
</tr>
<tr>
<td>Type of HH Business: Collective Farm = 1</td>
<td>-1.1705***</td>
</tr>
<tr>
<td>Type of HH Business: Household Farm = 1</td>
<td>-1.5114***</td>
</tr>
<tr>
<td>Type of HH Business: Both Collective &amp; Household = 1</td>
<td>-1.4452***</td>
</tr>
<tr>
<td>Stratum (Rural Village =1)</td>
<td>-2.0801***</td>
</tr>
<tr>
<td>Log of Household Income</td>
<td>0.4049***</td>
</tr>
<tr>
<td>Constant</td>
<td>-2.2765***</td>
</tr>
<tr>
<td>Observations</td>
<td>18,053</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1

Table 16. IV Probit Estimation of Non-Farm Labor using Household Head's Years of Education

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(IV) Probability of Non-Farm Labor</th>
<th>Reduced Form</th>
</tr>
</thead>
<tbody>
<tr>
<td>Years of Education (HHH)</td>
<td>0.1938***</td>
<td></td>
</tr>
<tr>
<td>Age, Years (HHH)</td>
<td>0.0439***</td>
<td>-0.1557***</td>
</tr>
<tr>
<td>Age Squared, (HHH)</td>
<td>-0.0004***</td>
<td>0.0008***</td>
</tr>
<tr>
<td>Gender, Female = 1 (HHH)</td>
<td>0.3401***</td>
<td>-1.4627***</td>
</tr>
<tr>
<td>HH Size</td>
<td>-0.0668***</td>
<td>-0.0824***</td>
</tr>
<tr>
<td>Log of Net HH income</td>
<td>0.2753***</td>
<td>0.4341***</td>
</tr>
</tbody>
</table>
### Table J1. 1989 Quantile Regression: Log of HH Income per capita & Years of HH Head’s Schooling

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>20th Quantile Log of HH Income per capita</th>
<th>40th Quantile Log of HH Income per capita</th>
<th>60th Quantile Log of HH Income per capita</th>
<th>80th Quantile Log of HH Income per capita</th>
</tr>
</thead>
<tbody>
<tr>
<td>Years of Education (HHH)</td>
<td>0.0476***</td>
<td>0.0396***</td>
<td>0.0320***</td>
<td>0.0242***</td>
</tr>
<tr>
<td>Age, Years (HHH)</td>
<td>0.0779***</td>
<td>0.0781***</td>
<td>0.0576***</td>
<td>0.0479***</td>
</tr>
<tr>
<td>Age Squared, (HHH)</td>
<td>-0.0008***</td>
<td>-0.0008***</td>
<td>-0.0006***</td>
<td>-0.0005***</td>
</tr>
<tr>
<td>Gender, Female = 1</td>
<td>0.2082***</td>
<td>0.2061***</td>
<td>0.1245***</td>
<td>0.0042</td>
</tr>
<tr>
<td>HH Size</td>
<td>0.1202***</td>
<td>0.1352***</td>
<td>0.1459***</td>
<td>0.1402***</td>
</tr>
<tr>
<td>Stratum (Rural Village =1)</td>
<td>-0.6819***</td>
<td>-0.5287***</td>
<td>-0.3514***</td>
<td>-0.2630***</td>
</tr>
<tr>
<td>Constant</td>
<td>4.7474***</td>
<td>5.4096***</td>
<td>6.1659***</td>
<td>6.7286***</td>
</tr>
<tr>
<td>Observations</td>
<td>2,378</td>
<td>2,378</td>
<td>2,378</td>
<td>2,378</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1
## Table J2. 2000 Quantile Regression: Log of HH Income per capita & Years of HH Head’s Schooling

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>20th Quantile Log of HH Income per capita</th>
<th>40th Quantile Log of HH Income per capita</th>
<th>60th Quantile Log of HH Income per capita</th>
<th>80th Quantile Log of HH Income per capita</th>
</tr>
</thead>
<tbody>
<tr>
<td>Years of Education (HHH)</td>
<td>0.0705***</td>
<td>0.0508***</td>
<td>0.0378***</td>
<td>0.0240***</td>
</tr>
<tr>
<td></td>
<td>(0.0072)</td>
<td>(0.0056)</td>
<td>(0.0054)</td>
<td>(0.0056)</td>
</tr>
<tr>
<td>Age, Years (HHH)</td>
<td>0.0711***</td>
<td>0.0711***</td>
<td>0.0605***</td>
<td>0.0666***</td>
</tr>
<tr>
<td></td>
<td>(0.0143)</td>
<td>(0.0097)</td>
<td>(0.0104)</td>
<td>(0.0102)</td>
</tr>
<tr>
<td>Age Squared, (HHH)</td>
<td>-0.0007***</td>
<td>-0.0007***</td>
<td>-0.0006***</td>
<td>-0.0006***</td>
</tr>
<tr>
<td></td>
<td>(0.0001)</td>
<td>(0.0001)</td>
<td>(0.0001)</td>
<td>(0.0001)</td>
</tr>
<tr>
<td>Gender, Female = 1</td>
<td>0.2290***</td>
<td>0.1523***</td>
<td>0.1378***</td>
<td>0.0793</td>
</tr>
<tr>
<td></td>
<td>(0.0822)</td>
<td>(0.0566)</td>
<td>(0.0530)</td>
<td>(0.0604)</td>
</tr>
<tr>
<td>HH Size</td>
<td>-0.0345**</td>
<td>-0.0400***</td>
<td>-0.0480***</td>
<td>-0.0659***</td>
</tr>
<tr>
<td></td>
<td>(0.0149)</td>
<td>(0.0147)</td>
<td>(0.0139)</td>
<td>(0.0142)</td>
</tr>
<tr>
<td>Stratum (Rural Village =1)</td>
<td>-0.4728***</td>
<td>-0.5412***</td>
<td>-0.4607***</td>
<td>-0.4154***</td>
</tr>
<tr>
<td></td>
<td>(0.0633)</td>
<td>(0.0447)</td>
<td>(0.0372)</td>
<td>(0.0451)</td>
</tr>
<tr>
<td>Constant</td>
<td>5.7653***</td>
<td>6.3857***</td>
<td>6.9938***</td>
<td>7.3031***</td>
</tr>
<tr>
<td></td>
<td>(0.3928)</td>
<td>(0.2472)</td>
<td>(0.2552)</td>
<td>(0.2572)</td>
</tr>
<tr>
<td>Observations</td>
<td>2,542</td>
<td>2,542</td>
<td>2,542</td>
<td>2,542</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

## Table J3. 2011 Quantile Regression: Log of HH Income per capita & Years of HH Head’s Schooling

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>20th Quantile Log of HH Income per capita</th>
<th>40th Quantile Log of HH Income per capita</th>
<th>60th Quantile Log of HH Income per capita</th>
<th>80th Quantile Log of HH Income per capita</th>
</tr>
</thead>
<tbody>
<tr>
<td>Years of Education (HHH)</td>
<td>0.0697***</td>
<td>0.0561***</td>
<td>0.0421***</td>
<td>0.0319***</td>
</tr>
<tr>
<td></td>
<td>(0.0082)</td>
<td>(0.0061)</td>
<td>(0.0052)</td>
<td>(0.0053)</td>
</tr>
<tr>
<td>Age, Years (HHH)</td>
<td>0.0188</td>
<td>0.0209</td>
<td>0.0206*</td>
<td>0.0109</td>
</tr>
<tr>
<td></td>
<td>(0.0167)</td>
<td>(0.0135)</td>
<td>(0.0113)</td>
<td>(0.0144)</td>
</tr>
<tr>
<td>Age Squared, (HHH)</td>
<td>-0.0002</td>
<td>-0.0002*</td>
<td>-0.0002**</td>
<td>-0.0001</td>
</tr>
<tr>
<td></td>
<td>(0.0002)</td>
<td>(0.0001)</td>
<td>(0.0001)</td>
<td>(0.0001)</td>
</tr>
<tr>
<td>Gender, Female = 1</td>
<td>-0.1780</td>
<td>-0.0987</td>
<td>-0.0671</td>
<td>-0.0093</td>
</tr>
<tr>
<td></td>
<td>(0.1117)</td>
<td>(0.0829)</td>
<td>(0.0647)</td>
<td>(0.0465)</td>
</tr>
<tr>
<td>HH Size</td>
<td>-0.1358***</td>
<td>-0.1291***</td>
<td>-0.1271***</td>
<td>-0.1214***</td>
</tr>
<tr>
<td></td>
<td>(0.0189)</td>
<td>(0.0154)</td>
<td>(0.0128)</td>
<td>(0.0127)</td>
</tr>
<tr>
<td>Stratum (Rural Village =1)</td>
<td>-0.2680***</td>
<td>-0.2604***</td>
<td>-0.2459***</td>
<td>-0.1993***</td>
</tr>
<tr>
<td></td>
<td>(0.0605)</td>
<td>(0.0500)</td>
<td>(0.0410)</td>
<td>(0.0416)</td>
</tr>
<tr>
<td>Constant</td>
<td>8.5344***</td>
<td>8.9410***</td>
<td>9.3589***</td>
<td>9.9758***</td>
</tr>
<tr>
<td></td>
<td>(0.4485)</td>
<td>(0.3784)</td>
<td>(0.3194)</td>
<td>(0.4244)</td>
</tr>
<tr>
<td>Observations</td>
<td>2,533</td>
<td>2,533</td>
<td>2,533</td>
<td>2,533</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
The Economic Impact of Psychological Distress on Former Child Soldiers

JONATHAN KAUFMANN
American University

ABSTRACT
While previous research demonstrates a significant negative relationship between post-traumatic stress disorder and earnings among adult veterans in the United States, a similar connection for children in developing nations has not been established. The literature indicates that both endogeneity and sample-selection biases are inherent in this relationship. This paper used ordinary least squares, two-stage least squares, Heckman selection, and instrumental variable Heckman selection models to progressively control for these biases, and is the first statistical analysis to explore the impact of psychological distress on the income and employment status of former child soldiers. Violence witnessed and feelings of helplessness during abduction were used as instruments for measuring distress. The results indicate that distress significantly diminishes income but has no significant effect on employment status. This study helps to bridge the gap between psychological and economic research on former child soldiers by demonstrating that interventions focused on mental health that reduce psychological distress can positively impact income as well.
Jewish Network Effects in Israeli Trade

JONAH KELLY

Dartmouth College

ABSTRACT

The Jewish people have long been associated with international trade. Because Jewish demography changed dramatically in the last century, the question remains as to what role Jewish networks have played in contemporary trade. Not only did the 20th Century see major migrations of Jews, but the Jewish People gained sovereignty for the first time in modern history with the establishment of the State of Israel. Stemming from a body of literature focused on the importance of ethnic networks in trade, this paper asks whether global Jewish ethnic networks impact Israel’s trade patterns. I find that a 1% increase in Jewish population in a diaspora country leads to a 0.17% increase in the value of imports from Israel, significant at the 5% level. Furthermore, Jewish populations have a significant impact on the importing of goods associated with Jewish culture, pointing towards “tastes” as one mechanism through which these Jewish networks operate.

1 Many thanks to Professor Nina Pavcnik for all of her help with this paper, and for helping foster my appreciation for economics. Thank you also to my friends and family for listening to me talk about this paper endlessly. Thank you to the Carroll Round Steering Committee for putting together a great program. And finally, thank you to Alex, Connor, and the rest of the Carroll Round crew for such a fun weekend. As they say, “It’s the Carroll Round, folks!”
1. Introduction

Most examinations of the determinants of bilateral trade account for some factors that are cultural in nature. Economists have asked not only whether two countries are far away from one another, but whether they speak the same language. They have wondered not only how wealthy two countries are, but whether they were ever engaged in a colonial relationship. Interestingly, in recent decades, researchers have looked more specifically at the effect of culture on trade by researching the role of networks. They have recognized the importance of business and social relationships – networks that are often tied to ethnicity – in establishing patterns of trade.

Of those ethnicities (or ethno-religions) that are strongly associated with world trade, Jews have long loomed large. From the second to seventh centuries, Judaism as a religion began requiring its males to read the prayers and learn the Torah. This made the Jewish people “a literate ethnic group in a world where the rest of the population was illiterate,” and fostered their transition from the agricultural economy to the merchant economy (Botticini and Eckstein, 2007). By the eleventh century, Jews had established their own unique patterns of trade (Greif, 1993). And by the sixteenth century, Shakespeare had created his most famous Jewish character, the “Merchant of Venice.”

Whereas for these years the Jews were a diaspora people, having been exiled by the Romans in the first century, much has changed in the last seven decades. On May 14th, 1948, the State of Israel was established. The first Jewish sovereign state in modern times, Israel quickly absorbed nearly all the Jews of the Middle East and North Africa, as well as many of those European Jews fortunate enough to have survived the Holocaust. Among those Jews that remained in the diaspora, the new geographic spread was very different from the historical one. There was no longer a Jewish Baghdad, but there was a Jewish Miami. There was no longer a Jewish Warsaw, but there was a Jewish Buenos Aires.

In this paper, I investigate whether the shared ethno-religious background of this new Jewish diaspora still plays a role in the global economy, particularly as it relates to the relatively young Jewish state. Do countries with Jewish populations trade more with Israel, all other factors considered equal? If the answer is yes, by what mechanisms does this Jewish network increase trade? I find that Jewish networks do still play a significant role, particularly when it comes to cultural goods – a result that has significance both in the realm of Israeli trade policy and in the realm of ethno-religious networks generally.

2. Previous Literature

As addressed in a literature base that consists of more than one hundred papers, the inhibitory effect of distance on trade is one of the most widely accepted results in international economics. This relationship can be captured in the gravity model, which suggests that bilateral trade between two countries is proportional to their economic mass and inversely proportional to bilateral trade costs. The question, remains, though: how do we measure trade costs? Geographic distance is the most commonly used proxy, likely because it gets at the root of transportation costs. Still, transportation is but one trade cost. Others include policy barriers, information costs, contract enforcement costs, and local distribution costs (Anderson and van Wincoop, 2004). These costs are more difficult to capture, but are easily influenced by the cultural similarities or differences between trading countries. Cultural biases affecting trade may be based on histories of conflict or on religious, genetic, or somatic similarities (Guiso, 2009). Perhaps unsurprisingly, the preference for trade with those of similar background translates into a significant, positive effect of immigration on trade (Gould, 1994, Herander and Saavedra, 2002, Head and Ries, 2001, and Peri and Requena, 2009).

Immigration may promote trade through two main mechanisms: the reduction of information costs and the influence of preferences. The first channel relies on economic agents that break down information barriers between domestic and foreign markets, providing information both on what is produced in another country and on what the tastes in the host country are. The second channel assumes that when migrants arrive, they bring with them their specific tastes, and even have the potential to alter the consumption patterns of nationals (Combes et al, 2005). In this way, “tastes” translate into trade. On top of these two main mechanisms, a third mechanism may exist: the pro-trade effect of ethnic networks.

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2 Research has pointed to the existence of a “home bias,” for example, showing that consumers prefer goods produced in their home country or state (McCallam (1995), Helliwell (1998)).
(Dunlevy and Hutchinson, 1999). This mechanism is effectively just an applied version of the first two, but forms an interesting case study, a case study that is underrepresented in the existing literature.

There also exists a strong empirical body of research on the impact of ethnic networks. This research is best exemplified by the work of Rauch and Trindade (2002), who investigate the impact of ethnic Chinese networks on bilateral trade. The authors find that, on average, these networks increased bilateral trade in differentiated products by sixty percent. More recent research has questioned whether these estimates were too low (because of heterogeneity) or too high (because of the omission of multilateral resistance terms) (Bandyopadhyay et al, 2008, Felbermayr et al, 2010).

No empirical research has been done, however, on the modern equivalent on today’s Jewish diaspora. Whereas there has long been academic interest in trade within the Jewish diaspora, there has been very little research done on the topic in the contemporary period. There exists no evaluation of Jewish trade since the 1948 establishment of the State of Israel, and certainly no empirical investigation.

Since its founding, Israel’s trade patterns have been affected positively by the signing of numerous free trade agreements (US, EEC, Turkey, Mexico, Canada, Jordan, Egypt, and the Mercosur trade bloc), and negatively by the Arab Boycott, which reached its peak in the 1970s (Abu-Bader, 2002). However, empirical research on Israeli trade patterns has overall been limited. Economists have looked at the impact of the massive surge of Soviet immigration to Israel in the early 1990s, but have not discussed the migration’s impact on trade (Gandal et al, 2002). More relevantly, research has shown that the 1990s peace process, particularly the signing of the Declaration of Principles in 1993, did have a positive effect on Israeli trade (Abu-Bader, 2002). Economists have also weighed the effects of various macroeconomic variables of trading partners on Israel’s exports to that country, and have concluded that Israel’s FTAs had a considerable positive effect on exports, and that a higher level of corruption in a destination country was associated with a lower level of exports (Lavee et al, 2012).

I ask whether there are characteristics of a given country outside of these macroeconomic determinants that affect Israeli trade. More specifically, I question whether the previously revealed relationship between ethnic networks and trade can be applied to the case of world Jewry and Israel.

As the first empirical analysis of Jewish trade in the modern era, this paper may guide Israel’s policies regarding the formation of general trading companies abroad. Japan, for example, relies on international companies called sogo shosha to disseminate information about its markets and act as intermediaries in trade. Should Israel be relying on Jewish populations globally to expand foreign markets for its products? If so, are there certain products that Israel should be focusing on? On a broader level, given that this is the first paper to address the impact of ethno-religious networks, it may open the door for further such study on, say, Sikh or Druze networks.

3. Data
To answer my core question, I examine data on the country-pair level for three years: 1995, 2005, and 2015. For each of these years, I merge three datasets: one that contains my dependent variable, one my independent variable, and one my control variable.

The dependent variable, lnvalue, is the log of the value of aggregate imports from Israel to the destination country. This measure comes from the United Nations International Trade Statistics Database ("UN Comtrade") and includes imported goods only. All trade data from UN Comtrade is reported in current US dollar values, calculated using an average annual exchange rate, which is in turn calculated by weighting the monthly exchange rate with the monthly volume of trade.3

The main independent variable is “CoreJewPop,” a measure of a given country’s Jewish population in any year. This data comes from Sergio DellaPergola’s annual report entitled “World Jewish Population,” which is published in The American Jewish Year Book and is based on data collected through various sources including government and private surveys.

DellaPergola defines “core Jewish population” as “all persons who, when asked, identify themselves as Jews, or, if the respondent is a different person in the same household, are identified by him/her as Jews; and do not have another religion” as well as “persons with a Jewish parent who claim no current religious or ethnic identity.” I believe this calculation provides me with an appropriate measure of Jewish population for my study. This is because even though the bounds of ethno-religious groups are blurred, it is those Jews that are self-identifying and/or children of Jews that are self-identifying that I hypothesize have the highest potential to impact trade with Israel. Because this dataset is comprehensive, I code any countries missing data on Jewish population as zeroes.

The gravity controls utilized are from the Centre d’Études Prospectives et d’Informations Internationales (“CEPII”). These serve the following purposes: \( \ln(GDP_d) \) captures the log of the GDP of the destination country in current US dollars, while \( \ln(dist_w) \) captures the log of the weighted distance between the destination country and Israel, captured in kilometers between the biggest cities of the two countries, weighted by the share of the city in the overall country’s population. \( Contig \) is a dummy variable indicating whether the destination country shares a border with Israel (Egypt and Jordan). \( Comlang_{\text{ethno}} \) is a dummy variable equaling one if a language is spoken by at least 9% of both countries’ populations, and \( colony \) is a dummy variable indicating whether the pair were ever in a colonial relationship (United Kingdom). Finally, \( gatt_d \) is a dummy variable equaling one if the destination country is a WTO member, and \( fta_{\text{wto}} \) is a dummy variable equaling one if the destination country has signed a trade agreement with Israel.

Selected summary statistics for these variables are as follows: The mean of \( \ln(value) \) is around $12 billion US Dollars, and the mean for \( \ln(CoreJewPop) \) is 2.54 Jews. These statistics are sensible and are in line with expectations. The median (non-logged) value of Israeli imports is $124,000, and the maximum is at around $24 billion, reflecting 2015 U.S. imports from Israel. The median for \( CoreJewPop \) is zero, and the maximum is 5,700,000, the U.S. Jewish population in 2015.

4. Empirical Analysis
4.1 Hypothesis
In asking whether Jewish networks play a role in Israeli Trade, my hypothesis is that the coefficient on \( CoreJewPop \) (\( \beta_1 \)) will be statistically significantly different from zero.

\[
H_0: \beta_1 = 0 \\
H_A: \beta_1 \neq 0
\]

4.2 Main Specification [Question #1: Do Jewish Networks Matter?]
As mentioned earlier, the gravity model of trade has a long history in the field of international trade. This paper utilizes a version of the model which mirrors that of Rauch and Trindade (2002). As in most gravity model specifications, this means that the log of country GDP is used to capture economic mass. The most common measure of bilateral trade costs, the log of distance between two countries, is also included. As discussed earlier, though, trade costs encompass much more than just distance, and unlike economic mass, the variables used to capture trade costs are much more varied and remain open to debate.

This paper controls for the same factors aiding or resisting trade as employed in Frankel (1997). First, a measure of adjacency is included. This is because the distance between Tel Aviv and Amman, for example, is a much less complete measure of the physical separation between Israel and Jordan than the distance between Tel Aviv and New York is for the distance between Israel and the U.S.. Common language or past direct colonial relationship between two countries, which might explain some cultural components of trade, are also included. Finally, I follow Frankel in including dummies

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5 While Israel also borders Lebanon and Syria, these countries are not included in the dataset because they do not trade with Israel.
6 Contrary to what readers might expect, Jordan imports Israeli goods in all three of the years observed. Israel’s other contiguous neighbor, Egypt, imports Israeli goods in every year but 2015.
indicating membership in certain trade organizations.\textsuperscript{7}

The main independent variables explored in this paper reflect those of Rauch and Trindade (2002). The authors use (1) the product of two countries’ ethnic Chinese populations and (2) the product of two countries’ ethnic Chinese population shares. Because Israel is a member of every country-pair this paper evaluates, I look not at the products but at the populations and shares themselves. My two independent variables, therefore, are (1) a country’s total core Jewish population\textsuperscript{8} and (2) the share of Jews within the larger population.\textsuperscript{9}

My main specification focuses on the first of these two independent variables, given that this variable does a better job at answering the question at hand. It is the sheer number of Jews within a country that I expect will have a larger impact on imports from Israel than the share, as the share of Jews is impacted significantly by the size of the country. Among the top twenty countries with the largest shares of Jewish population, a significant portion are very small countries/territories like Gibraltar, the Virgin Islands, and Estonia (See Figure 2).

The main ordinary least squares (“OLS”) specification utilized is written as follows:

\begin{equation}
\text{LnValue}_{j,t} = \beta_1 (\text{lnCoreJewPop})_{j,t} + \alpha_1 (\text{lnGDPd})_{j,t} + \alpha_2 (\text{lnDistw})_{j,t} + \alpha_3 (\text{contig})_{j,t} + \alpha_4 (\text{comlang_ethno})_{j,t} + \alpha_5 (\text{colony})_{j,t} + \alpha_6 (\text{gatt_d})_{j,t} + \alpha_7 (\text{fta_wto})_{j,t}
\end{equation}

The gravity controls utilized ($\alpha_1$ – $\alpha_7$) capture the GDP of the destination country, the weighted distance between that country and Israel, whether that country shares a border, common language, or colonial relationship with Israel, and whether that country is a WTO member or has signed a trade agreement with Israel.\textsuperscript{10}

I include data from each of three years (1995, 2005, 2015). I do this in order to ensure that any relationship I capture between Jewish population and trade with Israel is not the result of any external, time-contingent factor. However, because there has been very little variation in Jewish population by country since the dissolution of the Soviet Union before my evaluation period even begins, I run regressions on the cross-sectional level for each year, rather than on the panel level. In order to bolster my findings, I compare my results across years.

Lastly, it is important to note that my data only includes countries that trade with Israel in the first place.\textsuperscript{11} Therefore, on a more technical level, my results answer the question of whether, \textit{assuming a country does trade with Israel}, the country’s Jewish population exhibits network effects in the importation of Israeli goods.

4.3 Endogeneity

I contend that there is little reason for concern regarding the endogeneity of the independent variable. While it may be that there is some unobserved characteristic that is really behind the relationship observed, I argue that my specification resolves most feasible concerns. Whereas there is likely worry that Jews prefer to live in countries with certain characteristics, I argue that controlling for country GDP, trade agreement with Israel, and distance from Israel accounts for most, if not all, of this concern. Furthermore, those countries with significant Jewish populations are actually quite varied. Of the twenty-four countries with 2015 Diaspora Jewish populations greater than 10,000, thirteen are in Europe, three are in North America, four are in South America, and the remaining four are South Africa, Panama, Australia and Turkey.

4.4 Results

Given that 2015 is the most recent year in my data and therefore the most applicable to today’s world (demography, trade patterns, etc.), I focus in this paper on the results from the 2015 cross-sectional regressions. By this logic, as seen in column

\textsuperscript{7} A detailed account of these variables is provided above.
\textsuperscript{8} See Figure 1 for a breakdown of the global Jewish population by country.
\textsuperscript{9} This second independent variable is used in an alternative specification.
\textsuperscript{10} For a more detailed explanation of these variables, see page 7 “Data”.
\textsuperscript{11} For 2015, this means the data covers 131 countries.
1 of Table 1, a one percent increase in Core Jewish Population is associated with a 0.17 percent increase in the value of Israeli imports. This result seems reasonable given its magnitude, and it is significant at the 5% level. Furthermore, it is in line with both the 2005 and 1995 results (columns 2 and 3).

4.5 Specification Tests
An alternative specification swaps the main independent variable from \( \ln \text{CoreJewPop} \) to \( \text{JewsPer1000} \) in order to see if it is truly population shares that are most influential. This variable measures the share of Jews per 1000 people in the country. As per my expectation, the coefficient on this variable is less telling than the level measure used in the main specification, indicating a .03 percent increase in the value of imports from a one percentage point increase in \( \text{JewsPer1000} \), though not statistically significant at any level (See Table 2, column 1).

4.6 Robustness Checks
4.6.1 Examining only countries with Jewish populations
Because in my aggregate dataset there are nearly twice as many observations with \( \text{CoreJewPop} \) equal to zero than with \( \text{CoreJewPop} \) greater than zero, I focus here only on those observations with existing Jewish populations. I drop all other observations in order to assess whether the results presented earlier are based on the rarity of a Jewish population in the first place. The coefficient remains positive, but is smaller and not significant (See Table A1).

4.6.2 Dropping the U.S.
Because the U.S. has more than twelve times as many Jews as the next most densely populated Diaspora country, France, it is a considerable outlier. By removing the U.S from the evaluation, we ensure that our results are not biased in any way by this fact, nor by the close political relationship between the U.S. and Israel. Even without the U.S, our main result remains positive and significant. As seen in column 1 of Table A2, a one percent increase in Core Jewish Population is once again associated with a 0.17 percent increase in the value of imports from Israel.

4.6.3 OECD vs. Non-OECD Countries
The question remains as to whether a certain group of countries is driving our positive result. In this robustness check, I evaluate whether members of the Organization for Economic Cooperation and Development (“OECD”), of which Israel is a part, are the countries whose trade patterns with Israel are most likely to be influenced by their Jewish populations. This evaluation, as seen in Table A3, tells us that the positive and significant relationship we observe stands for both OECD and Non-OECD members, though it is slightly stronger and slightly more significant for non-OECD countries.

4.6.4 Pooling the Years
In a final check, I pool the three years of data together to affirm that the observed relationship holds over time. Whereas I have already shown that the relationship is positive and significant in each year at the cross-sectional level, these results, as seen in column 1 of Table A4, prove that on average, over time, a one percent increase in Jewish population is tied to a 0.13 percent increase in the value of Israeli imports. This result is significant even at the one percent level.

4.7 Mechanisms [Question #2: How do these networks operate?]
In an attempt to understand the mechanisms underlying the relationship captured above, I evaluate whether network-specific tastes play a role. I now swap my aggregate import data for another UN Comtrade dataset, one that details imports on nine specific HS codes. I divide these HS codes into a “treatment” and a “control” group, where the treatment group is comprised of cultural goods that I hypothesize will be imported more heavily to areas with Jewish populations. The control group, meanwhile, is comprised of Israel’s most common exports – goods which I hypothesize will not be imported differentially because of network effects (see Figure 3).

12 Unsurprisingly, the effect of interest is much smaller than the effect of, say, increasing GDP by one percent.
13 See Figure A1 for a graphical representation of the main relationship of interest.
14 This specification includes year fixed effects, meaning that any year-specific characteristics are accounted for and are not biasing the results.
I return again to my main specification, equation (1). This time, however, lnvalue does not capture the aggregate value of trade, rather the value of trade in the treatment group alone. These cultural goods are as follows: books, artwork, copper articles (including Judaica), cocoa products, and wine. In order to ensure no single good is driving our results, I control for commodity code fixed effects. This means that any trends specific to just one of the goods are accounted for. Output for this regression can be found in column 1 of Table 3, implying that a one percent increase in Core Jewish Population is associated with a .06 percent increase in the value of these imports, significant at the 5 percent level. As a comparison, when I performed this same regression on the “control” group, I received no significant results (Table 4).15

4.8 Specification Tests
In an alternate specification for this question, I test directly whether Core Jewish Population has a differential impact on the treatment group of goods described above as compared to the control group, comprised of Israel’s most commonly exported goods. I expect that the presence and size of a Jewish population is less likely to influence trade in the control goods, such as diamonds and refined petroleum, than to influence trade in the cultural goods mentioned above. In order to capture this difference, I include in my specification an interaction between a dummy for the “treatment” group of goods and CoreJewPop. The results of this alternate specification can be found in Table 5, and are not statistically significant.

4.9 Robustness Tests
For this second question, I run the same four robustness tests that I applied to the first: (1) dropping all countries with no Jewish Population, (2) dropping the U.S., (3) testing for OECD vs. Non-OECD countries, and (4) pooling the years. Of the four, only tests (2) and (4) leave us with results that remain positive and statistically significant.

I also apply a fifth test, which reflects an attempt to account for a shortcoming of my data: namely, that countries that don’t trade in the treatment goods are not included my main regressions. In this test, I focus my sample only on the year 2015 and only on trade in one of my “treatment” goods, books. I compare the list of countries that imported from Israel in 2015 generally with those that imported books specifically. If a country did trade with Israel in 2015 but reports no imports of books, I code the value of their imports of books as zero.16 After running my specification on this limited dataset, Jewish population remains positively associated with the value of imports, though not at any statistically significant level. This result may not be reflective of the inclusion of zeroes in the regression overall, but it does give us the impression that including zeroes would not change the sign of our results in any unexpected way (See Table A6).

5. Conclusion
Economically, the interpretation of these results implies that the size of a country’s Jewish population plays a significant role in its importing of Israeli goods. While understanding the mechanisms by which this relationship manifests itself is less clear-cut, it does seem that tastes play a role, as Jewish Population has a significant impact on the value of cultural imports.

Still, the results of this paper should be read with a conscious and critical eye for several reasons. First, I am constrained by the data available to me. There is an inherent challenge involved in gathering population data on world Jewry, as it is not collected uniformly, and because it is tough to define Jewish identity. Indeed, for some countries with very small Jewish populations, such as Costa Rica, Peru, and Kenya, no new data has been collected since 1993, meaning the numbers used in each of my cross sections are the same. While I am confident these flaws do not inhibit my principal investigation, they are important to note.

Perhaps even more important to note is the fact that my data reflects only those countries that trade with Israel (either overall, as in question one, or in the treatment group, as in question two). Therefore, with the exception of one robustness check, my data does not include values of trade equal to zero.

Second, I am constrained by time. My evaluation of the mechanisms behind Jewish network effects on trade focuses on

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15 See Figure A2 for a graphical representation of the relationship captured in this regression.
16 46 of 131 countries in this sample do not report any imports of books.
tastes rather than on the dissolution of information barriers. Rauch and Trindade (2002) conclude that ethnic Chinese populations increase trade in differentiated products (those without reference prices) more-so than homogenous products (those with reference prices). This paper does not break imports down along these lines. If, as in the case of Chinese populations, this sort of breakdown plays a key role in explaining Jewish networks, then further research is needed.

Another opportunity for further research would be to perform this same investigation on the state level. There is much less room for confounding variables within the United States than there is globally, and being able to compare whether states with larger Jewish populations trade more with Israel than those with smaller ones may be very telling.

A final piece that would serve to bolster these findings would be to evaluate this question using shocks to Jewish population. Because in recent decades most shocks to Jewish population have shifted the Jewish population to Israel,\textsuperscript{17} a question that grows out of this work might be whether immigration has a substantial impact on Israeli trade patterns.

References


**Appendices**

**Figure 1:** Jewish population by country

![Pie chart showing the distribution of Jewish population by country.](image)

*World Jewish Population, 2015 (Total: 14,310,500)*

**Figure 2:** Jewish population share by country

<table>
<thead>
<tr>
<th>Country</th>
<th>Jews Per 1000 People (2015)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Israel</td>
<td>738.54</td>
</tr>
<tr>
<td>Gibraltar</td>
<td>20</td>
</tr>
<tr>
<td>United States</td>
<td>17.94</td>
</tr>
<tr>
<td>Canada</td>
<td>10.87</td>
</tr>
<tr>
<td>France</td>
<td>7.29</td>
</tr>
<tr>
<td>Uruguay</td>
<td>5.03</td>
</tr>
<tr>
<td>Hungary</td>
<td>4.82</td>
</tr>
<tr>
<td>Australia</td>
<td>4.8</td>
</tr>
<tr>
<td>Virgin Islands (U.S.)</td>
<td>4.55</td>
</tr>
<tr>
<td>United Kingdom</td>
<td>4.48</td>
</tr>
<tr>
<td>Argentina</td>
<td>4.24</td>
</tr>
<tr>
<td>Belgium</td>
<td>2.66</td>
</tr>
<tr>
<td>Latvia</td>
<td>2.6</td>
</tr>
<tr>
<td>Panama</td>
<td>2.56</td>
</tr>
<tr>
<td>Switzerland</td>
<td>2.3</td>
</tr>
<tr>
<td>Netherlands</td>
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</tr>
<tr>
<td>New Zealand</td>
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</tr>
<tr>
<td>Estonia</td>
<td>1.62</td>
</tr>
<tr>
<td>Sweden</td>
<td>1.55</td>
</tr>
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**Table 1: Question 1, Main Specification** [Equation (1); independent variable *CoreJewPop*]

<table>
<thead>
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<th>VARIABLES</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
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<tbody>
<tr>
<td>InCoreJewPop</td>
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<td>0.0848*</td>
<td>0.207***</td>
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<tr>
<td></td>
<td>(0.0749)</td>
<td>(0.0475)</td>
<td>(0.0763)</td>
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<td>InGDPd</td>
<td>0.955***</td>
<td>1.168***</td>
<td>0.920***</td>
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<tr>
<td></td>
<td>(0.141)</td>
<td>(0.122)</td>
<td>(0.138)</td>
</tr>
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<td>Indistw</td>
<td>-0.410</td>
<td>-0.854***</td>
<td>-0.451*</td>
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<tr>
<td></td>
<td>(0.337)</td>
<td>(0.225)</td>
<td>(0.250)</td>
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<tr>
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<td>-3.853</td>
</tr>
<tr>
<td></td>
<td>(3.088)</td>
<td>(3.406)</td>
<td>(3.420)</td>
</tr>
<tr>
<td>Year</td>
<td>2015</td>
<td>2005</td>
<td>1995</td>
</tr>
<tr>
<td>Observations</td>
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<tr>
<td>R-squared</td>
<td>0.719</td>
<td>0.717</td>
<td>0.822</td>
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</table>

Add. Ctrls: contig, comlang_ethno, colony, gatt_d, fto_wto
Robust standard errors in parentheses

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18 In “Question 1” regressions, the dependent variable *lnvalue* captures the value of imports from Israel in aggregate.
Table 2: Question 1, Alternate Specification [Independent variable JewsPer1000; shares instead of levels]

<table>
<thead>
<tr>
<th>VARIABLES</th>
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<td>JewsPer1000</td>
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<td>1.241***</td>
<td>1.151***</td>
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<td>(0.0873)</td>
<td>(0.0967)</td>
<td>(0.0931)</td>
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<tr>
<td>lnDistw</td>
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<td>-0.840***</td>
<td>-0.393</td>
</tr>
<tr>
<td>(0.366)</td>
<td>(0.226)</td>
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<td>Constant</td>
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<td>-6.908**</td>
<td>-8.551***</td>
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<tr>
<td>(2.922)</td>
<td>(3.032)</td>
<td>(2.770)</td>
<td></td>
</tr>
<tr>
<td>Year</td>
<td>2015</td>
<td>2005</td>
<td>1995</td>
</tr>
<tr>
<td>Observations</td>
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<td>135</td>
<td>91</td>
</tr>
<tr>
<td>R-squared</td>
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<td>0.711</td>
<td>0.789</td>
</tr>
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</table>

Robust standard errors in parentheses
Add.Ctrls: contig, comlang_ethno, colony, gatt_d, fta_wto

*** p<0.01, ** p<0.05, * p<0.1

Table 3: Question 2, Main Specification (Treatment) [Dependent variable captures value of treatment goods only]¹⁹

<table>
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<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
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<td>0.0627*</td>
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<td>0.0921*</td>
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<td>0.0921*</td>
<td>0.108***</td>
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<tr>
<td>(0.0461)</td>
<td>(0.0502)</td>
<td>(0.0272)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(2.523)</td>
<td>(2.121)</td>
<td>(2.531)</td>
<td>(2.603)</td>
<td>(2.143)</td>
<td>(2.519)</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>278</td>
<td>243</td>
<td>152</td>
<td>278</td>
<td>243</td>
<td>152</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.457</td>
<td>0.564</td>
<td>0.529</td>
<td>0.470</td>
<td>0.561</td>
<td>0.542</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses
Additional Controls: lnGDPd, lnDistw, contig, comlang_ethno, colony, gatt_d, fta_wto

*** p<0.01, ** p<0.05, * p<0.1

¹⁹ In “Question 2” regressions, the independent variable InValue measures the value of only one group of imports.
**Table 4:** Question 2, Main Specification (Control) [Dependent variable captures the value of control goods only]

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>InCoreJewPop</td>
<td>-0.000310</td>
<td>-0.0500</td>
<td>0.000309</td>
<td>-0.145</td>
<td>0.0340</td>
<td>-0.00987</td>
</tr>
<tr>
<td></td>
<td>(0.0430)</td>
<td>(0.0485)</td>
<td>(0.0459)</td>
<td>(0.187)</td>
<td>(0.0752)</td>
<td>(0.0657)</td>
</tr>
<tr>
<td>JewsPer1000</td>
<td>-8.477***</td>
<td>-10.53***</td>
<td>-1.885</td>
<td>-8.680***</td>
<td>-10.02***</td>
<td>-2.041</td>
</tr>
<tr>
<td></td>
<td>(3.229)</td>
<td>(2.884)</td>
<td>(3.390)</td>
<td>(3.247)</td>
<td>(2.913)</td>
<td>(3.501)</td>
</tr>
<tr>
<td>Constant</td>
<td>-0.390</td>
<td>0.524</td>
<td>0.459</td>
<td>0.390</td>
<td>0.522</td>
<td>0.459</td>
</tr>
<tr>
<td>Observations</td>
<td>282</td>
<td>262</td>
<td>160</td>
<td>282</td>
<td>262</td>
<td>160</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.390</td>
<td>0.524</td>
<td>0.459</td>
<td>0.390</td>
<td>0.522</td>
<td>0.459</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses

Additional Controls: lnGDPd, Indis tw, contig, comlang_ethno, colony, gatt d, fta_wto

*** p<0.01, ** p<0.05, * p<0.1

**Table 5:** Question 2, Alternate Specification [Independent variable is an interaction between treatment and InCoreJewPop]

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Ob.TreatDummy#c.InCoreJewPop</td>
<td>0.0271</td>
<td>-0.0475</td>
<td>-0.122**</td>
<td>0.0271</td>
<td>-0.0475</td>
<td>-0.122**</td>
</tr>
<tr>
<td></td>
<td>(0.0404)</td>
<td>(0.0518)</td>
<td>(0.0489)</td>
<td>(0.0404)</td>
<td>(0.0518)</td>
<td>(0.0489)</td>
</tr>
<tr>
<td>1.TreatDummy#c.InCoreJewPop</td>
<td>0.0597***</td>
<td>0.0507</td>
<td>0.0242</td>
<td>0.0597***</td>
<td>0.0507</td>
<td>0.0242</td>
</tr>
<tr>
<td></td>
<td>(0.0286)</td>
<td>(0.0327)</td>
<td>(0.0284)</td>
<td>(0.0286)</td>
<td>(0.0327)</td>
<td>(0.0284)</td>
</tr>
<tr>
<td>Ob.TreatDummy#c.JewsPer1000</td>
<td>0.137**</td>
<td>-0.271</td>
<td>-0.0462</td>
<td>0.137**</td>
<td>-0.271</td>
<td>-0.0462</td>
</tr>
<tr>
<td></td>
<td>(0.0678)</td>
<td>(0.289)</td>
<td>(0.104)</td>
<td>(0.0678)</td>
<td>(0.289)</td>
<td>(0.104)</td>
</tr>
<tr>
<td>1.TreatDummy#c.JewsPer1000</td>
<td>0.169***</td>
<td>0.102**</td>
<td>0.108***</td>
<td>0.169***</td>
<td>0.102**</td>
<td>0.108***</td>
</tr>
<tr>
<td></td>
<td>(0.0454)</td>
<td>(0.0453)</td>
<td>(0.0240)</td>
<td>(0.0454)</td>
<td>(0.0453)</td>
<td>(0.0240)</td>
</tr>
<tr>
<td>Constant</td>
<td>9.747***</td>
<td>12.21***</td>
<td>-2.639</td>
<td>8.533***</td>
<td>12.08***</td>
<td>-2.221</td>
</tr>
<tr>
<td></td>
<td>(2.064)</td>
<td>(1.787)</td>
<td>(2.131)</td>
<td>(2.064)</td>
<td>(1.787)</td>
<td>(2.131)</td>
</tr>
<tr>
<td>Observations</td>
<td>560</td>
<td>505</td>
<td>312</td>
<td>560</td>
<td>505</td>
<td>312</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.527</td>
<td>0.621</td>
<td>0.549</td>
<td>0.531</td>
<td>0.620</td>
<td>0.542</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses

Additional Controls: lnGDPd, Indis tw, contig, comlang_ethno, colony, gatt d, fta_wto

*** p<0.01, ** p<0.05, * p<0.1
Figure A1: Relationship between CoreJewPop and total value of imports

Figure A2: Relationship between CoreJewPop and value of treatment goods
Table A1: Q1 Robustness Check: Drop if CoreJewPop = 0

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>lnCoreJewPop</td>
<td>0.0431</td>
<td>-0.0539</td>
<td>0.00608</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.0476)</td>
<td>(0.0781)</td>
<td>(0.0701)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>JewsPer1000</td>
<td></td>
<td></td>
<td></td>
<td>0.0722***</td>
<td>0.0171</td>
<td>0.0163</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>(0.0293)</td>
<td>(0.0347)</td>
<td>(0.0820)</td>
</tr>
<tr>
<td>Constant</td>
<td>-3.999</td>
<td>-5.992*</td>
<td>-2.513</td>
<td>-3.356</td>
<td>-5.563*</td>
<td>-2.259</td>
</tr>
<tr>
<td></td>
<td>(2.687)</td>
<td>(3.199)</td>
<td>(2.042)</td>
<td>(2.702)</td>
<td>(3.104)</td>
<td>(1.981)</td>
</tr>
<tr>
<td>Observations</td>
<td>74</td>
<td>46</td>
<td>56</td>
<td>74</td>
<td>46</td>
<td>56</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.785</td>
<td>0.851</td>
<td>0.898</td>
<td>0.790</td>
<td>0.851</td>
<td>0.899</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses

Additional Controls: lnGDPd, Indistw, contig, comlang, ethno, colony, gatt_d, fta_wto

*** p<0.01, ** p<0.05, * p<0.1

Table A2: Q1 Robustness Check: Drop US

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>lnCoreJewPop</td>
<td>0.168***</td>
<td>0.0851*</td>
<td>0.209***</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.0751)</td>
<td>(0.0491)</td>
<td>(0.0774)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>JewsPer1000</td>
<td></td>
<td></td>
<td></td>
<td>0.0454</td>
<td>0.0802</td>
<td>0.123</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>(0.0608)</td>
<td>(0.117)</td>
<td>(0.0771)</td>
</tr>
<tr>
<td>Constant</td>
<td>-5.815*</td>
<td>-5.311</td>
<td>-3.985</td>
<td>9.827***</td>
<td>-6.910**</td>
<td>8.601***</td>
</tr>
<tr>
<td></td>
<td>(3.161)</td>
<td>(3.438)</td>
<td>(3.503)</td>
<td>(2.918)</td>
<td>(3.034)</td>
<td>(2.786)</td>
</tr>
<tr>
<td>Observations</td>
<td>124</td>
<td>134</td>
<td>90</td>
<td>124</td>
<td>134</td>
<td>90</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.708</td>
<td>0.707</td>
<td>0.814</td>
<td>0.686</td>
<td>0.701</td>
<td>0.781</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses

Additional Controls: lnGDPd, Indistw, contig, comlang, ethno, colony, gatt_d, fta_wto

*** p<0.01, ** p<0.05, * p<0.1

---

20 “Q1” refers to “Question 1.” In Q1 regressions, the dependent variable lnValue captures the value of imports from Israel in aggregate.
### Table A3: Q1 Robustness Check: OECD vs. Non-OECD

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1)</th>
<th>(4)</th>
<th>(7)</th>
<th>(10)</th>
</tr>
</thead>
<tbody>
<tr>
<td>lnCoreJewPop</td>
<td>0.0791*</td>
<td>0.180**</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.0408)</td>
<td>(0.0868)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>lnGDPd</td>
<td>1.010***</td>
<td>1.068***</td>
<td>0.968***</td>
<td>1.157***</td>
</tr>
<tr>
<td></td>
<td>(0.0714)</td>
<td>(0.0745)</td>
<td>(0.176)</td>
<td>(0.118)</td>
</tr>
<tr>
<td>lnDistw</td>
<td>-0.620**</td>
<td>-0.704**</td>
<td>-0.329</td>
<td>-0.311</td>
</tr>
<tr>
<td></td>
<td>(0.239)</td>
<td>(0.313)</td>
<td>(0.392)</td>
<td>(0.424)</td>
</tr>
<tr>
<td>JewsPer1000</td>
<td>0.0472</td>
<td>0.212</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.0412)</td>
<td>(0.177)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>-2.991</td>
<td>-3.157</td>
<td>-6.830</td>
<td>10.66***</td>
</tr>
<tr>
<td></td>
<td>(2.981)</td>
<td>(3.996)</td>
<td>(4.106)</td>
<td>(3.704)</td>
</tr>
<tr>
<td>OECD</td>
<td>Yes</td>
<td>Yes</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>Year</td>
<td>2015</td>
<td>2015</td>
<td>2015</td>
<td>2015</td>
</tr>
<tr>
<td>Observations</td>
<td>34</td>
<td>34</td>
<td>91</td>
<td>91</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.888</td>
<td>0.885</td>
<td>0.590</td>
<td>0.561</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses

Additional Controls: comlang_ethno, colony, fta_wto, gatt_d, contig

* *** p<0.01, ** p<0.05, * p<0.1

### Table A4: Q1 Robustness Check: Pooling Years

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1)</th>
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</tr>
</thead>
<tbody>
<tr>
<td>lnCoreJewPop</td>
<td>0.128***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.0407)</td>
<td></td>
</tr>
<tr>
<td>lnGDPd</td>
<td>1.049***</td>
<td>1.176***</td>
</tr>
<tr>
<td></td>
<td>(0.0835)</td>
<td>(0.0605)</td>
</tr>
<tr>
<td>lnDistw</td>
<td>-0.562***</td>
<td>-0.543***</td>
</tr>
<tr>
<td></td>
<td>(0.196)</td>
<td>(0.202)</td>
</tr>
<tr>
<td>JewsPer1000</td>
<td></td>
<td>0.0587</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.0370)</td>
</tr>
<tr>
<td>Constant</td>
<td>-5.548**</td>
<td>-8.157***</td>
</tr>
<tr>
<td></td>
<td>(2.365)</td>
<td>(2.225)</td>
</tr>
<tr>
<td>Observations</td>
<td>351</td>
<td>351</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.740</td>
<td>0.726</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses

AddCtrls: contig, comlang_ethno, colony, gatt_d, fta_wto, CommodityDummy

Clustered by Country Code, Absorbed: Year

* *** p<0.01, ** p<0.05, * p<0.1
**Table A5:** Q2 Robustness Check: Drop US

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1) InValue</th>
<th>(4) InValue</th>
<th>(7) InValue</th>
<th>(10) InValue</th>
</tr>
</thead>
<tbody>
<tr>
<td>InCoreJewPop</td>
<td>0.0543*</td>
<td>0.00689</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.0293)</td>
<td>(0.0434)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>lnGDPd</td>
<td>0.833***</td>
<td>0.844***</td>
<td>1.122***</td>
<td>1.123***</td>
</tr>
<tr>
<td></td>
<td>(0.0847)</td>
<td>(0.0841)</td>
<td>(0.104)</td>
<td>(0.103)</td>
</tr>
<tr>
<td>Indistw</td>
<td>-0.458*</td>
<td>-0.516**</td>
<td>1.156***</td>
<td>1.166***</td>
</tr>
<tr>
<td></td>
<td>(0.248)</td>
<td>(0.250)</td>
<td>(0.363)</td>
<td>(0.360)</td>
</tr>
<tr>
<td>JewsPer1000</td>
<td>0.193***</td>
<td></td>
<td>0.0538</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.0696)</td>
<td></td>
<td>(0.131)</td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>9.850***</td>
<td>9.539***</td>
<td>-6.530**</td>
<td>-6.462**</td>
</tr>
<tr>
<td></td>
<td>(2.627)</td>
<td>(2.643)</td>
<td>(3.245)</td>
<td>(3.250)</td>
</tr>
<tr>
<td>Group</td>
<td>treatment</td>
<td>treatment</td>
<td>Control</td>
<td>Control</td>
</tr>
<tr>
<td>Year</td>
<td>2015</td>
<td>2015</td>
<td>2015</td>
<td>2015</td>
</tr>
<tr>
<td>Observations</td>
<td>273</td>
<td>273</td>
<td>278</td>
<td>278</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.431</td>
<td>0.432</td>
<td>0.362</td>
<td>0.362</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses.

Additional Controls: contig, comlang_ethno, colony, gatt_d, fta_wto

*** p<0.01, ** p<0.05, * p<0.1

**Table A6:** Q2 Robustness Check: Including zeroes & limiting sample to 2015 imports of books

<table>
<thead>
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<th>VARIABLES</th>
<th>(1) InValue</th>
<th>(2) InValue</th>
</tr>
</thead>
<tbody>
<tr>
<td>InCoreJewPop</td>
<td>0.0538</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.0461)</td>
<td></td>
</tr>
<tr>
<td>lnGDPd</td>
<td>0.717***</td>
<td>0.704***</td>
</tr>
<tr>
<td></td>
<td>(0.137)</td>
<td>(0.138)</td>
</tr>
<tr>
<td>Indistw</td>
<td>-0.968***</td>
<td>-1.103***</td>
</tr>
<tr>
<td></td>
<td>(0.354)</td>
<td>(0.380)</td>
</tr>
<tr>
<td>JewsPer1000</td>
<td>0.163***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.0566)</td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>-1.403</td>
<td>0.142</td>
</tr>
<tr>
<td></td>
<td>(3.936)</td>
<td>(4.136)</td>
</tr>
<tr>
<td>Observations</td>
<td>84</td>
<td>84</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.569</td>
<td>0.578</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses.

Addtl Controls: Contig, Comlang_ethno, Colony, gatt_d, fta_wto

*** p<0.01, ** p<0.05, * p<0.1
THE EFFECT OF AID ON TRANSPARENCY: IDA THRESHOLD-CROSSING AS A QUASI-EXPERIMENT

ARJUN KRISHNAN
Georgetown University

ABSTRACT
What is the effect of aid on transparency? I examine the political economy of aid commitments with respect to three of the six largest donors globally: the United States, Japan, and Sweden. First, I empirically investigate this question via OLS regression on a panel dataset of 985 observations of 56 countries from 1980 to 2010. In order to address the potential endogeneity of the aid-transparency relationship, I exploit an instrumental variable constructed by Galiani et al. (2017) based on the fact that, since 1987, eligibility for aid from the International Development Association (IDA) has been partially determined by a country’s level of per capita income with respect to a defined threshold, adjusted year-on-year only for inflation. I focus on the 35 countries that have crossed the income threshold from below between 1987 and 2010. A sizable, positive, and statistically significant effect of aid on transparency is found for the United States and Japan. In the case of Sweden, I find that there is no meaningful association between aid commitments and transparency. These results are consistent with the prevailing theory governing each country’s stated aid policy: the United States and Japan traditionally lever foreign aid as a means of influencing government conditions in recipient countries, whereas Sweden is largely focused on poverty-reduction.¹

¹ I would like to thank Professor Ludema and Professor Cumby for their guidance and feedback on my thesis over the past semester. I would also like to thank Professor Vreeland and Meghna Sinha for helping inspire my interest in this topic, and (in Meghna’s case) co-authoring an earlier iteration of this paper with me in IPEC-250 International Political Economy Quantitative Research Lab. Many thanks to the following people for their feedback and assistance during the editing process: Dr. Jeni Klugman, Professor Karthik Easwar, Duncan Hobbs, and Meghna Sinha.
1. Introduction
Foreign aid is a powerful shaping tool of global affairs. Its effects on transparency and institutional quality are highly debated by academics and practitioners. The majority of studies on foreign aid look at its effect on growth and per capita income (Easterly et al., 2004; Burnside and Dollar, 2000; Rajan and Subramanian, 2008; Arndt et al., 2010; Qian, 2015). This paper shifts away from that paradigm and instead focuses on the relationship between foreign aid commitments and the transparency of recipient countries. The literature on the aid-transparency relationship is mixed. Boone (1996) finds that foreign aid is most often wasted by recipient countries for public consumption with no impact on the institutional environment. Others find that aid can move the needle in terms of encouraging the adoption of good policies, but are more skeptical about the role that the quantity of aid plays in driving that relationship (Burnside and Dollar, 2000; Svensson, 2000). Some contend that countries are insensitive to the institutional quality of recipient countries and are instead primarily motivated by strategic considerations (Alesina and Dollar, 2000).

This paper adds to the literature by helping shed light on two questions. First, does foreign aid have a meaningful relationship with the transparency of its recipient countries? Second, if a meaningful aid-transparency association exists, in what direction does the relationship run? I measure aid policy’s ability to influence changes in transparency by evaluating the extent to which U.S., Japanese, and Swedish commitments explain variations in transparency between recipient states. Using a new index of transparency that focuses on the availability of credible, aggregate economic data, I define transparency as government disclosure of policy-relevant macroeconomic data to the public. I empirically investigate this question by first using OLS regression on a panel dataset of 985 observations of 56 countries between the years of 1980 and 2010. I find positive, statistically significant, and sizable results for the association between aid and transparency for all 3 countries.

Identification of the causal effect of aid on transparency has been elusive so far due to the endogeneity of foreign aid. I address this challenge by using an instrumental variable constructed by Galiani et al. (2017). This instrumental variable exploits the fact that, since 1987, eligibility for aid from the International Development Association (IDA) has been determined by a country’s per capita income relative to a pre-selected threshold. Galiani et al. (2017) show that other major donors use the IDA threshold as an informative signal about where development aid is most needed by demonstrating that total aid declines significantly once a recipient country crosses the IDA income threshold from below. The IDA threshold is ultimately an arbitrary level that does not reflect any structural transformation in economic growth. Threshold-crossing is therefore a plausibly valid and exogenous instrumental variable. There is no apparent channel of influence between threshold-crossing and transparency except through the impact that threshold-crossing has on aid receipts, thereby satisfying the exclusion restriction.

I focus on the 35 countries that have crossed the income threshold from below between 1987 and 2010. Using 2SLS regression, I find a sizable, positive, and statistically significant effect of aid on transparency for the United States and Japan. In the case of Sweden, I find that there is no meaningful association between aid commitments and transparency. The results are consistent with theory, which suggests that the United States and Japan frequently leverage aid policy as a means to influence governance conditions in recipient countries, while Sweden is motivated by other considerations.

2. Why transparency?
Transparency is an important determinant of institutional quality and economic growth. In that sense, measuring the association between aid and transparency will shed light on its relationship with an important determinant of economic development. Numerous studies confirm the positive effect of transparency on reducing corruption and stimulating economic activity (Stiglitz, 1999; Kaufmann and Bellver, 2005; Hameed, 2005; Hollyer et al., 2016).

We define transparency as the public’s ability to understand the link between government policies and their outcomes, as well as the quantity and accuracy of information that the government disseminates (Hollyer et al., 2016: 34; Broz, 2002; Kopits and Craig, 1998). Under this broad definition, we look specifically at macroeconomic transparency.
We operationalize transparency using the HRV Index (Hollyer et al., 2014; 2016). The HRV Index looks at the dissemination, or lack thereof, of credible data. The creators use missing data – specifically the missingness of aggregate economic data – to their advantage. They argue that “the availability of credible information in a polity [is] a measure of transparency” (Hollyer et al., 2014, 2016: 40). This involves the aggregation of data from the World Bank’s World Development Indicators and interprets the frequency of missing values as a key input for each country’s transparency score. The reasoning behind this is that missing data is systematic; the tendency to disclose accurate economic data speaks to a government’s overall level of transparency and accountability. Many other measures of transparency use subjective judgment from experts in order to determine transparency rankings and scores. The HRV Index develops a more precise, rigorous, and objective measure of transparency.

3. Choosing the U.S., Japan, and Sweden

I choose the U.S., Japan, and Sweden as my countries of analysis due to their different philosophies on aid policy. Broadly speaking, all 3 countries have varying attitudes on the role of foreign aid and the extent to which it can (and should) be levered in order to engender changes in governance conditions in recipient countries. Due to this natural variation in philosophy, I contend that these three countries would be interesting cases to analyze in the context of identifying a relationship between aid commitments and transparency.

In the U.S., the attitudes surrounding aid have evolved over time. Starting with the Point Four program of Harry S. Truman in 1949, the President advocated for “embark[ing] on a bold new program for the improvement and growth of underdeveloped areas” (Easterly, 2002). In 1961, John F. Kennedy changed the rhetoric around aid to suggest that is a concept that has an expiration date, aiming “to achieve a decisive turnaround in the fate of the less-developed world, looking toward the ultimate day...when foreign aid will no longer be needed” (Easterly, 2002).

In that sense, the U.S. has a diverse set of attitudes with respect to its aid program. On one hand, the U.S. levers aid as a means to improve welfare in poorer countries. On the other hand, the U.S. is concerned with using aid as a means to structurally transform developing economies into self-sustaining engines of growth. My analysis will contribute to a diagnosis of the extent to which governance conditions loom large in the eyes of U.S. foreign aid planners.

Sweden's foreign aid strategy is explicitly centered on alleviating unequal distributions of wealth in the international system (Sahakyan, 2017). As opposed to leveraging aid as a mechanism to influence governance or policy conditions, Sweden's foreign aid program is more concerned with improving the welfare of recipient countries. Thus, we should anticipate a weaker, or less positive association between aid and transparency in the Swedish context.

Japan's attitude towards foreign aid is more explicit than that of the U.S. or Sweden. It has stated the goals that inform its foreign aid allocations to be “structural reform in economic and social spheres” (Sunaga, 2004). In 1992, the Japanese cabinet approved an Official Development Assistance (ODA) charter that set the guidelines for the country’s aid policy. The most recent update in 2015 lists “preventing fraud and corruption” as one of its main goals and suggests that Japan will provide assistance for “improvements in governance which include the training of civil servants and institutional capacity building for anti-corruption” (Economic Bureau, 2015). Given the existing evidence on Japan’s use of foreign aid as a key factor in their grand strategy of global activism, the results of this analysis should shed light on the effectiveness of their stated aid policy.

4. The IDA threshold

Starting in 1987, a major criterion for IDA eligibility has been whether or not a country is below a certain threshold of per capita income, measured in current U.S. dollars. This “operational threshold” was established for the purpose of rationing scarce IDA funds that were allocated for disbursement. When a country crosses this per capita income threshold, they are said to “graduate” from IDA aid eligibility and will stop receiving aid funds from the institution. From 1987 until 2010, the threshold has increased from its original setting of $580 to $1175 and has been adjusted only for inflation (Galiani et al, 2017).
Actual graduation from IDA aid disbursements (as opposed to threshold-crossing) is likely to be endogenous to institutional quality, economic performance, and policies (Galiani et al 2017). Graduation itself, as compared to threshold-crossing, would thus not serve as a valid, exogenous source of variation for aid.

Once a country has exceeded the IDA income threshold, it is considered on track for graduation from the IDA. Allowance is made for the possibility of income fluctuations, so lending volumes are reduced only after a country has remained over the threshold for three consecutive years. Thus, threshold crossing will result in reductions of IDA flows beginning in the next replenishment period, not in the current one (World Bank, 2010). This decline in aid is amplified by similar behavior from other donors. Some agencies explicitly use the IDA income threshold in their own aid eligibility criteria. Other donors view crossing the threshold as a signal that countries are in less need of aid and cut their own aid (Moss and Majerowicz, 2012; World Bank, 1989).

5. Data and sample

Figure 1. IDA Threshold 1987-2010

![Graph showing IDA Threshold 1987-2010](image)

Figure 2. Distribution of HRV Index (Dependent Variable)

![Graph showing Distribution of HRV Index](image)
Figure 2 shows the distribution of the HRV Index. The minimum value is -6.67 and the maximum is 9.98. The mean is .079. See Appendix Table A1 for descriptive statistics on the HRV Index. The line in the figure is a comparison of the histogram to a normal distribution.

For our dependent variable, we use the HRV Index, a novel measure of transparency based on the dissemination of aggregate economic data through international organizations (Hollyer et al., 2014, 2016). The index is continuous from the value -10 (least transparent) to 10 (most transparent). States with greater scores on the Index have higher levels of transparency. The index assigns a transparency score for 125 countries between the years 1980 to 2010. Figure 2 shows the distribution of the HRV Index for the 56 countries who receive aid from the donors I examine. The line in Figure 2 illustrates a comparison of the histogram to a normal distribution.

For the independent variable, we use foreign aid commitments from the U.S., Japan, and Sweden. Following the literature, we use commitments instead of disbursements because they serve as a better measure of donor intention and control over the aid (Berthelemy and Tichit, 2002; Dreher et al., 2008). The data I use is originally aggregated from the OECD Development Assistance Committee’s (DAC) Credit Reporting System. In order to create a country-year panel, I exclude aid allocated through multilateral development banks (MDBs) and international financial institutions (IFIs). While disbursements are based on a recipient’s willingness and capability to acquire the money, disbursement data suffers from a greater amount of classical measurement error, which is further explored in Section 7.3.

Figure 3. Difference of Means: Japanese Foreign Aid and High/Low Transparency

Using overlaid histograms, Figure 3 shows the differences in the distribution of Japanese foreign aid commitments for countries with high transparency (positive HRV) and low transparency (negative HRV). As a preliminary analysis, the distribution of Japanese aid commitments for high transparency countries is higher than that of low transparency countries, which suggests a positive relationship between foreign aid and transparency. See Appendix Table A2 for descriptive statistics on Japanese aid commitments.
Figure 3 demonstrates that at a preliminary level, a positive association exists between U.S. foreign aid commitments and transparency. We find that the mean logged foreign aid commitments from Japan for states with negative transparency scores is 16.20. The mean is higher for states with positive HRV at 17.31. This suggests a positive association between our independent and dependent variable of interest.

In selecting our set of control variables, we follow the literature on the determinants of transparency and similar outcome variables relating to institutional quality (Hollyer et al., 2011, 2014, 2016; Hameed, 2005; Kaufmann and Belleyer, 2005; Andreula et al., 2009). We control for GDP per capita, which likely determines a government's ability to collect and disseminate high quality statistical data. Our measure of GDP per capita is measured in PPP terms and comes from the UNdata database with no missing values.

<table>
<thead>
<tr>
<th>Variable</th>
<th>Variable Type</th>
<th>Source</th>
<th>Description</th>
</tr>
</thead>
<tbody>
<tr>
<td>Transparency</td>
<td>Dependent</td>
<td>HRV Index</td>
<td>Transparency based on the dissemination of aggregate economic data.</td>
</tr>
<tr>
<td>Foreign Aid Commitments</td>
<td>Independent</td>
<td>AidData (from OECD DAC’s Credit Reporting System)</td>
<td>Log of foreign aid commitments from the U.S., Japan, and Sweden in constant U.S. dollars (USD)</td>
</tr>
<tr>
<td>GDP per capita</td>
<td>Control</td>
<td>UNdata</td>
<td>Natural log of GDP per capita in constant U.S. dollars (USD)</td>
</tr>
<tr>
<td>Participation in IMF Programs</td>
<td>Control</td>
<td>IMF</td>
<td>Binary indicator</td>
</tr>
<tr>
<td>Regime Type (i.e. Democracy)</td>
<td>Control</td>
<td>Polity IV’s Polity 2</td>
<td>Scale of 0-20</td>
</tr>
</tbody>
</table>

We also control for participation in IMF programs. The IMF often requires that governments receiving support publish relevant economic data, since it is a pillar of the IMF Fiscal Transparency Guidelines. Vreeland (2003) notes that the IMF has tended to extend programs to dictatorships during parts of its history. Our final control is regime type, using Polity IV’s Polity2 variable, which measures regime type on a scale from 0 to 20, with 0 representing a low democracy score and 20 representing a high democracy score. Studies find that democratizing countries receive greater levels of aid (Alesina and Dollar, 2000). Democracies tend to demonstrate greater accountability to their populations, which suggests that they might be more likely to release aggregate economic data (Hollyer et al., 2011, 2014, 2017; Rosendorff and Vreeland, 2006).

6. Foreign aid and transparency

6.1 Econometric model

I posit the following model in order to test the null hypothesis that aid does not affect transparency:

\[
\text{TransparencyIndex}_{i,t} = \alpha + \text{LogAid}_{i,t} + \text{Democracy}_{i,t} + \text{IMF}_{i,t} + \text{GDP}_{i,t} + \epsilon_{i,t}
\]

Following the literature on the IDA income threshold for our IV regression specification, I lag our instrumental variable by two periods. The effects of threshold-crossing should be most pronounced two periods after crossing, since aid volumes drop most precipitously in the period after crossing (Galiani et al., 2017). The first-stage results are included in Table A3 in the appendix. The instruments are found to be strong and relevant for the U.S. and Japan, but weak in the case of Sweden.
6.2 Empirical Results

Table 2 provides the OLS regression results. It confirms that the log of aid commitments from the U.S., Japan, and Sweden are positive and statistically significant in their association with recipient transparency when adjusting for other plausible determinants of the dependent variable. Although the magnitude of the coefficient on log of aid is slightly greater for the United States when compared to Japan (.238 vs .207), the results for both countries paint a similar picture. Sweden has a relatively smaller coefficient on the log of aid when compared to the U.S. and Japan.

In the case of aid from the U.S. and Japan, even though the other control variables are statistically significant as well, the magnitude of the coefficient on the log of aid is meaningfully larger. This suggests that it is, comparatively speaking, a bigger driver of the positive result than the set of controls. In the case of Sweden, however, we observe that the coefficient on IMF Program Participation is significant and meaningfully larger in magnitude than the point estimate on the log of aid.

Of course, theory and literature suggest a multitude of difficulties in teasing out the direction of causality between foreign aid and transparency. In order to address the endogeneity of foreign aid, I introduce the IDA income threshold-crossing instrumental variable. This provides a source of quasi-experimental variation, since recipient countries experience foreign aid inflow reductions upon crossing this important threshold. Table 3 suggests that the positive and statistically significant relationship holds, confirming our results from the OLS estimates.

Interestingly, the coefficients on the log of aid are higher in the 2SLS results than the OLS results – for both countries. This suggests that that the OLS estimates were downwardly biased, which is curious. Given that the OLS estimates plausibly suffer from the endogeneity issue of reverse-causality, we would intuitively expect the coefficients to be overstated. This intuition follows from the fact that the OLS estimates are also likely picking up the effect of transparency on the log of aid in addition to the effect of the log of aid on transparency. It is possible, however, that the OLS estimates are biased towards zero due to measurement error in the log of aid.

The results for Sweden starkly contrast with the U.S. and Japan. The OLS estimates in Table 2 suggest that the log of aid is statistically significant; however, the coefficient on aid pales in comparison to the magnitude of the coefficient on the IMF Program Participation control variable. Specifically, the coefficient on IMF Program Participation stands at .214, as compared to .081 on the log of aid. Interestingly, the statistically significant relationship between aid and transparency in the OLS estimates falls out in the IV results. Neither our variable of interest nor the set of controls are statistically significant as per Table 3. In the case of Sweden, the results suggest that the volume of Swedish foreign aid commitments do not explain variations in the transparency of recipient countries.

6.3 Why the difference in OLS and IV estimates?

Many other studies that tackle the endogeneity of foreign aid find that aid is likely to be measured with error and note that this is consistent with the observation that the 2SLS estimates are larger than OLS estimates (Galiani et al., 2017). There are several reasons why this might be the case. Not all donors report their aid to the Development Assistance Committee (DAC) in all years. Historically, aid from the former Soviet Union and from China under Mao to other Communist countries was not reported to the DAC. Aid from China and other major and emerging donors has increased in recent years but still suffers from a lack of universal reporting. With this form of classical measurement error, the OLS estimate of the effect of aid is biased towards zero, which might explain the difference in our results from OLS to IV.

A non-measurement error explanation in the differences in magnitudes of the point estimates may lie in the realm of country-level motivations. Our OLS estimates may be downwardly biased by the fact that the United States, for example, targets non-transparent countries. This, in turn, could have the effect of further justifying additional allocations to foreign aid in the budgetary process.

Donors’ impulse to produce observable positive outputs stemming from their aid commitments is well-documented. Easterly (2002) notes that donors feel this pressure more than ever in recent years with the increased success of non-
governmental organizations (NGOs) in popularizing particular causes of progress in the developed-country media. Conversely, NGOs will often blame donors for aid projects that produce visible damage or cause politically sensitive resettlement. A salient example of this was when a World Bank aid project caused the resettlement of poor Han farmers in a location considered to be a Tibetan culture area (Wade, 2001). Broadly speaking, aid organizations are finding that fewer donor dollars are flowing towards aid projects due to stingier selection criteria and higher thresholds for demonstrating positive impact. The need to show positive results is vital in order to refill increasingly thin allocations to aid in government budgets. Therefore, this might explain why large donors like the U.S. would select non-transparent locations where it might be easier to yield a positive change from aid. Further analysis would need to be conducted in order to test this claim.

Table 2. OLS Regression Results

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>Transparency</th>
<th>United States Aid</th>
<th>Japanese Aid</th>
<th>Swedish Aid</th>
</tr>
</thead>
<tbody>
<tr>
<td>Log of Foreign Aid</td>
<td>0.220***</td>
<td>(0.021)</td>
<td>0.250***</td>
<td>(0.024)</td>
</tr>
<tr>
<td>Democracy</td>
<td>0.033***</td>
<td>(0.005)</td>
<td>0.041***</td>
<td>(0.005)</td>
</tr>
<tr>
<td>IMF Program</td>
<td>-0.164**</td>
<td>(0.068)</td>
<td>0.153**</td>
<td>(0.066)</td>
</tr>
<tr>
<td>Participation</td>
<td>0.001***</td>
<td>(0.000)</td>
<td>0.001***</td>
<td>(0.000)</td>
</tr>
<tr>
<td>GDP per capita</td>
<td>-3.802***</td>
<td>(0.356)</td>
<td>-3.833***</td>
<td>(0.355)</td>
</tr>
<tr>
<td>Constant</td>
<td>985</td>
<td>978</td>
<td>739</td>
<td>739</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.144</td>
<td>0.213</td>
<td>0.284</td>
<td>0.335</td>
</tr>
</tbody>
</table>

Table 3. 2SLS Regression Results

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>Transparency</th>
<th>United States Aid</th>
<th>Japanese Aid</th>
<th>Swedish Aid</th>
</tr>
</thead>
<tbody>
<tr>
<td>Log of Foreign Aid</td>
<td>1.076***</td>
<td>(0.236)</td>
<td>1.052***</td>
<td>(0.272)</td>
</tr>
<tr>
<td>Democracy</td>
<td>-0.020</td>
<td>(0.020)</td>
<td>0.020**</td>
<td>(0.009)</td>
</tr>
<tr>
<td>IMF Program</td>
<td>0.161*</td>
<td>(0.091)</td>
<td>0.154**</td>
<td>(0.068)</td>
</tr>
<tr>
<td>Participation</td>
<td>0.001***</td>
<td>(0.000)</td>
<td>0.001***</td>
<td>(0.000)</td>
</tr>
<tr>
<td>GDP per capita</td>
<td>-18.802***</td>
<td>(4.126)</td>
<td>-18.324***</td>
<td>(4.744)</td>
</tr>
<tr>
<td>Constant</td>
<td>985</td>
<td>978</td>
<td>739</td>
<td>739</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.300</td>
<td>0.327</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

*Coefficients denote change in HRV Index score associated with unit change in the log of aid

*Coefficients denote change in HRV Index score associated with unit change in log of aid
7. Conclusion

I present novel evidence on the effect of foreign aid on recipient countries’ transparency. I start by looking at the aid-transparency relationship in three of the six top donors globally: the United States, Japan, and Sweden. Our OLS regression results suggest a positive association between aid and transparency for all three countries. I address the extant endogeneity of foreign aid by utilizing a novel instrument for aid first introduced by Galiani et al. (2017). This instrument is derived from an exogenously-determined quasi-experiment in aid policy, exploiting the substantial drop in aid after a country crosses an exogenous income threshold set by the World Bank for IDA eligibility. Using a sample of 35 countries that have crossed the IDA threshold since 1987, I find that rapid reductions in aid subsequent to crossing the IDA threshold have a sizable and statistically significant negative effect on transparency.

The results for the U.S. and Japan confirm my hypothesis that greater foreign aid commitments correspond with greater transparency. Potential drivers of this relationship include what Tavares (2003) terms the ‘conditionality effect’ as well as a potential ‘liquidity effect’. In the case of the U.S. and Japan, it is possible that strings attached to their aid disbursements (and the potential for penalties if stipulations are not followed) are resulting in institutional improvements in recipient countries, with an eye towards encouraging transparency and greater accountability. It is also plausible that aid disbursements provide greater ‘liquidity’ by alleviating public revenue shortages and increasing public employee salaries, thereby reducing the incentive for corrupt or rent-seeking behavior. The combined effect of both channels could manifest in greater public accountability and transparency.

The lack of results in the Swedish case is interesting. It is also worth noting that the initial association between aid and transparency in Sweden’s case was significant although weaker in magnitude when compared to the U.S. and Japan. Regardless, the IV regression results suggest that no real relationship exists between Swedish aid commitments and transparency. The differing results between the U.S., Japan, and Sweden are interesting given the relatively equal prominence that all 3 countries share in the global aid community. This, perhaps, suggests that there is something systematically different about Sweden’s aid program.

The results generated from the foregoing analysis confirm my expectation based on the earlier review of the varying philosophies that govern each country’s foreign aid program. In the case of the U.S. and Japan, there is evidence to suggest that both countries are concerned with using aid as a tool to engender transformations in the institutional and political frameworks of recipient countries. In the U.S. case, it stems from a desire to use aid as a means to enable recipient country self-sufficiency. In the Japanese case, it is related to the country’s foreign policy ethos of position itself as an active, interventionist power-broker of global affairs. The insignificance of the aid-transparency relationship in the Swedish case is unsurprising given the evidence (or lack thereof) of Swedish interest in using aid as a mechanism for influencing governance conditions in recipient countries. Instead, Sweden is primarily concerned with alleviating inequalities in the global allocation of wealth and using aid primarily as a means to improve the welfare of its recipients. There is no conclusive evidence that Sweden is concerned with governance conditions to the same extent as the U.S. and Japan, and the empirical evidence confirms this expectation.

Causal inference in the case of foreign aid is a daunting task due to the endogeneity of aid policy and significant potential for unobserved heterogeneity to complicate identification. In that sense, my estimates should be interpreted cautiously. However, as Galiani et al. (2017) note, researchers should still evaluate the impact and effectiveness of aid projects on a case-by-case basis. The group that crosses the IDA threshold over the period in question comprises a set of poor and financially constrained countries that receive large amounts of aid (Galiani et al., 2017). In that sense, our evidence shows that foreign aid commitments from major donors increase transparency among poor countries where aid is a large source of funding. With that said, aid may have heterogeneous effects depending on the characteristics of recipients, aid modalities, and motives of donors (Mekasha and Tarp, 2013). Aid provided by some bilateral institutions for political or commercial reasons may be less effective (Dreher et al., 2016) and may be less sensitive to crossing the IDA income threshold.

Future studies of aid and institutional quality should look to confirm the results presented in this paper by exploring alternative measures of transparency. Further work can be done to develop the broadest, most universal measure of
transparency that incorporates a multi-factor perspective on the determinants of transparency. With that said, the measure of transparency utilized in this paper is a novel and innovative index that should provide a strong foundation for future empirical studies on the determinants of transparency.
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Center for Global Development Working Paper 300, Washington, D.C.
Appendices

Table A1. Descriptive Statistics for HRV Index (Dependent Variable)

<table>
<thead>
<tr>
<th>HRV Index</th>
<th>Observations</th>
<th>Mean</th>
<th>Standard Deviation</th>
</tr>
</thead>
<tbody>
<tr>
<td>All Values</td>
<td>2,502</td>
<td>0.794</td>
<td>1.744</td>
</tr>
<tr>
<td>HRV [-10, 0]</td>
<td>1,306</td>
<td>-0.724</td>
<td>0.643</td>
</tr>
<tr>
<td>HRV (0, 10]</td>
<td>1,466</td>
<td>1.866</td>
<td>1.456</td>
</tr>
</tbody>
</table>

Table A2. Descriptive Statistics for the log of U.S., Japan, and Swedish aid

<table>
<thead>
<tr>
<th>Country</th>
<th>Observations</th>
<th>Mean</th>
<th>Standard Deviation</th>
</tr>
</thead>
<tbody>
<tr>
<td>United States</td>
<td>985</td>
<td>17.523</td>
<td>1.809</td>
</tr>
<tr>
<td>Japan</td>
<td>983</td>
<td>17.231</td>
<td>2.126</td>
</tr>
<tr>
<td>Sweden</td>
<td>777</td>
<td>15.619</td>
<td>2.294</td>
</tr>
</tbody>
</table>

Table A3. First-Stage Results

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>United States</th>
<th>Japan</th>
<th>Sweden</th>
</tr>
</thead>
<tbody>
<tr>
<td>threshold</td>
<td>1.101***</td>
<td>1.587***</td>
<td>-0.116</td>
</tr>
<tr>
<td></td>
<td>(0.195)</td>
<td>(0.231)</td>
<td>(0.303)</td>
</tr>
<tr>
<td>democracy</td>
<td>0.039***</td>
<td>0.020</td>
<td>-0.030</td>
</tr>
<tr>
<td></td>
<td>(0.011)</td>
<td>(0.013)</td>
<td>(0.020)</td>
</tr>
<tr>
<td>imf</td>
<td>0.157</td>
<td>0.455**</td>
<td>-0.050</td>
</tr>
<tr>
<td></td>
<td>(0.144)</td>
<td>(0.195)</td>
<td>(0.221)</td>
</tr>
<tr>
<td>gdp_pc</td>
<td>-0.000</td>
<td>-0.001</td>
<td>-0.001**</td>
</tr>
<tr>
<td></td>
<td>(0.000)</td>
<td>(0.000)</td>
<td>(0.000)</td>
</tr>
<tr>
<td>Constant</td>
<td>17.080***</td>
<td>16.717***</td>
<td>16.126***</td>
</tr>
<tr>
<td></td>
<td>(0.164)</td>
<td>(0.261)</td>
<td>(0.241)</td>
</tr>
<tr>
<td>Observations</td>
<td>739</td>
<td>738</td>
<td>546</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.084</td>
<td>0.085</td>
<td>0.025</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Raspberries vs Wheat: Economic Sophistication as a New Predictor of Income Volatility

FILIP DRAZDOU AND DARYA LABOK
Stockholm School of Economics in Riga

ABSTRACT
In a series of recent papers, Hidalgo and Hausmann introduce a novel approach to measure the nature of production capabilities and knowledge accumulated in a country over time. With the development of the Economic Complexity Index (ECI), the authors relate economic complexity to fundamental macroeconomic variables. In this paper, we study the ECI as a possible determinant of GDP growth volatility, developing hypotheses about several channels through which the effect of a higher sophistication of production can be transmitted into a lower output volatility. We decompose the index into its two constituent parts – export basket diversification and the ubiquity of the products in this basket – to test the effect of each component separately. In our panel regression with time-fixed and country-fixed effects, we model a 5-year volatility period in a sample of OECD countries over 1995-2015 as a function of ECI and export diversity and ubiquity, combined with a set of control variables. In addition, we perform several robustness checks to affirm our main finding: the negative effect of ECI mainly stems from its ubiquity component, while the diversification part lacks any significance. We demonstrate that while the absolute effect of ubiquity on volatility is relatively high, ubiquity itself is a slowly changing parameter, given its path dependence and countries’ resilience to developing productive structures vertically. Our conclusions provide a new rationale for the implementation of industrial policy, a subject that still involves polar opinions and contrasting views between scholars and policy makers.

1 We would like to express our gratitude to our supervisor – Konstantins Benkovskis, whose valuable suggestions helped us throughout the work on this paper, and whose continuous support was extremely beneficial.
1. Introduction

Think of an example of two countries, each of them producing and exporting either raspberries or wheat, the former being a petit-bourgeoisie crop, and the latter a proletarian one (Scott, 1998). What does it imply for these two countries, apart from their comparative advantage in production of either crop? Intuitively, one can say that it is more difficult to produce one ton of raspberries rather than the same amount of wheat: the farmers cultivating raspberries need a certain type of soil and use hand-picking in harvesting, putting up with the fragile nature of a crop that demands careful transportation and special storage conditions. As Scott (1998, p. 222) points out, “successful raspberry growing requires a substantial stock of local knowledge and experience,” highlighting the idea that, in fact, the types of goods produced in a country determine its economic and political structures.

If one decides to decompose a product into its separate inputs, in addition to the raw materials used, they should also include highly product-specific knowledge and know-how, the human capital and physical assets utilized, the type of a legal system, and institutions and infrastructure development. These inputs are quite specific to every product, and once a country chooses to specialize in a good, it starts to acquire these inputs by developing a set of necessary capabilities and establishing a corresponding productive structure.

However, the process of specialization is not an easy one: production capabilities, skills and competencies differ and are not perfect substitutes for each other, meaning that a set of accumulated capabilities in a wheat-based economy will hardly facilitate the development of a knowledge-intensive pharmaceutical industry. In this respect, countries will rather find themselves moving into manufacturing of goods requiring already available capabilities, and leapfrogging them is very unlikely.

Estimating the economic activity of a country and using the measure of GDP for this purpose can only deliver a sense of its size, and tells little about the complexity of production structures. On the contrary, it seems to be infeasible to distinguish between every product or service manufactured in the economy; even if we try, there is a risk of getting a biased estimate. We need an expert’s assessment of a product’s sophistication level, which is still a subjective estimation of a particular individual.

Recently, two prominent scholars – Hidalgo and Hausmann (2009) – put forward a full-fledged theory of economic complexity, developing a novel approach for ranking each product based on the variety of capabilities embedded into them. The authors view economic development as the ability to produce and export more products as well as the ability to produce and export products of a higher degree of sophistication, thus relating accumulated capabilities to overall economic development. As different products require different skills, they indirectly infer the number of locally available capabilities by looking at what countries produce and export in terms of diversification (how many various products are exported) and ubiquity (how complex the products are). As a result, a country’s export matrix reveals a set of accumulated knowledge and capabilities accrued from the nature of production activities.

Consequently, by employing a novel technique for estimating a valuable characteristic of a country, a new measure is derived. The development of the Economic Complexity Index (ECI), capturing the nature of productive capabilities accumulated in a country, stimulates further research into its relations with fundamental macroeconomic variables. In our paper, we extend the view on the effects of economic complexity in relation to key macroeconomic variables, namely, by measuring its impact on GDP growth volatility in a sample of developed countries. Business cycle volatility is closely linked to the stability of GDP growth, while the primary objective of counter-cyclical policies is to diminish its persistent negative effect on economic productivity.

We set our study focus on further research into the determinants of GDP growth volatility. Specifically, we argue that the countries which produce less ubiquitous products experience less output volatility. The idea behind the argument is that more complex products require more inputs, have more human capital embedded into their production, and enjoy more demand from rich countries. As a result, higher levels of economic complexity smooth the effect of negative exogenous shocks and makes a country less vulnerable to any market fluctuations.
The work is structured as follows: the next section reviews the existing research on theoretical growth models as well as the ambiguity of the volatility effect on economic growth and income volatility determinants. Section 3 describes the methodology of the research, and in Section 4, we present the empirical results and robustness checks in conjunction with discussion paragraphs. Section 5 concludes.

2. Literature Review
2.1 Growth, Volatility and its Determinants: Theory and Empirical Findings
Economic growth has been always on the agenda of academics and policy makers. In a pursuit of growth stability, high volatility has been commonly associated with amplified economic risks and vulnerability. As a part of this, some researchers relate high income volatility to persistent unfavorable effects on a long-run growth trend (Ramey & Ramey, 1994; Aizenman & Pinto, 2005). Particularly, this is of relevance for OECD members, where scholars did not find any contradictions within the concept of a negative relationship between long-term growth and volatility (Ramey & Ramey, 1994; Rodrik, 1997).

Several decades ago, business-cycle theories and economic development theories were separate. The studies distinguished between the cyclical and non-cyclical components of domestic income, interpreting any changes in the economy as irregular co-movements around an independent long-term growth trend. In some cases, those fluctuations were characterized by their non-cyclicality, thus generating little support for the in-depth analysis of cyclical fluctuations. This dichotomy perspective changed with the development of a real business cycle theory, when the theory of cyclical movements was incorporated into the analysis of economic fluctuations by several scholars (Nelson & Plosser, 1982; Kydland & Prescott, 1982; King, Plosser, & Rebelo, 1988; Mankiw, 1989).

One of the prominent papers in the field, written by Garey Ramey and Valerie Ramey (1994), presented the empirical evidence of the existence of a strong negative link between volatility and growth. As the authors argue, this effect primarily stems from the volatility of technological innovations that reflect the degree of uncertainty in the economy. The contribution of Ramey and Ramey (1994) spurred academic interest once the explanatory power of income volatility to GDP growth was proved to be empirically robust.

The point that is worth additional attention is the type of the growth model itself: in fact, this is what prescribes the sign in the volatility-growth relationship. According to several scholars (Aghion & Saint-Paul, 1998; Martin & Rogers, 2000), the theory-based explanations of the effect of income volatility on economic growth are dependent on how we treat the very mechanism of macroeconomic growth origination. In other words, the choice of the theoretical model of economic growth development matters.

The positive correlation of growth and volatility was supported by several economists within various time periods, with the first theoretical argument coming from Schumpeter’s (1939, 1942) idea of “creative destruction”. The second strand of academic studies incorporates the endogenous models of economic growth, highlighting the existence of a negative correlation between growth and volatility. These types of models link productivity enhancements to the improvements in internal country’s processes, with a particular focus on a human capital component.

As Aizenman and Pinto (2005) suggest, “the so-called deep determinants” of output volatility – trade openness, geography, political institutions and financial market development might either magnify or alleviate the effect of volatility on GDP growth (p. 2). Academic literature on the topic considers various factors in the analysis of income volatility: trade-related mechanisms, individual characteristics of the countries such as their geographic locations, the current stage of economic development, the economy’s size, and institutional and financial system determinants.

What we have shown so far is that there is an ambiguous, yet in most cases present, effect of business cycle volatility on income growth. In the meantime, the absence of any consensus on this topic requires additional attention. A better understanding of its underpinnings plays a crucial role in the choice and design of counter cyclical policies, developed to minimize the detrimental effects of external fluctuations in the economy. The other part of the story is about addressing
the existing contradicting views by studying output volatility itself. Understanding what affects business cycle volatility and its variation in different countries provides another perspective on the relationship between income growth and volatility. Not only do well-tailored counter cyclical policies, but also mechanisms for decreasing volatility magnitude play a crucial role in this relationship.

2.2 The Theoretical Channels behind Complexity and Volatility
Many researchers have noted that poor countries experience higher macroeconomic volatility (Lucas, 1988; Koren & Teneyro, 2007), and a number of explanations for that phenomenon focus on the complexity of countries’ products. First, there is a wide body of literature relating the low volatility of complex products to their resilience to external shocks. Krishna and Levchenko (2013) model sectoral volatility as the function of a sector’s complexity (measured by the number of intermediate inputs), and find that, as a variety of inputs used in the production process of a specific good increases, a single input-specific shock is increasingly unable to significantly alter the output.

Maggioni, Lo Turco, and Gallegati (2016) argue that a production comprised of many inputs is also characterized by skilled labour and more sophisticated technology, so, considering only the number of inputs is misleading. They use both the quantity of imbedded inputs and human capital intensity of products as the variables for explaining firm-level output fluctuations. The findings show that the effect of complexity on volatility largely represents the effect of a larger share of human capital contained in complex products – thus validating Hidalgo and Hausmann's theory on a micro-level.

Another branch of research emphasizes a lower elasticity of substitution as the main reason behind the low volatility of complex products. Kraay and Ventura (2007), for example, argue that industries with skilled workers and considerable technological knowledge face less elastic demand, thus experiencing a smoothing effect from business cycle movements. Moreover, the demand for more complicated products largely comes from rich countries importing more from countries specializing in high quality products (Hallak, 2006). Consequently, the more complex a product is, the less market volatility it experiences, since it receives benefits from its sophisticated nature and wealthy consumers.

In sum, we see that the theory provides us with several possible hypotheses on how a product’s complexity can affect the volatility of growth. While the theoretical discussion gives us an overall perception of the possible economic mechanisms lying behind the volatility – growth relation, in the next section, we introduce our methodology framework to familiarize a reader with our quantitative econometric model underpinning further results analysis.

3 Methodology
3.1 Data
In our paper, we use two major types of data: (a) international trade flows data, used for calculations of diversification, ubiquity and the Economic Complexity Index (ECI); and (b) macroeconomic data used for control variables.
### Table 1. Description of the dataset

<table>
<thead>
<tr>
<th>Variable name</th>
<th>Definition</th>
<th>Source</th>
</tr>
</thead>
<tbody>
<tr>
<td>VOL</td>
<td>Standard deviation of annual GDP growth rate</td>
<td>World Bank</td>
</tr>
<tr>
<td>GPC</td>
<td>GDP per capita in real terms</td>
<td>World Bank</td>
</tr>
<tr>
<td>TOT</td>
<td>Terms of trade volatility</td>
<td>OECD</td>
</tr>
<tr>
<td>OPEN</td>
<td>Total trade as a share of GDP</td>
<td>World Bank</td>
</tr>
<tr>
<td>SPEN</td>
<td>General government spending as a share of GDP</td>
<td>World Bank</td>
</tr>
<tr>
<td>CRED</td>
<td>Credit to the private sector as a share of GDP</td>
<td>Eurostat, BIS</td>
</tr>
<tr>
<td>VA</td>
<td>Voice and accountability</td>
<td>World Bank</td>
</tr>
<tr>
<td>PV</td>
<td>Political stability and violence</td>
<td>World Bank</td>
</tr>
<tr>
<td>GE</td>
<td>Government effectiveness</td>
<td>World Bank</td>
</tr>
<tr>
<td>RQ</td>
<td>Regulatory quality</td>
<td>World Bank</td>
</tr>
<tr>
<td>RL</td>
<td>Rule of law</td>
<td>World Bank</td>
</tr>
<tr>
<td>CC</td>
<td>Corruption control</td>
<td>World Bank</td>
</tr>
<tr>
<td>DIV</td>
<td>Diversification</td>
<td>Own calculations</td>
</tr>
<tr>
<td>UBI</td>
<td>Ubiquity</td>
<td>Own calculations</td>
</tr>
<tr>
<td>ECI</td>
<td>Economic Complexity Index</td>
<td>Atlas of Ec. Complexity</td>
</tr>
</tbody>
</table>

We choose 35 current members of OECD for our sample due to the better quality of data available for these countries.

### 3.2 Revealed Comparative Advantage

In assessing the complexity of a country’s economy, we use export data. However, countries export a wide range of products, exporting some in very small quantities. That is why we must distinguish ancillary exports from the exports that are truly representative of a country’s capabilities. Thus, we use the concept of Revealed Comparative Advantage (RCA), similarly to Hidalgo and Hausmann (2009) and other research.

Revealed Comparative Advantage measures if a country’s share in the export of a good is larger than the market share of the same good in the world market. Mathematically, it is represented as

\[
RCA_{cp} = \frac{S_{cp}}{T_p}
\]

where \( S_{cp} \) is a country share \( c \) of exports of a product \( p \) in export basket, while \( T_p \) is the trade share of a product \( p \) in the world market.

By definition, the country has Revealed Comparative Advantage in a product when \( RCA \geq 1 \).

### 3.3 Method of Reflections

To calculate diversification, ubiquity, and eventually ECI, we employ the Method of Reflections developed by Hidalgo and Hausmann (2009). Although the method is useful for analyzing both countries and products, in our study we focus on country-related metrics.

First, the diversification of a country is calculated as the number of products exported by a country with RCA and the ubiquity of a product as the number of countries exporting the product with RCA. The country which exports many Diversification and ubiquity are calculated as follows:
\[ k_{c,0} = \sum_{p} M_{cp} \text{ (Diversification of a country)}, \]

\[ k_{p,0} = \sum_{c} M_{cp} \text{ (Ubiquity of a product)}, \]

where \( c \) is a country, and \( p \) is a product.

Based on the first two measures, the Method of Reflections goes on to generate the higher-order elements iteratively, adding the information from the previous reflections. The formulas for the \( N \)-th reflection are given by:

\[ k_{c,N} = \frac{1}{k_{c,0}} \sum_{p} M_{cp} k_{p,N-1}, \]

\[ k_{p,N} = \frac{1}{k_{p,0}} \sum_{c} M_{cp} k_{c,N-1}. \]

The interpretation of values generated in the first three iterations is provided in Table 2.

**Table 2. Interpretation of the Method of Reflections’ first three iterations**

<table>
<thead>
<tr>
<th>Definition</th>
<th>Description</th>
</tr>
</thead>
<tbody>
<tr>
<td>( k_{c,0} )</td>
<td>Diversification – number of products exported by country ( c )</td>
</tr>
<tr>
<td>( k_{p,0} )</td>
<td>Ubiquity – number of countries exporting product ( p )</td>
</tr>
<tr>
<td>( k_{c,1} )</td>
<td>Average ubiquity of products exported by country ( c )</td>
</tr>
<tr>
<td>( k_{p,1} )</td>
<td>Average diversification of countries exporting product ( p )</td>
</tr>
<tr>
<td>( k_{c,2} )</td>
<td>Average diversification of countries with an export basket similar to country ( c )</td>
</tr>
<tr>
<td>( k_{p,2} )</td>
<td>Average ubiquity of the products exported by countries that export product ( p )</td>
</tr>
</tbody>
</table>

**Source:** Supplementary material for “The Building Blocks of Economic Complexity”, Hidalgo & Hausmann (2009, p. 8).

### 3.4 ECI Index Calculations

The Economic Complexity Index for the countries in question is obtained by increasing the number of iterations until no any new information can be extracted (i.e., the relative positions of the countries in ECI ranking stay unchanged). For instance, in Hidalgo and Hausmann (2009), \( n=18 \) iterations are used to calculate ECI as a predictor of growth for a subset of countries.

In our study, we do not calculate ECI ourselves, but take ECI data from a dataset provided by the Observatory of Economic Complexity (2018).

### 3.5 Empirical Model

We estimate the effect of economic complexity on growth volatility using a panel regression with time- and country-fixed effects, with the robust errors adjusted for heteroscedasticity and autocorrelation.
We divide our 20-year sample period (1995-2015) into four 5-year subperiods and calculate the average values of the explanatory variables and the sample standard deviation of GDP growth rate.

For the regression, we standardize some variables (GPC, CRED, SPEN, OPEN, UBI). We do not perform any transformations with ECI and institutional variables as they are already transformed in the raw dataset. The diversification measure – number of products – stays untransformed as well.

We estimate the following equation:

$$ GDP_{vol_{it}} = \beta_0 + \beta_1 Diversification_{i,t} + \beta_2 Ubiquity_{i,t} + \beta_3 ECI_{it} + X_{it} Controls_{it} + \mu_i + \eta_t + u_{it} $$

We test different specifications of the model, adding and excluding controls and interactions depending on their significance. We test the robustness of the coefficients to the minor changes in particular specifications of the model.

4. Analysis of Results
4.1 Baseline Results

Our baseline results are the country- and time- fixed effects panel regressions, in which we separate ECI from its two structural components – diversity and ubiquity.

The baseline results are reported in Table 3.

<table>
<thead>
<tr>
<th>Variable</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>ECI</td>
<td>-3.20***</td>
<td>-3.18***</td>
<td>0.0012</td>
<td>0.0024</td>
</tr>
<tr>
<td>DIV</td>
<td>-0.93*</td>
<td>-0.59</td>
<td>-0.82</td>
<td>-0.38</td>
</tr>
<tr>
<td>UBI</td>
<td>-16.95***</td>
<td>-17.32***</td>
<td>-15.18***</td>
<td>-15.43***</td>
</tr>
<tr>
<td>GPC</td>
<td>1.10***</td>
<td>1.10***</td>
<td>1.06**</td>
<td>1.09**</td>
</tr>
<tr>
<td>OPEN</td>
<td>1.60***</td>
<td>1.50***</td>
<td>1.35***</td>
<td>1.16***</td>
</tr>
<tr>
<td>TOT</td>
<td>-0.06</td>
<td>0.00</td>
<td>0.19</td>
<td>-0.33</td>
</tr>
<tr>
<td>CRED</td>
<td>0.14</td>
<td>-1.19</td>
<td>-1.94</td>
<td>-0.98</td>
</tr>
<tr>
<td>SPEN</td>
<td>5.03***</td>
<td>7.37***</td>
<td>5.84***</td>
<td>7.16***</td>
</tr>
<tr>
<td>PV</td>
<td>33</td>
<td>33</td>
<td>33</td>
<td>33</td>
</tr>
<tr>
<td>RL</td>
<td>0.09</td>
<td>0.11</td>
<td>0.14</td>
<td>0.19</td>
</tr>
</tbody>
</table>

**Note:** The dependent variable is the volatility of real GDP growth. Significance at the 1%, 5% and 10% levels are denoted respectively by ***, ** and *.

When separated, both ubiquity and ECI are significant at 1% in all of the model specifications, with or without inclusion of institutional variables. Moreover, the coefficients have similar absolute values, meaning that a one standard deviation increase in ECI has approximately the same effect on growth volatility as a one standard deviation decrease in ubiquity (apx. 3.2-3.4pp).
Keeping in mind that, by construction of the ECI index, it is impossible to decrease ubiquity without increasing ECI, and considering the insignificance of the diversification variable, we can argue that it is, in fact, the ubiquity – and not the diversification – part of the ECI index which explains the volatility of growth. To affirm this claim, we will check the robustness of this result in the next few sections.

The interpretation of displayed estimates is clear: we have quite strong evidence that the economies producing less ubiquitous products experience more stability in their growth. This finding is robust to the inclusion of institutional and macroeconomic variables as controls, as well as in line with economic theory: the more sophisticated products are, the less exposed to outside shocks their manufacturers will be, so the economies specializing in them are more stable as well.

To understand the magnitude of the effect, let us give an example: a country which decreases its ubiquity by 1 standard deviation will experience a 3.4pp drop in growth volatility. To put this into perspective, a one standard deviation drop in ubiquity would mean changing a product basket from one similar to Slovakia’s to one similar to Germany’s. That is why even minor changes in a product basket may drastically affect volatility.

The control variables have coefficient signs consistent with predictions from economic theory.

In Figure 1, we see how the ECI for each country has changed over a period of more than 10 years. The countries located above the red line have increased their complexity, while those which are below decreased their complexity.

**Figure 1.** ECI development over time

4.2 Robustness Check: Different Measures of Diversification

Extensive previous research shows that export diversification is a significant determinant of income volatility (Jansen, 2004; Agosin, Alvarez & Ortega, 2012). While we find the effect of export diversification to be insignificant, we acknowledge that our measure is rather basic. We use the number of products exported with a comparative advantage as a proxy for diversification. By this measure, we omit information about products which may be sophisticated, but take up a share of their country’s exports that is less than, or similar to, the average share in the global market.
To check the robustness of our results, we obtain three different measures of diversification from UNCTAD: the number of products exported (DIV1), concentration index (DIV2), and diversification index (DIV3). The data is available for all the OECD countries, so the number of observations stays unchanged.

<table>
<thead>
<tr>
<th>Variable</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>UBI</td>
<td>3.39***</td>
<td>3.25**</td>
<td>3.34**</td>
<td>3.38**</td>
</tr>
<tr>
<td>Diversification</td>
<td>0.0024</td>
<td>-0.034</td>
<td>2.64</td>
<td>0.38</td>
</tr>
<tr>
<td>DIV</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>DIV1</td>
<td>-0.034</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>DIV2</td>
<td></td>
<td></td>
<td>2.64</td>
<td></td>
</tr>
<tr>
<td>DIV3</td>
<td></td>
<td></td>
<td>0.38</td>
<td></td>
</tr>
<tr>
<td>Controls</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>GPC</td>
<td>-0.38</td>
<td>-0.48</td>
<td>-0.59</td>
<td>-0.48</td>
</tr>
<tr>
<td>OPEN</td>
<td>1.09**</td>
<td>1.02**</td>
<td>1.06***</td>
<td></td>
</tr>
<tr>
<td>TOT</td>
<td>-15.43***</td>
<td>-15.44***</td>
<td>-16.70***</td>
<td>-15.81***</td>
</tr>
<tr>
<td>CRED</td>
<td>1.26***</td>
<td>1.45***</td>
<td>1.30***</td>
<td>1.26***</td>
</tr>
<tr>
<td>SPEN</td>
<td>0.13</td>
<td>-0.10</td>
<td>0.18</td>
<td>0.12</td>
</tr>
<tr>
<td>PV</td>
<td>-0.33</td>
<td>-0.41</td>
<td>-0.29</td>
<td>-0.33</td>
</tr>
<tr>
<td>RL</td>
<td>-0.98</td>
<td>-0.72</td>
<td>-0.72</td>
<td>-0.89</td>
</tr>
<tr>
<td>Constant</td>
<td>7.16***</td>
<td>15.59***</td>
<td>6.30***</td>
<td>7.37**</td>
</tr>
<tr>
<td>Observations</td>
<td>127</td>
<td>127</td>
<td>127</td>
<td>127</td>
</tr>
<tr>
<td>Countries</td>
<td>33</td>
<td>33</td>
<td>33</td>
<td>33</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.21</td>
<td>0.21</td>
<td>0.21</td>
<td>0.21</td>
</tr>
</tbody>
</table>

Note: Dependent variable is the volatility of a real GDP growth. Significance at the 1%, 5% and 10% levels are denoted respectively by ***, ** and *.

We find that in the baseline regressions, all three measures are insignificant and fail to alter the coefficients of the other control variables, the openness and private credit variables are still significant, and the numerical effect of ubiquity stays around 3pp within 4 model specifications.

We also perform a number of other robustness checks (not reported), by taking different trade data, adding squared terms or new control variables. None of those contradicted the baseline results.

5 Conclusions

Recent developments in international trade provided a novel method for a quantitative estimation of production capabilities accumulated in a country. In light of a newly developed index of economic complexity, the role of these capabilities has already showed its great importance in relation to GDP and income inequality. Given the significant persistent impact that business cycle volatility has on long-term growth trends, a question to ask is whether available set of capabilities a country possesses affect its income volatility and make it less exposed to exogenous shocks. This paper looks at the determinants of income volatility in OECD countries over the last 20 years and models 5-year volatility through two specifications: as a function of a) the Economic Complexity Index and b) export product diversity and ubiquity, in conjunction with a set of control variables.
The main finding of the paper relates to one of the two integral components of ECI: the ubiquity of a country’s products. We find that economic complexity has a significant negative effect on output volatility, and are also able to conclude that this effect is attributable to the ubiquity part of the index. Thus, diversification – previously considered to be a good predictor – is irrelevant in our research. Our findings are robust to different model variations and alternative choices of trade data. We argue that a set of capabilities a country has, measured by its complexity index, affects income volatility. Our findings are relevant to countries seeking economic stability and can guide industrial policy in a country. While this is largely out of the scope of our study, we still provide some discussion on the effectiveness of government support of complex industries. At the same time, we acknowledge that our main limitation is the homogenous sample of OECD countries. Research incorporating developing countries would be better suited for a more comprehensive discussion on policy implications.

Another branch of research could employ an action-oriented approach. While there is plenty of evidence by now that economic complexity matters, which specific structural reforms are the most efficient in growing the number of capabilities is still an open question.

Finally, the limitations of the methodology, mainly concerning global value chains and the services sector, should be noted. In this respect, a more accurate proxy for the economy’s production structure, replacing the currently used exports figure, could be highly relevant.
References
Does Agglomeration Account for Process Innovation in Vietnamese Small and Medium Enterprises?

VAN ANH LE

Macalester College

ABSTRACT

Although small and medium enterprises (SMEs) play a crucial role in the Vietnamese economy, this sector’s growth is hindered by low levels of technology and innovation. This paper uses firm-level panel data to examine whether process innovation activities in SMEs are influenced by their industrial environments. It measures the effects that agglomeration (the geographic concentration of firms within the same locality) has on firms’ total outputs and their propensity to introduce new technology. Using a logistic model with firm fixed-effects, I find that agglomeration decreases outputs of informal firms and the likelihood of new technology introduction in all firms. However, there is evidence of positive lagged effects of agglomeration on innovation and heterogeneous effects across industries.

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Lobbying Preferences and Participation in International Environmental Agreements: A Renewed Evaluation

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ABSTRACT

Industrial and environmental lobbying define the political economy of participation in international environmental agreements. While the relationships are widely understood and studied, they have not been examined over an extended period of time. And even when these relationships are examined, the number of agreements in the sample size remains small. Employing 247 unique multilateral environmental agreements and 638 unique bilateral environmental agreements ranging from 1996 to 2016, I hypothesize that industrial lobbies and environmental lobbies have opposite preferences for participation in international environmental agreements, with industries opposing participation and environmental groups supporting participation. These effects are amplified by corruption as well as the freedom of the press, respectively. Using panel data of 3,325 observations covering 194 countries, I find evidence supporting the observation that industrial lobbying increases the likelihood of participation, and inconclusive evidence on corresponding environmental lobbying. These results are robust to several sensitivity checks. I conclude that though industries may have traditionally preferred not to support international environmental agreements at the turn of the 21st century, that preference has shifted to a more positive and inclusive attitude in recent years.

1 The author extends his gratitude to Dr. Marko Klasnja, Dr. Charles Udomsaph, Dr. Rodney Ludema, Gloria Kim, Grady Killeen, Duncan Hobbs, members of his IPEC 401 discussion group, and Carroll Round XVII participants for feedback. All remaining errors are my own.
Determinants of Diversification: A Study of Ecuadorian Exports

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ABSTRACT

Export diversification enjoys wide support as a policy recommendation for Ecuadorian economic development, praised both by international policy institutions as well as Ecuador’s own government. Nevertheless, a gap remains in understanding what works to encourage that diversification specifically in Ecuador, as evidenced by two divergent Ecuadorian political movements that both claim diversification as a goal. This report takes humble steps toward a better understanding of the determinants of Ecuadorian export diversification. A large dataset is constructed describing all real and possible Ecuadorian export trade flows to the world’s 50 largest GDPs at the level of six digits in the HS coding system between 1991 and 2015. Using a gravity model of trade, an initial Probit estimation is used to test the determinants of market entry for Ecuadorian firms, and then those results are incorporated via the Heckman method through their inverse Mills ratios into an OLS estimation of what drives greater export trade value. Next, a novel approach is used at both stages of the Heckman method to measure diversification along its extensive and intensive margins. Key results include that free trade agreements and measures of macroeconomic stability are consistently associated with greater diversification along the extensive and intensive margins, while the revolución ciudadana and broader policies of President Rafael Correa are associated with lesser diversification along both. The report ends with a more micro-oriented perspective afforded by twelve interviews conducted with community leaders and Ecuadorian businesses.

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1 The author is an undergraduate student at the University of Minnesota, Twin Cities, pursuing a bachelor’s of science in economics, a bachelor’s of arts in mathematics, and a minor in Spanish. He would like to thank the Fundación CIMAS of Ecuador for its help in the development of this thesis, with special thanks to Emilia Castelo and Carlos Domenech, who gave excellent suggestions on the initial drafts. Dr. Julio Oleas Montalvo of the Instituto de Altos Estudios Nacionales (IAEN) and Dr. Thomas Holmes of the University of Minnesota graciously served as mentors for the duration of this project. The investigations were supported by a grant from the International Undergraduate Research Opportunities Program (IUROP) of the University of Minnesota.
1. Introduction
The contentious Ecuadorian elections in April 2017 were indicative of a broad political division between two near opposite visions of Ecuador’s economic development. Despite their differences, however, both the left and right seem to agree on one thing: The importance of diversifying Ecuador’s general economy, and its exports in particular.

Ecuadorian legislators are not the only ones who think that diversification would be advantageous to the country’s economic development. Economists’ standard advice over the last century, which presented free trade as a sort of panacea, has been recently questioned, and focus has shifted instead toward the specific paths along which trade can generate growth (Kali, Reyes, McGee, & Shirrell, 2013; Lederman & Maloney, 2003; Mejía, 2011). One of the paths to have risen from this literature is export diversification, that is, the movement of domestic companies into new export markets or the production of new products. International organizations, like the World Bank (Hesse, 2008), the International Monetary Fund (IMF) (International Monetary Fund, 2014), and the Food and Agriculture Organization (FAO) of the United Nations (The State of Agricultural Commodity Markets, 2004), have likewise united to the cause of recommending diversification among developing nations.

Even among developing nations, however, it seems that diversification is particularly important for Ecuador. Its economic history is marked by an excessive dependence on few products—first cacao (1860–1920), then bananas (1948–1966), and now petroleum (Gonzalez, 2010; Hanratty, 1991). Little has changed in the distribution of Ecuadorian exports since the 1970s, such that in 2010 a full 72% were composed of just five products: crude petroleum, bananas, fuel oils, shrimp, and flowers (Freire, 2012). As a result, many of the economy’s booms and busts throughout history can be explained by changing international prices of their key exports, with greater macroeconomic volatility visible whenever the economy was particularly dependent on a single product (Gonzalez, 2010; Rochlin, 2011). Burneo and Oleas (1996) found that this type of volatility is inimical for Ecuadorian growth. Moreover, despite a general upward trend worldwide in export diversification among developing countries (International Monetary Fund, 2014), the Herfindahl-Hirshman Index (HHI) shows that concentration in Ecuadorian exports actually rose during the first decade of the millennium, and more so than comparable Latin American countries (Freire, 2012).

This stagnant and at times even negative trend in Ecuadorian diversification is particularly troubling for two reasons beyond the government wanting a diversification that has not materialized. First, based upon its GDP per capita, large segments of the literature would predict that Ecuadorian exports should be diversifying each year more rapidly than the one before in order to achieve higher levels of GDP per capita (Klinger & Lederman, 2004; Lederman & Maloney, 2003). Second, a study completed by Lederman and Maloney (2003) showed that the counterintuitive idea of a “resource curse,” which has often been blamed for Ecuador’s poor economic condition, is not actually supported in the data. Rather, when controls for the concentration of exports are added to the models, resource abundance has the positive effect on the economic growth of a country that is to be expected. This implies that for Ecuador, which has vast natural resources (Freire, 2012), export concentration is greatly depressing possible growth.

1.1 Problem
It is a significant problem, then, that there are not clearly understood methods by which Ecuadorian policymakers can spur diversification. Across a growing body of literature on this subject, one of the conclusions to emerge is that no single plan for diversification can be applied to all developing countries (International Monetary Fund, 2014; Kali et al., 2013). This raises the necessity of specific studies for each country, but in the case of Ecuador and the specific topic of export diversification, there are few. The objective of this study is to identify those factors which most encourage the

2 For example, Agosin (2007), Al-Marhubi (2000), Hesse (2008), Lederman and Maloney (2003), and Mau (2016).
3 Of the literature on Ecuadorian economic development, this report found four studies that stand out for having given some mention of export diversification: Freire (2012), Freire, Salvador, and Katiuvshka (1997), Gonzalez (2010), and Han and Rhee (2012). The insights obtained through these studies are important and will be referenced throughout this present report. While their analyses focused on qualitative indicators and stylized facts, however, this report will be more rigorous in its treatment of econometric methods. By considering several variables in the same model, the results should be more resilient against biases in the data than those of preceding studies.
1.2 Methods
The quantitative portion will make use of a dataset recording real and possible Ecuadorian exports between 1991 and 2015. This period is divided into two political eras—neoliberal and interventionist—by the revolución ciudadana of 2007, permitting a comparative analysis of the policies in each. Using the Melitz (2003) estimation equation as a framework, a Probit model is used to estimate the probability that Ecuadorian industries enter certain global markets with certain products. Then, the results are added into an ordinary least squares (OLS) regression through the inverse Mills ratio by the Heckman method. The results will allow a calculation of the effects of variables along the extensive and intensive margins of diversification.

In the qualitative portion, the quota method is used to select 12 experts living in Ecuador for interview. Four people will be selected in each of three categories: community leaders, credit and savings cooperatives, and foreign business owners who entered the Ecuadorian economy through foreign direct investment (FDI). These interviews will be compared against the results of the macro level quantitative work, creating a fuller picture of diversification in Ecuador.

1.3 Results
The most important results of this report are that free trade agreements and measures of macroeconomic stability are consistently associated with greater diversification along the extensive and intensive margins, while the revolución ciudadana and broader policies of President Rafael Correa are associated with lesser diversification along both. Other effects that remain consistent across the intensive and extensive margins include a positive effect associated with the size of the receiving economy and negative effects associated with both the remoteness of the receiving economy and its distance from Ecuador. The method derived for measuring diversification is also significant in that it can be repeated for studies of diversification in other countries.

To conserve space in this abbreviated version of the paper, sections on relevant Ecuadorian economic history, a literature review, the community interviews, and discussion have been removed entirely. Insights from these sections nevertheless informed the quantitative results that will be presented here. Section two describes the econometric methodology that will be used for the results presented in section three.

2. Methodology
2.1 The Data
A dataset was constructed noting every Ecuadorian export completed to the 50 largest GDPs between 1991 and 2015 at the level of six digits in the HS code. Then, a trade flow of zero was added to the dataset for every possible trade flow that was not actually realized, where a possible trade flow was defined as the export of goods from any six-digit HS category $j$ to any of the 50 countries $d$ during any year $t$ between 1991 and 2015. Studies that ignore these zeros in the trade matrix ignore important information about positive trade flows and fall victim to sample selection bias (Helpman et al., 2008). The resulting dataset was next merged with several other datasets from different sources. Most important among these secondary data sources was the Gravity database prepared by Head et al. (2010), the GeoDist dataset from CEPII (Mayer & Zignago, 2011), and the World Bank's World Development Indicators (2017).

As is to be expected, this method resulted in a rather large dataset, with some 6,297,500 observations. In the portions of the upcoming models that deal directly with diversification, then, the dataset is limited to the top 20 GDPs in the world for computational simplicity. Three points are relevant to note in this decision. First, the top 20 GDPs in the world still account for 68.49% of Ecuadorian trade value realized in 2015, a representative year. Second, this reduction helps to limit the diversification measured by the models to that which is of a more significant kind. Increased international trade with countries already well developed is the only type of increased international trade that Vamvakidis (1998) found to be important for the growth of developing countries. Third, and finally, the reduced dataset remains very large, with some
2.519 million observations. Nevertheless, it will be important to remember in the results section that the conclusions are only directly applicable for trade with the 20 largest economies in the world by GDP.

Two of the variables included in the dataset are noteworthy for their potential to be used as controls against heteroskedasticity in the data. First, there must be a factor variable added to control for heteroskedasticity across product categories. Much of Ecuador’s potential for export diversification has to do with the placement of their current exports in the topological product space. It may be easier to diversify into new products related to excavation, for example, because of the country’s strong presence in the petroleum industry. To control for this type of variation, a factor is added that will be interpreted by the model as 15 dummy variables, each one representing a different group of products.

Second, it will be necessary to add a control against heteroskedasticity across economies receiving Ecuadorian exports, that is, to add multilateral resistance terms. The exclusion of these effects can create large biases in estimation results (Anderson & van Wincoop, 2003), such that their exclusion has been identified as a “gold medal” error (Baldwin & Taglioni, 2007). As this study only considers the exports of one country, Ecuador’s multilateral resistance term can be considered as part of the constant, and its specific value will be of little interest. Among countries receiving Ecuadorian exports, however, these terms are very relevant and must be included. This paper constructs such a “Remoteness” variable after the suggestion of Head and Mayer (2002) according to the formula:

\[ \text{Remoteness}_d = \sum_{m=1}^{M} \left( \frac{Y_m Y_d}{\sum_{k=1}^{K} \left( \frac{p_k}{p_d} \right) \sum_{l=1}^{L} \left( \frac{p_l}{p_m} \right) d_{kl}} \right) \]

Where \( m, d \in \{1, \ldots, M\}, \ m \neq d \) are countries; \( k \in \{1, \ldots, K\} \) is the set of all distinguishable districts in country \( d \); and \( l \in \{1, \ldots, L\} \) is the set of all distinguishable districts in country \( m \). Defined by these indexes, \( p \) represents population, \( Y \) is GDP, and \( d_{kl} \) is the measure of distance between districts \( k \) and \( l \). Remoteness then, amounts to a standard gravity relationship, in which the product of market sizes is divided by the distance that separates them, summed over all possible world trading partners.

One final issue that ought be mentioned about the dataset is possible endogeneity in the dummy variable for free trade agreements. Baier and Bergstrand (2004) found decisively that the formation of these agreements does not come completely by chance but rather is influenced by many factors which connect some countries more than others. This presents a challenge for the appropriate estimation of free trade agreements, but two points largely remove that concern for the present case. To begin, the first three of five factors that their 2004 study found to be significant in predicting free trade agreements—the distance between two countries, remoteness, and the relative size of two countries—will be included separately in the gravity model used by this paper (S. Baier & Bergstrand, 2004). Therefore, any omitted variable bias that might have been introduced for free trade agreements on account of the direct influence of these three areas on diversification will be removed. Second, the effect of this bias is, in the literature, largely thought to be an underestimation of the effects of free trade agreements. Therefore, if the effect of free trade agreements is found to be significant and

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5 Adam and Cobham (2007) provide a helpful definition of multilateral resistance terms, describing them as “the barriers to trade that each country faces with all its trading partners.” This is contrasted with general bilateral trade resistance, which includes “the barriers to trade between a pair of countries, but also multilateral trade resistance.”

6 The method most often used in the literature to include terms of multilateral resistance is a fixed effects estimator (Head et al., 2010). Again, however, the present dataset only has information regarding Ecuadorian exports, so in this case a fixed effects estimator would not represent differences in trading partners that are felt by every other country in the world. Instead, it could only describe differences across trading partners that were experienced by Ecuador.

7 Here, to be a distinguishable district means that data was available on its population and geographic location. Each country was divided into the greatest number of divisions possible given national data limitations. The data necessary to calculate this indicator is available through the Gravity dataset (Head, Mayer, & Ries, 2010).

8 Trefler (1993) found that ignoring the endogeneity of free trade agreements resulted in an estimation of its effect ten times too low, and Baier and Bergstrand (2007) found the results to be underestimated by 75–85%. The large
positive in the models to come, then the possibility of this bias in the data should only support that result.

2.2 Econometric Method

2.2.1 Formulation of the Gravity Model

In its most basic form, the gravity model holds that trade between two countries \(i\) and \(j\) increases log-linearly with the product of their GDPs and decreases with the distance between them. The empirical reliability of the model is firmly established in the literature, and in recent decades it has served as the main workhorse model for questions of international trade (Helpman et al., 2008).\(^9\)

\[\]

2.2.2 Estimation Equation

The theoretical framework that this study will rely upon is that of Melitz (2003), which models the decision of domestic firms to enter into international markets based on their own profits and the level of fixed costs required for entry. Amurgo-Pacheco and Pierola (2008) expressed one of the conclusions of this model in the following equation:

\[ V_{od} = \begin{cases} \alpha \cdot \frac{n_\sigma}{\sigma - 1} \cdot \frac{B_d \cdot dG[a \cdot a_{oo}]}{\sigma - 1} & \text{if } \sigma \leq \alpha \cdot \overline{a}_{od} \\ 0 & \text{otherwise} \end{cases} \]

Where \(V_{od}\) is the total per firm value of bilateral exports between countries of origin \(o\) and of destination \(d\); \(\tau_{et}\) represents the bilateral trade costs; \(B_d\) is a demand shifter for country \(d\); \(n_\sigma\) is the endowment of country \(o\); \(\sigma\) gives the elasticity of substitution between products; \(a\) provides the marginal costs of trade; and \(G[a | a_{oo}]\) is a conditional density function showing the distribution of marginal costs in country \(o\). Note that \(\overline{a}_{od}\) and \(a_{oo}\) are constants, where \(\overline{a}_{od}\) is the fixed cost of entering the market in country \(d\) and \(a_{oo}\) is the fixed cost of entering the domestic market in country \(o\). \(G[a | a_{oo}]\) is therefore conditional on \(a_{oo}\), as a company that cannot surmount the fixed costs of entering their own domestic market will clearly not be exporting to international markets.

Again following after the method of Amurgo-Pacheco and Pierola (2008), each product category \(j\) will be treated as coming from a single firm within the model. Then, proxies can be chosen to stand in for the different variables in the model. \(B_d\) will be represented by the GDP of country \(d\); \(n_\sigma\) by the GDP and other internal characteristics of country \(o\); and \(\tau_{et}\) by distance and other sources of bilateral trade costs between countries \(o\) and \(d\). The remaining terms can be incorporated into a constant \(\alpha\), and, because the country of origin \(o\) will always be Ecuador in this study, that subscript is replaced with \(ec\). What results is a gravity model of trade in which

\[ V_{ec,d} = \alpha \cdot \frac{M_{ec} \cdot M_d}{d_{ec,d}} \left( \prod_{k=1}^{K} \tau_{ec,d,k} \right) \left( \prod_{l=1}^{L} n_{ec,l} \right) \]

Where \(M\) is the size of the economy measured by GDP; \(d_{ec,d}\) is the distance between Ecuador and country \(d\); \(\alpha\) is a constant; \(\tau_{ec,d,k}\) is comprised of \(k \in \{1, \ldots, K\}\) variables on bilateral trade costs; and \(n_{ec,l}\) represents the \(l \in \{1, \ldots, L\}\) variables on characteristics internal to Ecuador. This formula will be estimated according to the following log-linear construction, indexed over export destination \(d\), product category \(j\), and year \(t\):

\[ \log v_{ec,d,j,t} = \alpha \cdot \gamma_t + \kappa_j + \lambda_j + \beta_1 \log M_{et} + \beta_2 \log M_{dt} - \beta_3 \log d_{ec,d} + \sum_{k=1}^{K} \beta_k \cdot \tau_{ec,d,t,k} + \sum_{l=1}^{L} \beta_l \cdot n_{ec,t,l} + \varepsilon_{djt} \]

difference between these two studies likely has to do with their samples. Trefler (1993) limited his focus to data on US trade, while Baier and Bergstrand (2007) used data from around the world. Nevertheless, both showed an effect in the same direction, and both are relevant in the case of Ecuador because the United States is the destination of a large portion of Ecuadorian exports—almost 40% in 2015.

\(^9\) A few of the more influential studies that have used it include Helpman et al. (2008), McCallum (1995), and Rose (2000). Leamer and Levinsohn (1995) held that gravity models “have produced some of the clearest and most robust empirical findings in economics,” expressing a sentiment that echoes throughout the literature. For examples, look to Anderson and van Wincoop (2003), Bikker (2009), and Deardorff (1998).
The only additional variables to mention here are $\gamma_t$, the year; $\kappa_d$, which is the remoteness variable calculated earlier; and $\lambda_j$, the factor variable categorizing the export into one of fifteen product categories. These are important controls, but their coefficients will be of little interest.

### 2.2.3 Estimation Method

A key problem with the standard gravity model is that it cannot explain or incorporate zero trade flows. Their existence would require a pair of countries where either the distance between them were infinite or the market size of at least one of them was zero. Melitz’s theory partially solves this problem in providing a model that allows for zero trade flows, but its log-linear form still does not allow for the existence of these zeros to be estimated because the log of zero does not exist. Fortunately, there are several ways of estimating the model that solve this problem, like adding one to every export trade flow, using a quasi-maximum likelihood Poisson estimator,\(^{10}\) or using nonlinear least square estimation (Gómez-Herrera, 2013).

Among these methods, the Heckman method stands out for its ability to separate the intensive and extensive margins of diversification.\(^{11}\) It begins with a Probit estimation, where the dependent variable is binary and indicates whether Ecuadorian companies have entered into the market defined by product category $j$ sent to destination country $d$ in year $t$. In this way, the probability of entry into the sample of positive export values is estimated, providing a control against sample selection bias (Helpman et al., 2008). In this paper, three separate models are estimated: first, a point of reference is established by only the standard gravity variables, then Ecuador-specific variables are included, and finally information on business costs is added.\(^{12}\) Baldwin and Harrigan (2011) used results from this kind of Probit model directly as a measurement of what determines market entry. To examine the extensive margin specifically, a dummy variable is added to the Probit which takes on a value of one if that product was exported to that destination in the last three years.\(^{13}\) With this dummy in place, the coefficients of the other terms can be interpreted directly as effects on the extensive margin, measuring change against the status quo set by the previous three years.

Next, in the second stage of Heckman estimation, the results of the three Probit models are incorporated by the inverse Mills ratio into three corresponding OLS estimations where the log of trade value in US dollars is the dependent variable. A few of the original Probit variables are taken out as exclusion variables,\(^{14}\) but the majority are included a second time so that their impact on the intensive margin of trade can be checked. Much like in the Probit model, the dummy variable for a given export product category and market destination can again be incorporated to examine the marginal impact of variables on the value of trade in new products specifically. This will be completed through an interaction term between the dummy variable and other variables of interest.

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10 The full paper includes estimations from these first two methods as a robustness check to the selected method.
11 The Tobit method also allows for this separation. The full paper discusses why the Tobit method was not used, despite its being the preferred method of Amurgo-Pacheco and Pierola (2008). A major reason was that, of the methods tested by Gómez-Herrera (2013), the Heckman method worked the best and gave results that were easiest to interpret.
12 The reason business costs are not immediately included in the Ecuador-specific variables of model two is that they are available over fewer countries and fewer years, forcing the total number of observations to be cut in half. Specifically, the business cost variable is available beginning in 2003.
13 This follows the methodology of Amurgo-Pacheco and Pierola (2008).
14 Variables will be chosen for exclusion from this second stage of the Heckman model in two ways: first, by the theory. It is argued by Helpman et al. (2008) that the costs and procedures involved in opening a business ought to be considered as factors that affect market entry but do not influence the value of goods traded once there. Therefore, the two variables which address these topics in Ecuador and destination market $d$—Entry_cost_esc, and Entry_cost_dt—are excluded at this second stage. Second, empirically, variables are excluded if they are insignificant in a simple OLS estimation of the log of trade value against the three models outlined above. Appendix 3 in the full paper shows this regression. As this method ignores all zero trade flows, the estimations are likely biased and only serve the decision of what variables to remove. In this way, a measure of ethnic similarity, Common Ethnicitiesdt; a binary variable that indicates if a country is surrounded by land without access to an ocean, Landlockedd; and a variable representing macroeconomic stability, Ecu.Inflationt-1, are also removed from the second stage Heckman estimation.
4.3 Measuring Diversification

With the gravity model and Heckman method now selected, it is important to pause for a moment and consider how this combination will be used to measure diversification specifically. Two factors were particularly important in the selection of a diversification measurement for this study.

First, both the extensive and intensive margins of diversification are significant. The intensive margin, on one hand, explains the large majority of export growth (Cadot, Carrère, & Strauss-Kahn, 2011). Amurgo-Pacheco and Pierola (2008) found that at the six-digit level of HS classification, the intensive margin accounted for some 86% of the growth in exports over the time periods and countries studied. On the other hand, the extensive margin accounts for 60% of the difference in export structures between rich and poor countries (Hummels & Klenow, 2005), and the most recent theoretical predictions on the benefits of diversification center on the extensive margin (Mau, 2016). Therefore, a measurement of diversification that can analyze effects along both margins ought to be selected.

Second, considering that there is only one country of interest in this study, an index which aggregates the diversification of the entire country into one measurement is not likely to give many interesting results. Of the three methods most commonly used in the literature—the Herfindahl, Gini, and Theil indexes (Agosin et al., 2012)—only the Theil can be disaggregated to consider diversification at a more local level.

Instead of using the Theil index, however, a measurement of diversification will be used that gets as close to measuring individual products as possible within the HS system. This is done by exploiting the existence of zeros in the trade matrix. As mentioned earlier, a zero trade flow is defined as an export of goods from any six-digit category j to any country d in any year t from 1991 to 2015 that could have been, but was not. A diversification event along the extensive margin, then, comes anytime one of these zero trade flows becomes positive. Specifically, this study will count a positive trade flow as new, and therefore representative of diversification along the extensive margin, if the given product category was not sent to the given export destination market during the three years prior. Note that under this definition, a diversification event could take the form of either an entirely new product added to Ecuador’s export basket or simply a new destination market for some Ecuadorian product.

Once estimations have been made with the Probit equation as to what is driving diversification along this extensive margin, the results can be used by the Heckman method to help remove sample selection bias in an estimation of what

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15 The Thiel index requires some degree of aggregation, but can be used to get very close to the level of disaggregation possible in the selected method. The method selected instead of the Theil Index faces many criticisms in the literature, each of which are discussed and refuted in the full paper. When reading that section, however, it will be worth remembering that when the Theil index is disaggregated in this way it suffers under the same criticisms (Cadot, Carrère, & Strauss-Kahn, 2011).

16 The export of a new product to an entirely new market technically fits the bill under both categories, but will not, for that reason, be counted as a “double” diversification.
drives greater trade value among these export flows, that is, trade along the intensive margin. In summary, then, the chosen method breaks the act of diversification down into two sub processes—adding new products to the export basket, and achieving greater trade value among those new products—measuring each separately to gain a more complete understanding of the full process of diversification.

3 Results

3.1 Probit

Table 1 shows the marginal effects of each variable on market entry, or the probability that Ecuadorian firms will complete a trade flow marked by product category $j$ and destination $d$. The Trade Last Three Years Dummy $_{dt}$ takes on a value of one when the export flow $\{d,j,t\}$ was executed at some point over the last three years. With this variable in place, the coefficients of the remaining estimators can be interpreted as effects on the probability that Ecuador’s market entry status from the last three years will change.

Unsurprisingly, having completed a given trade flow in the past three years is the strongest determinant that it will be completed in the present year $t$. Whereas remoteness, distance, and the internal distance of a country make it more difficult to enter into new markets, the size of the receiving market facilitates that entry. While dollarization and entry into free trade agreements are found to be highly significant in promoting diversification along the extensive margin, macroeconomic instability and the revolución ciudadana of President Correa are significant in decreasing it.

Table 1. Probit Estimation Measuring the Effect of Each Variable on the Extensive Margin of Diversification. Dependent Variable: Trade Dummy

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<td>Log(GDP$_{ec}$)</td>
<td>-0.009***</td>
<td>-0.001</td>
<td>-0.015***</td>
</tr>
<tr>
<td>(0.017)</td>
<td>(0.024)</td>
<td>(0.080)</td>
<td></td>
</tr>
<tr>
<td>Log(GDP$_{d}$)</td>
<td>0.004***</td>
<td>0.004***</td>
<td>0.002***</td>
</tr>
<tr>
<td>(0.007)</td>
<td>(0.007)</td>
<td>(0.010)</td>
<td></td>
</tr>
<tr>
<td>Log(Unweighted Distance)</td>
<td>-0.003***</td>
<td>-0.003***</td>
<td>-0.003***</td>
</tr>
<tr>
<td>(0.008)</td>
<td>(0.008)</td>
<td>(0.010)</td>
<td></td>
</tr>
<tr>
<td>Common Currency</td>
<td>0.002***</td>
<td>-0.000</td>
<td>0.018***</td>
</tr>
<tr>
<td>(0.014)</td>
<td>(0.015)</td>
<td>(0.020)</td>
<td></td>
</tr>
<tr>
<td>Common Official Language</td>
<td>-0.010***</td>
<td>-0.014***</td>
<td>0.006***</td>
</tr>
<tr>
<td>(0.023)</td>
<td>(0.023)</td>
<td>(0.011)</td>
<td></td>
</tr>
<tr>
<td>Common Ethnicities</td>
<td>.011***</td>
<td>0.013***</td>
<td></td>
</tr>
<tr>
<td>(0.020)</td>
<td>(0.021)</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

17 Regressions over the entire dataset of 50 export destinations are available in the full version of the paper, as well as other estimation methods included for a check of robustness.

18 One somewhat confusing result is the negative sign associated with the GDP of Ecuador. Gonzalez (2010) found something similar in the case of Ecuador, which is that the most rapid periods of GDP growth are typically associated with a focus on a single export product. In the case of this time sample, that would likely be the rise of petroleum, the export value of which increased dramatically following higher oil prices in 2003. Gonzalez (2010) also notes, however, that these periods of rapid growth were generally associated with much greater macroeconomic volatility that needed to be calmed before further growth occurred. Burneo and Oleas (1996) hold that in the long term, when these temporary shocks are evened out, Ecuadorian trade data shows that this macroeconomic stability is critical for lasting growth in GDP. Therefore, the negative sign associated with Ecuador’s GDP is likely a bias created by the relatively short period of time considered by this study, a period that centered around a large uptick in the export value of petroleum.
Note: This table reports the marginal effects at the average value of each variable. In other words, a one-unit increment in the variable beginning at its average value would result in a change in the probability of an Ecuadorian firm changing its market entry status from the past three years equal to the listed effect. There was also a constant in this regression and a factor variable controlling for each of 15 product categories, but neither of these were included in the above table. In the case of the constant, it was not included because reporting the marginal effects of a constant is nonsensical. The factor variable over 15 categories was not included to conserve space. Note also that Entry_cost_ec and Entry_cost_d were only available after 2003, and so in the resulting subsample of the data, it was impossible to separately estimate the effect of dollarization.

3.2 Second-stage Heckman Estimation

The three models shown in Table 1 incorporate the inverse Mills ratios from their corresponding models in Table 1. Otherwise, the models shown here are almost identical, with the only other difference being the removal of certain exclusion variables from this second stage.

Panels 1 and 2 of Table 2 disentangle the effect of each variable on previously completed trade flows, in which the product category $j$ was exported to export destination $d$ in the past three years, and new trade flows in which that was not the case. This separation is achieved in each of the three models through an interaction term with the $Trade\ Last\ Three\ Years\ Dummy_{djt}$. As a result, the statistical significance markings in panel 2 represent the significance with which that variable’s effect on old trade flows differs from its effect on new trade flows. A quick look over Panel 2, then, reveals that many of the variables differ significantly in their effects on these two trade types. The effect of free trade agreements, for example, is found to be positive in both groups, but consistently higher for new than old. This makes it a policy of special importance in the pursuit of diversification along the intensive margin. Other effects, however, like the GDP of the destination country, seem to be quite consistent between the two panels.
Table 2. OLS Estimation Measuring the Effects of Each Variable on the Value of New and Old Trade Flows. Dependent Variable: Natural Log of Export Trade Value in Nominal US Dollars

<table>
<thead>
<tr>
<th></th>
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<tbody>
<tr>
<td>Log(GDP_ec)</td>
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<td>0.719***</td>
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<td>(0.115)</td>
<td>(0.059)</td>
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<td>Log(GDP_d)</td>
<td>0.723***</td>
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<td>(0.059)</td>
<td>(0.081)</td>
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<tr>
<td>Log(Unweighted Distance)</td>
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<td>-0.865***</td>
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<tr>
<td></td>
<td>(0.081)</td>
<td>(0.072)</td>
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<tr>
<td>Common Currency</td>
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<td>-0.294***</td>
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<tr>
<td></td>
<td>(0.104)</td>
<td>(0.019)</td>
</tr>
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<td>Common Official Language</td>
<td>0.630***</td>
<td>0.964***</td>
</tr>
<tr>
<td></td>
<td>(0.077)</td>
<td>(0.100)</td>
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<tr>
<td>Common Legal</td>
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<tr>
<td></td>
<td>(0.085)</td>
<td>(0.087)</td>
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<tr>
<td>Internal Distance</td>
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<td>0.0005***</td>
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<td></td>
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<td>(0.0001)</td>
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<td>Pta</td>
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<td>(0.113)</td>
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<td>(0.243)</td>
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<tr>
<td>Ecu.Inflation_{t-1}</td>
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<td>(0.001)</td>
<td>(0.015)</td>
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<td>(0.110)</td>
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<tr>
<td>Correa</td>
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<tr>
<td></td>
<td>(0.081)</td>
<td>(0.120)</td>
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<tr>
<td>Gatt</td>
<td>-0.550***</td>
<td>-0.197***</td>
</tr>
<tr>
<td></td>
<td>(0.084)</td>
<td>(0.095)</td>
</tr>
</tbody>
</table>

*p<0.1**p<0.05***p<0.01

Of special importance in panels one and two are the variables relating to Ecuador specifically, because the Ecuadorian government has more control over these. The revolución ciudadana of President Rafael Correa, to begin, shows a negative effect that is quite consistent between old and new trade flows, and echoes the same negative effect found at the extensive margin of diversification. Together, these results present evidence that the policies of Correa have hurt diversification efforts. Similar findings globally (Pollard, Shackman, & Piffaut, 2011) and from Ecuador (Burneo & Oleas, 1996) support that increases in public spending like that of the Correa administration are generally associated with lower growth.

Next, the proxy variable for macroeconomic stability, Ecu.Inflation_{t-1}, actually switched signs between old and new trade flows. This trend fits with the previous theory and findings of Gonzalez (2010), who held that periods of rapid growth in GDP and Ecuadorian trade are often centered on one or a few previously traded products, and that this trend comes with greater macroeconomic instability. Greater diversification into new trade flows, however, comes when there is lesser macroeconomic instability. For this reason, then, the new trade flows decrease with Ecu.Inflation_{t-1} and the old trade flows increase. These results also support the previous argument made for why Ecuadorian GDP was at times inversely associated with diversification along the extensive margin—in short time periods like the one measured by this study, rapid growth is often clustered around the rapid rise of a few products at the expense of others.19

19 One result that is more difficult to interpret is a negative effect associated with the dollarization of the economy. An attempted explanation is available in the full paper.
3.3 Relating the Results to Aggregated Measures of Diversification

These results can be easily reaggregated to show a broader trend in diversification. This subsection will do just that using the HHI Index.\textsuperscript{20} Figure 2 shows the HHI of the non-petroleum economy calculated over time for both of these data sources, first using the real data, and then the fitted values from model two.\textsuperscript{21}

With a method of aggregating the fitted values, it is now possible to see what the marginal effects listed for each variable mean over an entire economy. It is not enough, however, to aggregate the effects in a particular year, because the effects of every variable are cumulative across years. A robust and expected finding from the results was that if some product category \( j \) was exported in one year, then its chances of being exported the next went up drastically. Therefore, to the extent that any variable in the model causes a product to be exported, that variable also indirectly affects the probability of said product being exported in the years to come. By adding a variable to the second stage of the Heckman analysis controlling for the export value of each product category \( j \) last period, a similar effect can be seen there as well: Export lines with high trade value one year are likely to have high trade value the next.

![Figure 2: HHI in the Non-petroleum Economy](image)

In order to capture this cumulative effect, then, and truly represent an aggregation of a variable's impact, it is necessary to simulate the model across time. A convenient period in the present sample over which to do that simulation is 2007-2009, which corresponds to Rafael Correa's first term in office and was a time of great change in the Ecuadorian political landscape. The simulation will be completed as follows: First, the coefficients from Probit Model two will be used to find fitted values of the probability that each category \( j \) will be exported in the year 2006. Second, the simulation will decide whether a particular export line happened or not by pulling a value from a binomial distribution defined by the fitted probabilities found in the first step. If the resulting value is one, then the export is considered to have happened, and if the value is zero, then the export is considered not to have happened. Third, a subset is taken of the data to include only those product categories which have been traded under this simulation. That reduced sample is run through the second stage OLS calculation to determine fitted values for what the hypothetical value of each trade would be. Fourth, the simulation updates the \( Trade \ Last \ Three \ Years \ Dummy_{dj} \) and \( Value \ Traded \ Last \ year \) variables in 2007 to reflect the fitted values from 2006.\textsuperscript{22} Once these four steps are completed, they can be done again for 2007 with the updated data, then for 2008, 2009, and 2010.

\textsuperscript{20} The HHI Index is calculated as the sum of the squared market percentages taken over each of 15 product categories defined by the HS system.

\textsuperscript{21} Model two will be used for the duration of this section because unlike model three, its results are available across the entire sample period of the study. Also, this section will consider metrics of the non-petroleum economy. This is because Ecuadorian petroleum exports account for around 50% of all exports over the sample period, which is an outlier so extreme that it cannot be captured meaningfully in the chosen statistical method and yet has a large impact on calculations like that of the HHI.

\textsuperscript{22} Notice that no other variables in 2007, and all subsequent years in the iterations to come, are changed. Thus, other included variables like the GDP of the receiving country, the remoteness of the receiving country, and the consumer price inflation in Ecuador are all viewed as exogenous changes in this process.
In order to remove improper variation in the data, this full process over all of the years is completed by the computer 2500 times, and the mean values over all of those trials are recorded.

This method can be used to trace the hypothetical effects of counterfactuals in the model. Perhaps the simplest of these counterfactuals involve the dummy variables, which can only take on one of two values. The remainder of this subsection, then, will illustrate the above simulation in the case of the free trade agreements dummy variable. One of the clearest conclusions coming from the Probit and Second-Stage Heckman above was that free trade agreements reap benefits along both the extensive and intensive margins, and so these effects should be visible in the aggregate as well. Three versions of the simulation will be run: A version in which Ecuador removed all of its free trade agreements in 2007, one in which Ecuador formed free trade agreements with all twenty countries in the dataset in 2007, and one in which the free trade agreements remain exactly as they did historically. Figure 3 shows the results, which are as would be expected.

![Figure 3: HHI in the Non-petroleum Economy under each of the Three Simulations](image)
References
704–723.
Cannibalism of Attendances by High-Profile TV Broadcasts – The Case of the English Football League

JAKE OWEN
Lancaster University

ABSTRACT
The English Football League (EFL) sits below the top tier of English football, the Premier League (PL), and consists of 3 leagues of 72 professional teams tiered, in order, from the Championship League to League One and then League Two. These leagues are connected by promotion and relegation of the best and worst performing teams respectively between the leagues at the end of each season. EFL teams create less revenue through TV broadcasting and sponsorship than PL teams, making match-day revenue a greater proportion of their income. Due to the sheer number of teams in the EFL, games are regularly scheduled concurrently with PL and UEFA Champions League (UCL) games that are broadcast on television. This paper empirically tests whether these concurrent broadcasts adversely affect match-day attendance for EFL games using data from 8,280 matches from the 2012/13 season to the 2016/17 season. By using fixed effects panel regression, it is found that there is potential for negative effects from live broadcasting high-profile games concurrently with EFL games on match-day attendances, in some cases by as much as 13.3%. However, this effect is not universally apparent.

1 I would like to thank Professor Rob Simmons and Dr Maria Navarro Paniagua for their support and guidance throughout this project.
Measuring the Returns to Public Innovation: Patent Quality and Federal R&D Spending

ADITYA PANDE
Georgetown University

ABSTRACT
The importance of research and development (R&D) for economic growth is a well-established fact. Using the Hall, Jaffe, Trajtenberg (2001) dataset on patent citations and budget data for 8 U.S. Federal government agencies, I analyze the impact of R&D spending on standard metrics of patent quality—number of citations and Hall et. al.’s synthetic measure, generality. I find consistent evidence of an extensive-intensive margin tradeoff between expanding patent output and lowering average quality. Federal government patent production seems to be a long-term process, perhaps even 15 to 20 years in duration. Elasticities of spending with respect to quality are harder to isolate but appear higher on longer-term projects. These findings are naturally extendable to international public-sector R&D.

1 Many thanks to Dr. Rodney Ludema and Dr. Robert Cumby for their encouragement and guidance throughout the semester. I am also very grateful to Dr. Fuad Hasanov for his limitless patience and invaluable advice.
1. Introduction
Using the Hall, Jaffe, Trajtenberg (2001) dataset on patent citations along with budget data for eight U.S. Federal government agencies, I analyze the impact of R&D spending on two standard metrics of patent quality via various fixed effects panel regressions. The metrics of quality used are the number of citations a patent acquires, as well as Hall et. al’s synthetic measure of the distribution of those citations, generality. I find, in line with previous literature, consistent evidence across many regression specifications of an extensive-intensive margin tradeoff between expanding patent output and lowering the average quality of patents. Unlike previous work, I examine the length of the federally-funded patent production process by regressing dependent variables on lagged R&D spending. This process seems to be a long-term endeavor, perhaps even 15 to 20 years in duration. Elasticities of lagged spending with respect to quality are harder to isolate but tend to be higher on longer-term projects. As one would expect, there are differences in patent quality across federal agencies.

2. Literature Review
Henderson, Jaffe, and Trajtenberg (1997), in a pioneering evaluation comparing patents held by universities to those held by the private sector, devised two seminal measures of patent “quality”: generality and originality. Using the US Patent and Technology Office’s categorization of patents into 800 technological classes, they proposed calculating a Herfindahl index of concentration for patent citations: \( 1 - \sum s_i^2 \), where \( s \) is the share of patent \( i \)’s forward citations coming from class \( j \). This synthetic measure is generality. (Higher numbers here imply less concentration and hence a more general patent.) Originality is analogous but uses backward citations instead. Therefore, a patent citing a wide variety of fields and cited in many fields will have high originality and high generality. I use generality along with number of citations as my key dependent variables as these have become the benchmark for analysis.

Henderson et. al find two interesting results that I explore further. First, studying a dataset including both university and corporate patents from 1975-1980, Henderson et. al (1997) find a strong correlation between the originality and the generality of a given patent. Secondly, at least for university patents in the 23 years between 1965 and 1988, Henderson et. al (1998) find a tradeoff between increased patent production and a fall in the average quality of patents, driven largely by the patenting of poor quality innovations. They do not, however, study R&D expenditure’s effect on quality. Jaffe, Fogarty, and Banks (1998) examine time trends in NASA R&D (from 1963-1994) and raw patent production per dollar of R&D spending, but only regress generality on time and patent class dummies rather than on R&D. They do compare NASA patents to other federal patents, but again, not systematically in a panel regression. Furthermore, they did not have access to a newly updated patent data series which runs through 2006. The Dept. of Energy is the subject of another paper by Jaffe and Lerner (2001). They use the various laboratories of the D.o.E. and patent data from 1981-1993 to construct a panel dataset, finding a significant effect of R&D on patent production but, as above, a negative association between R&D expenditure and patent quality.

I model my empirical strategy on Jaffe and Lerner’s, regressing as they do the logarithm of citations per patent on the logarithm of R&D while including lab fixed effects. They do not, however, regress on generality as a dependent variable, include time-fixed effects, or lag R&D expenditure. They find an elasticity of patents with respect to R&D of .22 but insignificant effects of R&D on quality when controlling for number of patents. Number of patents, in their specification, has a significantly negative correlation with citation count. This negative correlation is as expected if there exists an extensive-intensive margin tradeoff.

3. Model
I propose a Cobb-Douglas production function of R&D, wherein for example,

\[
#\text{citations}_{i,t} = \sum_{n=1}^{N} R&D_{i,t-n} \beta_n (#\text{patents})^\mu \lambda^e \tau_t
\]

Categorizations varying from Class 002, “Apparel” to Class 800, “Multicellular Living Organisms and Unmodified Parts Thereof and Related Processes”.

Forward citations: citations to patent i made by other patents in the future. Backward citations: citations of past patents made by patent i.
where ift indexes a particular patent produced by a given agency in a given year. I add number of patents produced by entity f in year t on the right-hand side to capture the extensive margin effect. Adding originality on the right side may also be useful, following Henderson et. al (1997)—it is plausible that more original patents are also more general, independently of the effect of R&D:

\[
\text{generality}_{ft} = \sum_{t=1}^{N} \beta_n (\#\text{patents}_{ft-n})^\mu \text{originality}_{ft} \gamma e^{\eta}.
\]

Note that filing year is the relevant index year t. Given that patents take an average of two years to be processed by the USPTO (Hall et. al (2001), using the year the patent was granted would add noise and require the assumption that the processing delay is uncorrelated with R&D spending. f is an agency fixed effect, \(\tau\) a time trend, and \(\beta_n, \mu, \gamma, \eta\) the various coefficients for lags on number of patents and R&D. Taking logs for convenience,

\[
\ln(\text{generality}_{ft}) = \sum_{t=1}^{N} \beta_n \ln(\text{R&D}_{ft-n}) + \sum_{t=1}^{N} \mu_n \ln(\#\text{patents}_{ft-n}) + \xi \ln(\text{originality}_{ft}) + f_f + \tau_t.
\]

There does not seem to be reason to assume constant returns to scale. Although generality and originality are synthetic measures ε [0,1], the interpretation is still valid here. We drop observations where generality and originality are 0.

4. Description of Data

The dependent variable of interest is patent quality, as measured by number of citations, generality, and originality. After dropping unassigned patents, multi-assignee patents, and patents without generality or originality data from the NBER Patent dataset, we are left with 2,536,929 unique patent observations and 2.5 million accompanying citation counts, associated with 207,948 unique issuers. However, these patents are associated with only 1.7 million generality and 2.1 million originality observations. We again assume the missing values are random. Finally, we eliminate most of the observations in the dataset, because we can only use patent observations with associated spending data.

The independent variable of interest is annual federal R&D spending across eight agencies, here compiled by the American Association for the Advancement of Science from yearly agency reports to the Office of Management and Budget. There are no missing spending values for the entire 1976-2006 period, giving 240 budget observations. The data are in constant 2016 dollars. Given that we only study 8 agencies, this leaves 10,898 patent observations associated with these agencies and their activities over the 30-year period.

5. Empirical Results

5.1 Simple OLS Regression

I first regress number of patents on R&D spending. Unless otherwise noted, all regressions are log-log—in line with the model and for easy interpretation of coefficients as elasticities. The coefficient is positive and significant at the 1 percent level (Table 1). This accords with intuition—more R&D spending should yield higher quality patents. There are many sources of possible bias, however. Entities with higher R&D spending could be inherently better at producing quality patents, perhaps due to mission or culture. General technological trends associated with time may mean patenting simply increased alongside increased budgets rather than because of them. The coefficients of OLS regressions of number of citations, originality, and generality on R&D (Table 1, Columns 2, 3 and 4) are either insignificant or appear perversely signed. However, similar concerns about bias still apply. For quality variables, technological trends within patent class combined with changes in patenting patterns across classes, correlated with R&D spending, could also contribute to this. (Note that standard errors are robust given that errors might be heteroskedastic.)

4 As defined previously.
5 The Departments of Agriculture, Commerce, Energy, Transportation, and Interior, as well as the EPA, HHS (including the National Institutes of Health), and NASA.
6 Spending is in constant 2016 dollars.
### Table 1. OLS regression without controls, robust p-values in parentheses

<table>
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<tr>
<th>VARIABLES</th>
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<td>-0.301***</td>
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<td>R-squared</td>
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<td>0.000</td>
<td>0.003</td>
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</tbody>
</table>

5.2 OLS with fixed effect dummies (associated: Table 2, see Appendix)

I add, then, time, agency, and the 800 patent class dummies to improve the first-pass estimates. Standard errors are robust given that there are only 8 entities, too few to cluster. The effect of R&D on number of patents now becomes strongly significant and positive. Indeed, the estimated elasticity of 1.3 is too positive--increasing returns to R&D seems implausible. Interestingly, adding controls fails to raise the generality and number of citations regressions to significance.

### Table 2. OLS regression including 8 agency, 30 year, and 800 patent class variables, robust p values in parentheses

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5.3 Fixed effects regressions (See Appendix for associated tables)

Insofar as R&D spending in the year of filing is correlated with R&D spending in the years prior, the above regressions are still useful. However, the above regressions do not account for lags. Hence, I create a panel dataset. The quality dependent variables now become average citations per patent (or average generality per patent) for a given agency in a given year.

\[
\ln \left( \frac{\sum_{t=2}^{N} \text{citations}_{ft}}{\# \text{patents}_{ft}} \right) = \alpha + \sum_{t=1}^{N} \beta_n \ln(\text{R&D}_{f,t-n}) + \mu \ln(\# \text{patents}_{f,t}) + \eta_t + \tau_t + \varepsilon_{ft}
\]

This makes a standard panel with 222 entity-year level observations (8 agencies over 30 years), each with associated R&D spending.

5.4 Interpretation

The negative effect of R&D on patent quality when not controlling for number of patents, in the initial OLS (Table 1), is clearly anticipated by the model and in the literature. This confirms the prediction that assume that returns to R&D expenditure in terms of quality diminish faster than the returns to R&D in terms of number of patents. Again, this recalls Romer’s “fishing out” effect discussed above—the best ideas are caught first. When budgets expand, the most promising projects with the highest quality patents are funded first, followed by projects with more marginal patents. This appears in the data as a reduction in average quality due to an increase in R&D. However, we are also interested in the effect of R&D spending on quality itself. Hence, we control for the number of patents. This should yield unambiguously positive coefficients— in other words, patent quality in two different years with the same patent output should be positively correlated with R&D. Column 8 of Table 2 illustrates this for number of citations—the elasticity of .138 is significant with a p-value of .06. The coefficient on number of patents is also negative, as expected. Controlling for budget, more patents filed in a given year implies lower R&D per patent and hence lower quality. This expected relationship does not hold in the regression for generality but does not strongly contradict our expectation.

As previously discussed, patents vary in the length of their development. Some may be the result of R&D conducted 20 years prior to the filing date, some fruits of efforts just two years prior. There is no way to know the origination date of a particular project, and hence, patent. Therefore, lags are necessary to capture proper causal effects. Additionally, there may be a similar quality effect of R&D based on project length—expanding budgets could mean longer-term projects with higher but more uncertain returns get funding. In this case, one would expect higher quality returns to be associated with greater lags.

Regressions on lagged R&D with number of patents as a control (Tables 3-5) suggest, albeit weakly, that the quality benefit of increased R&D dominates the negative extensive margin effect of increased R&D, resulting in a positive correlation between average quality and long-run R&D. This makes the intuitive point that R&D is likely a long-term (possibly 15-20 year) process, but one with positive elasticities with of number of patents\(^7\) (1.3 > \(\varepsilon\) > .0001), citations\(^8\) (surely >.138), and generality\(^9\) (0.2-0.4) with respect to R&D.

5.5 Puzzles

---

7 See Table 2.
8 Table 2.
9 Table 5.
The most puzzling results are the persistently negative coefficients on lagged R&D and patent output. Griliches and Pakes (1984) find similarly negative coefficients in their 5-year distributed lag model as well as large contemporaneous R&D effects. They point out that “…the data cited above do point to a gestation lag highly skewed with large early year coefficients, and any minor misspecification in the model could push all this effect into the coefficient of [year t].”

I regress quality variables on lagged R&D and multiple lags of number of patent variables in Tables 9 and 10, relaxing the assumption that the quality of patents filed for in a given year is affected only by the number of patents filed in a given year (i.e. the assumption that patents filed in a given year all have the same duration). The trends found previously (i.e. increasing quality for longer lags, substantial positive elasticities for generality) weaken for number of citations but grow stronger for average generality.

6. Conclusion
The balance of the evidence, then—from OLS regression on patent-level observations (Table 2) to panel regression on entity-year averages of dependent variables along with a variety of lags (Tables 3-5)—supports several conclusions consistent with the theory. Patent production appears to be a relatively long process, perhaps even 20 years long. Originality matters independently of R&D spending as a determinant of quality. Federal agencies differ in the quality of their patents, NASA and the Dept. of Health and Human Services standing out as the best. The extensive margin effect by which R&D stimulates patent production mostly dominates the intensive margin improvement in quality of patents, as predicted by theory. At longer lags of 15-20 years, which we can assume to be longer-duration projects, the quality improvement may dominate the extensive margin effect.
References


### Table 3. Panel Regression: ln(Average Generality) with Number of Patents as Control

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### Table 4. Panel Regression: ln(Average Citations) with Lagged Number of Patents as Control

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Table 5. Panel Regression: ln(Average Generality) with Lagged Number of Patents as Control

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Credit Ratings and the Pricing of Sovereign Default Risk: 
A Retreat to Moderation?

CONNOR REGAN¹
Dartmouth College

ABSTRACT
This paper examines the relationship between the change in sovereign credit ratings and the change in sovereign CDS pricing using low frequency data to incorporate macro variables. This work is largely an extension of Aizenman et al.'s (2013b) “Credit Ratings and the Pricing of Sovereign Debt during the Euro Crisis”. I update the analysis with a more recent time-period and additional countries. I find that the changes in sovereign credit rating still significantly predict changes in monthly average CDS pricing; however, the strength of the effect is muted in recent years. Aizenman indicates there is an apparent shift in the pricing of sovereign risk beginning with the 2007-08 financial crisis. Based on this updated analysis, I suggest this shift may have been a passing trend caused by increased volatility and perceived risk of sovereign default during the financial crisis and subsequent Eurozone Crisis.

¹ Many thanks to the Dartmouth College Department of Economics. In particular, thank you to Professor Nancy Marion for your guidance and support.
1. Introduction and Overview
Market participants use credit ratings determined by private rating agencies as a measure of risk when making investment decisions. The marginal benefit offered by credit rating agencies (CRAs), that is, information not privy to the average market participant, could come from a variety of sources. CRAs have access to restricted information which could be essential to accurately measure risk. They could provide superior analysis or aggregation of market information in order to determine credit ratings. Additionally, the easily digestible format of a credit rating could be more impactful to a participant who is “rationally inattentive,” or, in other words, chooses to use readily available credit ratings when making investment decisions, instead of analyzing the plethora of underlying market data themselves. Regardless of the reason, it is important to consider the economic effects that CRAs can have on the entities which they rate – in this case, sovereign nations.

It is important to understand the implications of a credit rating movement. During the Eurozone crisis, it appeared that Greece was veering toward a sovereign default. Critics of CRAs, including prominent politicians in the European Union, were concerned that the agencies “decided” too early that a credit event was inevitable. In turn, they suggested that by issuing negative ratings movements, agencies increased the cost to secure external capital, creating a self-fulfilling default. This was an extreme case, but the concern persists in less drastic instances.

Credit Default Swap (CDS) spreads are a useful measure of default risk, given they quantify the cost of “insurance” against sovereign default. Aizenman et al (2013b) find that there was a significant relationship between CRA rating movements and CDS spreads for European countries during the most recent financial crisis. Aizenman et al. (2013b) notes a shift in historical trends for CDS pricing at the onset of the 2007-08 financial crisis. In order to understand if this shift has persisted or reverted to previous trends, I extend their analysis to 2018. I find that credit rating changes are still a significant predictor of CDS pricing, both statistically and economically. However, the economic impact of rating movements after 2014 is lower than in prior years. In line with current literature, I find that rating changes are still an important determinant of CDS pricing for countries in the European Union, and more so for those in the Eurozone. Additionally, I analyze the lead or lag of rating changes and CDS pricing to determine if rating agencies are timely in their movements. The results of this analysis are not particularly robust, but there is reason to suggest CRAs may lag behind market information or act contemporaneously and likely do not predict future market movement.

I start with an overview of prevailing literature on this topic (section 2), continue with data description and analysis (section 3), empirical specification and analysis (section 4), and close with concluding remarks (section 5).

2. Literature Review
Credit rating agencies have been the subject of economic research for decades. Early research focused on the efficacy of rating agencies and eventually turned outward to understand the economic impacts of CRAs. Cantor and Packer (1994) study the temporal nature of credit ratings. They find that while agencies effectively rank the risk of sovereign bonds at a point in time, the significance of specific ratings changes over time both within and across agencies. Considering credit ratings are used to evaluate investment decisions in institutional portfolios, in order for these regulations to be relevant they must be updated to reflect the current ratings trends of private agencies. Later, Cantor and Packer (1996) study the correlation between credit ratings and sovereign bond spreads in an effort to understand if rating agencies provide new information to market participants. They find that sovereign credit ratings explain 92 percent of cross-country variation in sovereign bond spreads for a group of advanced and emerging economies. Much of this correlation can be attributed to market data the public is aware of; however, Cantor and Packer do identify that credit ratings have an independent effect on sovereign spread, particularly in non-investment grade countries.

Interest in the economic effect of credit ratings diminished during the great moderation period. The 2007-08 financial crisis lead to increased volatility and, in turn, greater fluctuation in rating movements. This provided a new framework for studying the movement of credit ratings and their subsequent effects. Alsakka and Gwilym (2010a) analyzed the relationship between pairwise movements across multiple agencies. They find an interdependence between agencies' ratings. For example, a recent downgrade in sovereign credit rating greatly increases the probability that another agency
For many years, the prevailing literature regarding credit ratings and CDS spreads consisted of event studies. Hull et al (1994) study the effect of rating actions on corporate CDS pricing. They find that in the corporate sector, credit movements do not have a significant effect on CDS pricing. They suggest that rating agencies provide lagged information and actually respond to shifts in CDS pricing. Ismailescu and Kazemi (2010) employ the event-study framework on a set of 22 countries. Using rating actions from Standard and Poor’s, they identify that a rating increase leads to an average decrease in sovereign spread of 11 basis points in the two days following the event. They identify an increase of 67 basis points in spread given a rating downgrade. The disparity between rating movement direction is consistent through prevailing literature, particularly for investment grade sovereigns. Recently, Chari et al (2017) analyze rating movements for Puerto Rican bonds and change in spread of Puerto Rican government bonds, as well as sub-sovereign bonds from the territory. They find that rating movement in either direction is associated with significant cumulative change in basis points; however, negative rating actions have a stronger effect. This study, and other event-based analyses, typically use daily data which is helpful for isolating the immediate impact of a rating change but fails to capture long-term macro effects which affect rating movements and CDS pricing.

There are many studies which attempt to determine the factors of sovereign CDS pricing but few that incorporate credit rating movements. Longstaff et al. (2011) incorporate both local and global macro factors such as local stock prices, foreign exchange reserves, and U.S. equity indices. Aizenman et al. (2013a) focus on CDS pricing in Europe with a focus on fiscal variables. They identify a number of significant variables and note a change in the pricing trend of CDS spreads during the Eurozone crisis, particularly for GIIPS countries.

Aizenman et al. (2013b) study the effect of a combination of macro and financial factors in addition to the effect of credit rating changes on changes in CDS pricing. They find that credit rating changes are both statistically and economically significant. Their results are robust to the inclusion of other statistically significant variables. Building upon other research, they find that the relationship between credit ratings and CDS prices shifted before and after the financial crisis.

In short, there is a fair amount of variability in the body of research regarding credit ratings in CDS pricing. Over time, independent studies have informed our understanding of the influence of CRAs. Given Aizenman et al. notes a shift in the relationship between credit ratings and CDS prices shifted before and after the financial crisis.

### 3. Data and Descriptive Statistics

I use monthly data in the analysis ranging from February 2007 to January 2018. Daily data on CDS pricing is aggregated from the Historical Pricing function in Bloomberg. The data are five-year on-the-run CDS spreads for sovereign bonds denominated in USD. A CDS is a contract between two market participants in which the seller assumes the risk of loss given default or restructuring in exchange for an annual premium, similar to an insurance contract. The quoting convention for CDSs is the annual premium payment as a percentage of the notional amount of the reference obligation. Sovereign CDS spreads are reported in basis points, with one basis point equal to $1,000 to insure $10 million in debt.\(^1\)

Sovereign CDS contracts grew immensely in popularity during the most recent financial crisis as investors began to worry about the financial solvency of certain nations in addition to corporations.

Now, CDS contracts are a common instrument used by market participants to assess the default risk of sovereigns given they operate within a standard framework and are comparable across countries and time. Considering I wish to determine

\(^{1}\) For example, a spread of 350 basis points for a 5-year tenor means that it costs $350,000 to insure $10 million in sovereign debt for 5 years. 3.5% of the notional amount must be paid each year, so the premium, then, is 
\[
.0035 \times 10,000,000 = 350,000
\] per year.
the effect of credit rating announcements on CDS spreads, this dependent variable will ultimately be expressed as the monthly change in basis points.

Table 1 reports summary statistics on CDS spreads for the countries in my sample, showing country mean, median, standard deviation, minimum, maximum, and number of observations. The countries in my sample comprise 32 member countries of the Organization for Economic Co-operation and Development (OECD). There is significant variation across all metrics in this sample. Greece is the only nation in this sample which suffered a “credit event,” meaning partial or full default. This caused extreme volatility in the change in monthly CDS spread in 2012. Figure 1 graphs the evolution of Greece’s CDS spread over the sample period marking each credit movement. Even qualitatively, it is possible to see the correlation between rating change announcements and variation in the CDS spread, yet it is unclear from this figure if the rating change announcements precede or follow changes in spread. Figure 2A demonstrates the degree to which Greece overshadows the variation in all other countries. Considering this and considering that the high variation in its CDS spread can be reasonably explained by the credit event, Greece is ultimately left out of the sample. Figure 2B shows the variation in monthly change across all other countries in the sample. There is clearly a moderation in volatility around 2014 which likely reflects recovery from the financial crisis.

Historical credit rating data is sourced from the Sovereign Credit Rating function in Bloomberg for each country in the sample. Ratings are compiled from two top rating agencies, Standard and Poor’s and Fitch, which both use an ordinal-alphabetic scale to indicate the credit-worthiness of a country. In their explanation of credit ratings, Standard and Poor’s reports that risk of default is the most important factor in the rating decision. There are a variety of other factors considered but they are all ultimately linked to a debtor’s perceived ability to repay obligations in full. Standard and Poor’s states that the five most important factors considered in sovereign credit ratings are: institutional and governance effectiveness and security risks, economic structure and growth prospects, external liquidity and international investment position, fiscal performance and flexibility (including debt burden), and monetary flexibility. (Standard and Poor’s, 2012)

I employ the same numeric rating scale to the sovereign ratings used by Aizenman et al. (2013b), which applies a rank between 1 and 25 to each rating. Investment grade countries rated “AAA” by Standard and Poor’s or Fitch have the highest rating of 25 while a “D” rating, indicating full or partial default on sovereign debt obligation, yields a 1. There is a fair amount of variation amongst the countries in my sample. Only one country, Denmark, had no rating changes from either agency between 2007 and 2018. Greece has by far the largest number of rating movements at 43, while Ireland and Iceland are the next highest with 23 rating events each. Considering OECD countries are largely considered “developed,” the share of investment grade countries in the sample is high, but there is still a range of average ratings.

4. Empirical Results and Discussion
4.1 Baseline Specification
I first test the basic specification, which is the effect of sovereign credit rating movements on changes in CDS spreads, adding control variables to test the robustness of the initial result. I estimate with a dynamic panel regression for 31 OECD countries with monthly data from February 2007 to January 2018. The estimation equation is of the form:

\[ \Delta \text{CDS}_{it} = \beta_0 + \beta_1 \text{CDS}_{i,t-1} + \beta_2 \Delta \text{CreditRating}_{it} + \beta_3 \text{Controls}_t + \epsilon_{it} \]

where \( \Delta \text{CDS}_{it} \) is the change in average credit default swap spread in basis points for country \( i \) in period \( t \), \( \Delta \text{CreditRating}_{it} \) is the change in the numeric credit rating variable, and \( \epsilon_{it} \) is an error term.

In order to account for the inherent endogeneity between the lagged CDS value and the error term, I use the Arellano

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2 Three countries (United States, Canada, Luxembourg) were omitted due to data unavailability.

3 During the greatest periods of volatility, Greece received rating upgrades and downgrades in the same month. This was a further limitation to including Greece in the analysis as the numeric change in rating is predicated on an average monthly change. Computing the average change when there has been movement in opposite directions simply mutes the effect in this framework.
and Bond (1990) generalized method of moment (GMM) approach. This is a dynamic panel regression which generates instruments from further lagged variables of the dependent variable (the number of lags is determined by $T_i - p - 2$).\footnote{As an example, consider a simple panel with only one independent variable and $t = 10$ periods where observables and unobservable are correlated. In order to solve this problem we can turn to first differences, yet this leads to a new endogeneity issue where a regressor (the lagged value of our dependent variable) is correlated with the error term. This problem is easily solved with the IV method and luckily Arellano Bond identifies further lagged values to be a suitable instrument. Given there are $T_i - p - 2$ moments generated there will ultimately be 36 GMM-type instruments in addition to the instruments from the independent variable and the constants term for a total of 38 instruments.} Luckily, this specification allows for the introduction of other endogenous right-hand side variables, which account for possible endogeneity of credit rating movements. This method was designed for a large $T$ and small $N$. The opposite case, like in this sample, can lead to overidentification of the lagged dependent variable. However, the results from this method are nearly identical to the static panel model with fixed effects, suggesting the resulting coefficient is accurate and not subject to overidentification. I report robust standard errors to correct for potential autocorrelation and heteroscedasticity.

The control variables included in this analysis are those which were consistently significant for Aizenmen at al. (2013b). This includes a variable which captures the monthly change of a general commodities index, monthly average crude oil price, and monthly average of the VIX index.\footnote{VIX is the ticker symbol for the Chicago Board of Options Exchange volatility index. It is constructed using a range of implied S&P 500 index options and as such, meant to be a forward-looking measure of expected volatility over 30 days.} In lieu of adding new control variables, I chose to include the same variables which were previously significant predictors to understand if there has been a shift in CDS pricing over time.

I report the results of the baseline specification in Table 4. Unsurprisingly, the lagged value for change in CDS pricing is highly significant. In terms of credit rating movement, the results suggest a 1 point increase on the credit rating scale decreases CDS spreads by approximately 7 basis points in the same month. While the results are statistically significant and robust to the addition of control variables, this certainly differs from the parallel result in Aizenman et al. (2013b). They report a decrease between 42 and 46 basis points for an identical shift in rating value. This is the first instance of “moderation” of the effect of credit rating movement on CDS spreads in this later time-period. Additionally, of note: for this new period, crude oil prices are no longer a significant predictor of CDS pricing. This could be due to the trend of decreasing oil prices, which causes market participants to focus attention towards other information with potentially greater ability to reduce financial solvency.

### 4.2 Regional and Temporal Variation

Table 5 reports results for the baseline specification with all controls segmented by three regional groups and two time periods. Hibbert et al. (2017) and Aizenman et al. (2013b) suggest credit rating movements are particularly significant for countries in the Eurozone and the European Union. This is understandable given the severity of the Eurozone Crisis, which was accompanied by frequent rating movements and fear of sovereign default. Figure 1b shows the variation in monthly CDS spread for countries in the sample. The line in the figure separates the sample into two periods of varying volatility. Although my sample includes newer data, I am still able to capture the turbulence of the financial crisis. By including this temporal analysis, I attempt to explain the relationship between credit rating changes and CDS spreads in recent years after financial markets had largely recovered from the crisis.

This analysis yielded interesting results based on the region, time-period, and combination of both metrics. Previous findings are supported by my results, as the coefficient for change in credit rating decreases in absolute value from the Eurozone to the E.U. to the entire sample. In regressions prior to 2014, crude oil price is a significant predictor of change in CDS pricing; however, this does not hold for any region in the later period, or throughout the sample overall. This serves as further evidence that while crude prices at one time were a significant driver in CDS spreads, the pricing convention has strayed away from this metric.

I did not find strong results for regressions in the post-2014 period. A possible explanation is the trend of credit rating
movements during this period. As countries recovered from global financial turmoil, rating upgrades were more common. The ratio of upgrades to downgrades in this period is roughly 2.50, while the same ratio is roughly 0.40 for the entire sample. As stated, previous literature has found that CDS prices are more responsive to negative rating movements than positive movements.

4.3 Leading or Lagging Credit Rating Movements
Theoretically, CRAs provide information regarding credit worthiness contemporaneously, along with other market factors that measure risk. However, this principle is easier said than done, considering agency analysts must aggregate the necessary information and issue a rating to reflect changes in the underlying criteria in a timely manner. Ideally, credit rating agencies would issue credit rating movements prior to market repricing of risk, thus adding value to the body of information which informs sovereign risk factors.

While the results are not very robust, contemporaneous effects are statistically significant and robust, with a similar change in approximately 7 basis points for a one point increase in credit rating. Leading effects, suggesting credit rating movements can explain variation in CDS spread of the previous month, are significant at the 10 percent level when regressed on their own including controls. It appears that leading values are more significant than lagging values, which could potentially suggest that overall CRAs release information behind the market.

An important limitation to this analysis concerns the release of credit rating changes. Change in CDS prices are averaged monthly, while rating announcements are released periodically on random dates. Due to this, some rating changes are announced on the 1st of a given month, while others are announced on the 31st of another. At a glance, the variation in announcement date appears random and hence should not affect the analysis regarding monthly change in CDS spread. However, for this analysis, higher frequency data may be more suitable to understand the response of sovereign CDS spreads immediately before and after a credit rating movement.

4.4 Discussion
This analysis was initially motivated by extending the work of Aizenmen et al. (2013b) to update the time period and panel of countries in the study. Unsurprisingly, the qualitative economic significance of credit rating movements on change in CDS pricing has not changed in the updated period; however, the size of the coefficient has decreased. It appears the effect of credit rating movements is strongest in the Eurozone, which can likely be attributed to the lingering debt issues of GIIPS and other nations who suffered contagion effects during the crisis. Earlier in the period, crude oil was a significant predictor of monthly change in CDS pricing; however, it no longer serves the same purpose. While there is some evidence to suggest that rating agencies issue ratings behind market repricing of sovereign risk, it is safer to say that agencies do not predict market repricing, based on the statistical insignificance of this constructed variable.

There are a number of important questions arising from this analysis. What is currently driving the change in monthly CDS pricing? It is clear that credit rating movements play a role even now, but that role appears to be diminishing. Volatility rates in sovereign CDS spreads are beginning to approximate volatility rates during the great moderation. Similarly, if market volatility is lowered, there will be fewer rating movements as well.

Considering the qualitative nature of credit ratings, there are many additional ways one could analyze them. There is evidence in prior literature that suggests poorly rated sovereigns are more strongly affected by a rating movement in either direction. Additionally, certain rating thresholds determine important classifications such as “investment grade” granted to bonds rated “BBB+” or higher. It is possible there would be a large impact on CDS spreads if a credit rating were to move in either direction across this threshold. These analyses may be more fitting in a group of developing countries, which frequently have low ratings.

Finally CRAs issue additional information regarding credit ratings in the form of “watch list” notices. If an agency is speculating about a rating change, they will typically indicate this by releasing a positive or negative “outlook” announcement. I chose not to incorporate these notices to streamline analysis, but there is potentially valuable predictive power in these
announcements, which could significantly affect sovereign CDS spreads. Further analysis on this topic could include the severity of impact based on current rating and the gap in time between watch list announcement and credit rating movement, if one ultimately occurs.

5. Conclusion
The pricing of sovereign risk has important implications for a nation’s ability to obtain external capital. One of the primary ways market participants evaluate this risk is through the sovereign credit ratings of top CRAs. Considering the serious economic stakes involved, it is important to understand the impact that credit ratings may have on the key thing they are meant to explain. Aizenman et al. (2013) study the effect of credit rating changes on sovereign CDS spreads for European countries. Updated analysis suggests that the relationship still holds but is decreasing in economic significance. Aizenman suggests the Euro Zone Crisis ushered a new paradigm for sovereign CDS spreads; however, based on updated analysis, it appears that historical trends may be returning, and the new paradigm may simply be an outlier based on heightened default risk globally.
References
Appendices

Figure 1. Monthly CDS and S&P Movements: Greece

Source: Bloomberg, author’s calculation

Figure 2A. Sample Variation of Monthly ΔCDS, including Greece

Source: Bloomberg, author’s calculation
**Figure 2B.** Sample Variation of Monthly ΔCDS, excluding Greece

![Graph showing variation of monthly ΔCDS excluding Greece](image)

**Table 1.** Descriptive Statistics for Sovereign Credit Default Swap (CDC) Spreads

<table>
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<tr>
<th>Country</th>
<th>Mean</th>
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<th>SD</th>
<th>Max</th>
<th>Min</th>
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Source: Bloomberg, author's calculation

Notes: Table reports statistics on monthly average for five-year Sovereign CDS contracts between February 2007 and January 2018. Canada, Luxembourg and United States omitted due to data unavailability. CDS spreads are measured in basis points.

### Table 2. Linear Scaling of Credit Ratings

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Source: Bloomberg, Aizenman et al. (2013b)
Table 3. Average Sovereign Ratings by Agency

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Source: Bloomberg, author's calculation
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Notes: GMM Arellano-Bond dynamic panel estimates. *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors reported.

Table 5. Regional and Temporal Variation, Credit Ratings and CDS Pricing

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Notes: GMM Arellano-Bond dynamic panel estimates. *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors reported.

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Notes: GMM Arellano-Bond dynamic panel estimates. *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors reported.
Public Capital Investment: Does Infrastructure Spur Growth?

SEBASTIAN SENLLE
University of Buenos Aires

ABSTRACT
This work analyzes the role of public capital investment (associated, mainly, to infrastructure) as one of the determinants of economic growth. It goes (departing from a literature-review) through possible positive transmission mechanisms, related to a greater productivity on other factors of production, a crowding-in effect on private investment and a spur on export-capacity. Using growth-accounting and panel-data-estimation techniques for a 70-country sample in the 1990-2014 period, I find evidence of a significant and positive contribution of the public-capital-stock growth rate on the output growth rate. Effects are persistent over time (considering 1-year and 2-year lags). Evidence suggests that contribution is particularly high in developing countries, which lag behind both in quantity and quality of public capital relative to high-income economies and suffer from bottlenecks arising from their lack of adequate infrastructure.
1. Introduction
The link between public investment in infrastructure and economic growth has long been discussed in literature, providing arguments (and describing transmission mechanisms) that establish causality from public investment to acceleration in the rate of growth.

Many development programs, encouraged by governments all over the world or by multilateral lending agencies, have taken this relationship as its theoretical basement, concluding that infrastructure represents “the wheels – if not the engines – of economic activity” (World Bank, 1994).

After this introduction, the article goes on with a literature review, detailing lines of reasoning that support and deny the existence of a positive causality between public capital investment and economic growth.

Even though the definition of public capital includes investment in equipment and building of administrative centers, schools, and hospitals, for the sake of extension and availability of cross-country data I will focus on infrastructure, understood as transport networks and energy investments.

The third section, following Romp and De Haan (2005), develops an empirical growth-accounting exercise, evaluating the effect of public capital investment on the growth rate.

In the fourth section, we introduce a panel data analysis evaluating the effect of the public-capital-stock growth rate (lagged one year, as we assume that many investment projects take a while to get completed) on the economic growth rate. Finally, the fifth and last section displays the concluding remarks and introduces some policy-recommendations as well.

2. From infrastructure to growth: main transmission mechanisms
2.1 Positive transmission mechanisms
The main transmission mechanism that contemporary literature has identified to link public investment and economic growth has been its positive effect on factor productivity. This approach assumes that aggregate output can be described through an aggregate production function, where public capital comes in as a productive factor together with private capital and labor. Consequently, an increase in public capital stock, ceteris paribus, will raise marginal productivity of the other factors (Ágenor and Moreno Dobson, 2006).

Additionally, infrastructure has been listed as one of the main determinants of “export capacity” and external competitiveness (UNCTAD-UN, 2005). Nordas and Piermartini (2004) conducted a survey for the World Trade Organization, focusing on the issue considering infrastructure deficiencies as part of the transaction costs of exporting. Accordingly, an improvement in the availability of infrastructure would be equivalent to a reduction in exporting costs, through the concurrence of several simultaneous mechanisms:

- Lower costs in transportation of goods, telephone or digital communication with customers and suppliers, business trips, etc.
- Lower time-related problems, such as delays in the delivery of products due to poor infrastructure conditions (for example, a road closed after heavy rains), and lower uncertainty regarding shipment and delivery conditions.
- New urban areas with access to productive chains resulting from the availability of new roads or ports.

My own estimates seem to confirm the intuitively positive link between a greater availability of infrastructure and a higher export capacity. I evaluated a sample of 70 countries, expressing their per capita exports in constant 2011 dollars, according to the CIA World Factbook, as a function of the punctuation scored in the Global Competitiveness Report (GCR)-Infrastructure Pillar from the World Economic Forum in its 2016-2017 edition. For the sake of clarity, scores were reconstructed on a 1-to-100 scale, where 100 is the ideal punctuation.

For the sample as a whole, I find evidence of a positive and significant correlation between per capita exports and the
quality of infrastructure, measured through the punctuation scored by each country in the “Overall infrastructure quality” item from the GCR.

**Chart 1.** Exports as a function of score in infrastructure quality

![Chart showing the relationship between exports and infrastructure quality](chart.png)

*Source: Own estimates based on the World Economic Forum, CIA and UN data*

Intuition behind these estimates has been empirically treated in many papers. Limao and Vernables (2001) find evidence that infrastructure explains up to 40% of transport costs in the exports of coastal countries and up to 60% in landlocked countries. In a paper released for IADB, Molina and Heuser (2016) find evidence of a positive linkage between infrastructure quality and export performance for Pacific Alliance members. They estimate that a 1% reduction in the ad-valorem shipping costs would result in an increase in exports that ranges from 1.3% for Mexico up to 4.5% for Chile.

In recent years, different alternative mechanisms have been proposed. It has been suggested that a reduction in transport costs among states from a country or between countries in a region produces agglomeration-economy effects, making it possible to enlarge the size of the market and to create specialized clusters which exploit scale benefits (Krugman, 1991).

It has also been documented that there are indirect positive effects of infrastructure quality on labor productivity (Ágenor and Neanidis, 2006) by providing better transport conditions and better access to electricity and the Internet.

Good infrastructure quality is also one of the determinants of private capital durability, making it less exposed to deficient roads or electric blackouts (Ágenor and Moreno Dobson, 2006). Thus, investment in public capital may reduce private expenditure in capital maintenance, allowing it to be reallocated to investment.

### 2.2 Negative transmission mechanisms

As commented on in the introduction, literature has not reached a consensus over this issue. While many papers support the idea of a positive association between public capital investment and growth, some others reject it.

The most frequently-commented on negative mechanism is the alleged “crowding-out” effect, where a greater public investment reduces private investment, particularly via higher interest rates. We should note that this mechanism, if valid, would not be exclusively for infrastructure investment, but relevant to any expansive fiscal policy. Warner (2014) finds evidence supporting the crowding-out hypothesis, pointing out that public-investment booms are correlated to a lower share of private investment on GDP.
Some other articles focused on potential incentives for corruption that arise from public capital spending. Explanations point to principal-agent problems and rent-seeking behavior on public officials. It has been found that a higher capital expenditure is associated to more extended corruption (Tanzi and Davoodi, 1997). Among the several mechanisms that were proposed, we highlight incentives for corruption in agents responsible for project-selection (with the risk of firms paying bribes to get hired or to make the design of the project bigger and more expensive than necessary) and for progress-control (firms paying bribes for carrying out the project with lower-quality raw materials or getting extended delivery terms). Incentives for corruption may also arise when choosing location of projects: frequently, the geographical allocation is not selected on efficiency grounds, but on political preferences (it may be picked to favor some Mayor or Governor, to benefit the official`s homeland or even to valorize their own private property).

Consequently, the scenario is one where those countries that devote relatively more resources to public investment (either as a share of total public expenditure or as a share of GDP) end up with a bigger and more complex than necessary public capital stock, which additionally happens to be incorrectly located and built on low-quality raw materials. Their structure of the government becomes more prone to corruption as well. Tanzi metaphorically says that these countries end up producing “white elephants and Cathedrals in the desert”.

Apart from corruption, other papers have also focused on incapacities that the state may have which would affect the average public capital productivity. The effect would be more adverse in developing countries, where public sector operates in a weaker institutional environment and suffers from more operative deficiencies. Such incapacities may be related to failures in selection criteria, which lead to the selection of poor-quality projects with little productivity gains (Warner, 2014).

These arguments call into question the idea that capital investment is necessarily more productive than current expenditure as an option for fiscal policy. Gosh and Gregoriu (2008) find a significant negative relationship between the share of capital expenditure and the growth rate, while the current expenditure (particularly Operation and Maintenance) is positively correlated to growth, for a 15-country panel-data sample over 28 years.

3. Growth accounting: estimating the contribution of public capital
3.1 Motivation
Growth accounting exercises were introduced by Robert Solow in 1957 and are intended to decompose the contribution to growth of each productive factor. Traditionally, those exercises assume a Cobb-Douglas production function (which allows for intermediate substitution possibilities). Labor and capital are assumed to be the productive factors, with no separation between public and private stock.

Nevertheless, several authors have attempted to distinguish between the contribution of public and private capital. Aschauer (1989) estimated that output elasticity of public capital stock ascended to 0.35 for 1949-1985 in the US. Munnell (1990) also found a positive output-elasticity of public capital, but it was lower than the one calculated by Aschauer using state-level data. On general grounds, most subsequent studies have found evidence of a positive contribution, but it seems to be lower than in Aschauer’s work (Romp and De Haan, 2005).

Inversely, Hulten and Schwab (1991) found this relationship is not significant for the USA in the twentieth century, and it becomes negative after controlling for the tax rate imposed to finance higher public investment. The authors explain that infrastructure in this country is already well-developed, thus resulting in low productivity marginal gains.

In this section, I will try to get new estimates, considering a broader sample of countries for a 24-year period (1990-2014). I aim to evaluate the direction and intensity of the relationship between the rate of growth of the public capital stock and the rate of economic growth.

3.2 The Model
Formally, I will suppose that a country`s GDP can be represented through an aggregate production function where public
Capital stock is one of the relevant productive factors. Therefore, the productive factors considered will be called KG (public or “government capital”), KP (private capital), and labor (L). “A” will stand for the Total Factor Productivity component. For the sake of clarity, I will assume the function takes the form of a Cobb-Douglas relation, such that:

\[ Y = A(K_G^\alpha L^\beta K_P^\theta) \]

where \( \alpha < 1, \beta < 1 \) and \( \gamma < 1 \).

The supposition of \( \alpha < 1 \) implies that public capital stock exhibits decreasing marginal returns, given by the first derivative:

\[ dy/dK_G = \alpha AL^\beta (K_G^\alpha K_P^\theta) = \alpha AL^\beta K_P/(K_G^\alpha) \]

For turning the aggregate production function into an economic-growth function, I operate with logarithms on both sides and then work with logarithm properties, getting to:

\[ dY/Y = C + \alpha (dK_G/K_G) + \beta (dL/L) + \theta (dK_P/K_P) \]

Considering that the differential of each variable divided by the variable itself can be interpreted as a growth rate, we express as follows:

\[ \hat{Y} = C + \alpha \hat{KG} + \beta \hat{L} + \Theta \hat{KP} \]

Where \( \alpha, \beta \) and \( \theta \) are interpreted as output-elasticity of public capital, labor, and private capital respectively, \( Y \)-hat is the annual growth rate of output, \( L \)-hat is the annual growth rate of labor, \( KG \)-hat is the annual growth rate of public capital stock and \( KP \)-hat is the annual growth rate of private capital stock.

### 3.3 Sources

The detail of countries considered is listed in the Appendix. Data about labor is taken from Penn World Tables version 9.0 and is calculated as “number of persons engaged”, meaning the number of active workers (millions of persons).

As regards capital, Penn World Tables estimate the stock for each country in each year in 2011 constant dollars, but do not distinguish between public and private property. Thus, I will take the “Investment and Capital Stock dataset” released on January 2017 by the IMF to calculate the share of public or private capital that is assigned to each country yearly.\(^1\)

### 3.4 Results

The results exhibit a positive and statistically significant contribution of the public-capital growth to output growth for the sample in the 1990-2014 period.

Estimator \( \alpha \) of output-elasticity of public capital is 0.1917, a positive value but lower than Aschauer’s, in line with the literature mentioned above. A one percent increment in the public capital growth rate adds an additional 0.1917 point to the output growth rate.

Estimators \( \beta \) and \( \theta \), corresponding to private capital and labor, also take positive values and are individually significant, confirming our a priori expectations.

---

1 IMF dataset divides capital stock into three categories: private capital, public capital and PPP capital (public-private partnership). I will consider the last two groups as “public capital stock”, as PPP projects are usually devoted to infrastructure purposes, responding to government proposals.
Table 1. Decomposition of Growth for the Complete Sample

<table>
<thead>
<tr>
<th>ESTIMATOR</th>
<th>VALUE</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
</tr>
<tr>
<td>C</td>
<td>0,3450***</td>
</tr>
<tr>
<td></td>
<td>(0,2141)</td>
</tr>
<tr>
<td>α (output elasticity of public capital)</td>
<td>0,1917*</td>
</tr>
<tr>
<td></td>
<td>(0,0445)</td>
</tr>
<tr>
<td>β (output elasticity of labor)</td>
<td>0,4950*</td>
</tr>
<tr>
<td></td>
<td>(0,0476)</td>
</tr>
<tr>
<td>θ (output elasticity of private capital)</td>
<td>0,2855*</td>
</tr>
<tr>
<td></td>
<td>(0,0433)</td>
</tr>
<tr>
<td>N (countries included)</td>
<td>68</td>
</tr>
<tr>
<td>Observations</td>
<td>1,582</td>
</tr>
</tbody>
</table>

R2: 0,3260. Adjusted R2: 0,3246 DW-Stat: 1,6041. Prob(F-statistic): 0,0000

Note: standard error is indicated in brackets
* implies significance at 1% / ** implies significance at 5% / *** implies significance at 10%

Complementarily, I carried out several specifications of the model. Firstly, by dividing the sample of countries according to their level of income, I find the positive effect is particularly high in developing countries - the ones with upper-middle income levels – where the estimator gets the value of 0.3531, which is consistent with the reviewed mechanisms. Secondly, the effect shows that it is persistent through time: when I introduce lags in , output-elasticity of public capital remains positive and significant. However, this effect decreases as the lag increases. One additional percentage point of increment of public capital stocks adds 0.1917 percentage points to output growth in the same year; 0.0783 points the following year, and 0.0484 after two years.

Finally, results remain robust when changing the period considered. Yet the positive effect of the investment in public capital is remarkably higher (it gets doubled) in the 2001-2014 period relative to the first ten years considered in the sample (1990-2000).

4. Growth Regressions
4.1 Motivation
Panel data has frequently been used in the literature in order to identify potential determinants of growth. Among the determinants that were suggested from the literature, we choose exchange rate undervaluation (Rodrik, 2008), direct foreign investment (De Mello, 1999; Carkovic and Levine, 2002), convergence to steady-state levels (Islam, 1995), low inflation combined with trade openness (Dewan, 2001) and even carbon dioxide emissions (Narayan, 2010).

In this paper, I will aim to evaluate, using panel data techniques, the traits of the relationship between the growth of output as the independent variable and the growth of public capital stock as an explanatory variable.

4.2 Methodology and sources
The sample gathers information from 56 countries in the five selected continents for 1990-2014. A balanced panel was built, listing the GDP growth rate and the growth rate of public capital stock for each country. I added some control variables that vary for each nation on each year (inflation rate, investment rate, share of government consumption, and the Score in Human Capital Index, which takes returns to education and average schooling years into account).

Additionally, I included variables that change for each country but not over time (for instance, the initial per capita public capital stock) and others that are invariant in time but are common to all countries in each year (for instance, the global output growth rate).

Information comes from Penn World Tables 9.0, except for inflation rates, which were taken from the World Development
Indicators (World Bank). Additionally, I used the IMF Investment and Capital Stock Dataset (2017) to obtain the share of public and private capital for each country in each year.

Following methodology suggested by Baronia and Vianco (2014), I developed a general model of fixed effects estimating by ordinary least squares (OLS), considering cross-section effects (or “country” effects) and period or “time” effects.

There were six specifications carried out, considering successively further control variables, with a specification similar to the following:

$$\hat{Y}_{i,t} = \alpha \hat{G}_{i,t-1} + \phi \hat{Y}_{i,t-1} + \beta X_{i,t} + \Theta Z_i + \Omega W_i + U_{it}$$

where \( \hat{G}_{i,t-1} \) stands for the growth rate of public capital stock with a one year lag, \( X_{i,t} \) is a vector of variables that change over country and over time, and \( \hat{Y}_{i,t-1} \) is the growth rate of each country lagged one year.

\( Z_i \) stands for a vector of variables that change for each country but are invariant in time. \( W_i \) stands for a vector of variables that change over time but are common in each year for every nation. \( U_{it} \) is the error term.

As we are considering a fixed effects model, the error term can be expressed as:

$$U_{it} = u_i + v_t + W_{it}$$

where \( W_{it} \) stands for the purely aleatory effect (unique for each country and each year), \( u_i \) represents a fixed country effect, and \( v_t \) represents a fixed time effect.

The aim of evaluating \( \hat{G}_{i,t-1} \) considering a one-year lag is to catch the contribution of public capital to growth more purely, considering the long periods required to build up the assets that constitute growth.

As explained in the motivation of this paper, our first focus is to determine the value the of estimator \( \hat{a} \), which captures the effect of the increment in the public capital stock and its significance.

### 4.3 Results

The results I got confirm, in all the specifications, that the rate of growth of public capital stock (lagged one year) contributes positively and significantly to the output growth rate.

<table>
<thead>
<tr>
<th>Table 5. Results for Different Specifications of Growth Regressions</th>
</tr>
</thead>
<tbody>
<tr>
<td>Independent variable: Output growth rate</td>
</tr>
<tr>
<td>------------------------------------------</td>
</tr>
<tr>
<td>C</td>
</tr>
<tr>
<td>K(t-1)</td>
</tr>
<tr>
<td>Human Capital Index</td>
</tr>
<tr>
<td>Inflation</td>
</tr>
<tr>
<td>Y(t-1)</td>
</tr>
<tr>
<td>(G/Y)</td>
</tr>
</tbody>
</table>
If we analyze the variable $\hat{K}$ simultaneously with the output growth rate (i.e. with no lags), the effect remains positive and significant, but the estimator gets higher in value. In that case, its assumed effect is reinforced by the traditional mechanism of direct stimulus to aggregate demand, which is inherent to every expansive fiscal policy.

5. Conclusion

In this work, I aimed to examine the relationship between infrastructure investment and economic growth.

Literature review allowed me to address a number of possible mechanisms that would explain the positive contribution of infrastructure to growth. Exercises of growth accounting and the use of panel-data methodology for a broad sample of countries in the period 1990-2014 confirm this positive contribution, which remains significant if we consider lags in the explanatory variable. Evidence suggests that positive effect is particularly strong for middle-income countries.

It was found, as well, that export performance (measured as exports per capita) appears as an outcome that is correlated with the incidence of satisfactory availability of infrastructure. This point is especially interesting for Latin America, as many believe that the region suffers from a relative backwardness, not only compared to developed countries, but also to other blocs of emerging countries (such as the nations from Southeast Asia or Eastern Europe) in terms of scores in land, port and airport connectivity.

Having verified the positive association between a larger investment in public capital and more rapid growth, my policy recommendation argues that infrastructure investment should be in the core of any development program in emerging countries.

For Latin America as a whole, managing to get over the relative backwardness in infrastructure and catching up with other emerging blocs seems to be one of the most difficult challenges for embarking in sustainable growth cycles, particularly considering that the region has historically been affected by chronic problems related to the insufficiency of its exports.
References


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IMF, Regional Economic Outlook (2016). “Western Hemisphere: Managing Transition and Risks”.
Kaldor, Nicholas (1967). “Strategic Factors in Economic Development”, Cornell University
World Bank, World Development Indicators (available at: https://data.worldbank.org/data-catalog/world-development-indicators)
## Appendices

The countries included in the sample are listed below. They are classified according to its level of income and the geographical region where they belong.

<table>
<thead>
<tr>
<th>Region</th>
<th>Level of Income:</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>High</td>
</tr>
<tr>
<td>Latin America and Caribbean</td>
<td>Chile</td>
</tr>
<tr>
<td>North America</td>
<td>Canada, USA</td>
</tr>
<tr>
<td>Western Europe</td>
<td>Belgium, Denmark, Finland, France, Germany, Ireland, Italy, Luxembourg, Netherlands, Norway, Portugal, Spain, Sweden, UK</td>
</tr>
<tr>
<td>Eastern Europe</td>
<td>Estonia, Hungary</td>
</tr>
<tr>
<td>Africa</td>
<td>South Africa</td>
</tr>
<tr>
<td>Asia</td>
<td>Hong Kong, Israel, Japan, Qatar, Saudi Arabia, South Korea, Singapore, Taiwan, United Arab Emirates</td>
</tr>
<tr>
<td>Oceania</td>
<td>Australia, New Zealand</td>
</tr>
</tbody>
</table>

This sample is used all throughout the article many times: in section 2, to estimate relationship between infrastructure, exports and GDP per capita; in section 3 for growth accounting exercises and in section 4 for panel-date growth regressions.

Note: in the growth accounting exercise, I excluded from the sample China and Vietnam because, given its socialist condition, separation between public and private property of capital in national accounting does not respond to the criteria we aim to evaluate.

Note 2: In the panel data exercises, I also dispensed with countries like Croatia, Ukraine, Russia and Estonia as information is not complete for the whole period (they became independent after the initial date). I aimed to keep a balanced panel data.
FDI, Political Risk, and Inequality in Host Countries

TUYET-ANH TRAN
Macalester University

ABSTRACT
This paper uses a panel of 28 developing and developed countries from 2009 to 2014 to study whether the interaction effect between inward FDI and political risk is substantial in determining how inward FDI impacts income inequality. I employ a fixed-effect model to find that FDI has a statistically significantly negative effect on inequality both with or without the interaction effect. In particular, a 1% increase in the amount of inward FDI leads to a reduction of 0.03 points in the GINI index, or 0.06 points if we account for the interaction term. However, the interaction term is not significant. The results are robust to several checks.

1 I owe my special thanks to professor Amy Damon for her dedicated instruction and valuable advice, and to Eleanore Fuqua and Johannes Davies for their precious comments. Any errors are the responsibility of the author.
1. Introduction
Over the last few decades, foreign direct investment (FDI) has significantly increased around the world. Although FDI brings benefits to host countries, how the benefits are distributed among citizens of the host countries is not clear (Figini and Gorg, 2006). Many previous studies have tried to investigate the connection between FDI and income inequality, and they have come to various conclusions. Some papers find the different impacts of FDI on income gap across time span (Herzer and Nunnenkamp, 2011; Cho and Ramirez, 2016). Other empirical works have observed regional heterogeneity in the relationship between FDI and income disparity (Chintrakarn, Herzer, and Nunnenkamp, 2012; Figini and Gorg, 2006).

This paper contributes to the ongoing discussion by studying whether the interaction effect between inward FDI and political risk can impede positive effects of FDI on inequality. To my knowledge, this is the first research that studies how the interaction effect impacts inequality. The motivation for this research question comes from the research by Wang and Blomstrom (1992) and Herman, Chisholm, and Leavell (2005). The papers argue that a country’s unstable political circumstances are likely to hinder technology transfer. Since technology transfer is an important channel through which FDI affects inequality (Taylor and Diffield, 2005), this may imply that the interaction between political instability and FDI plays an important role in determining how FDI affects inequality.

My main finding is that a 1% increase in the amount of inward FDI leads to a reduction of 0.03 points in the GINI index, or 0.06 points if we account for the interaction term. I find that the coefficient of the interaction term is positive and jointly significant with inward FDI. However, it is individually insignificant, and its sign and magnitude may be imprecisely estimated as the interaction effect seems to be multicollinear with the fixed effects given the data. The results are robust to several checks.

2. Theory
The theoretical framework adopted in this paper is a supply-demand model for relatively high-skilled labor. Figini and Gorg (2006) claim that wage distribution importantly implies income inequality because wage is the major source of income for a large portion of the population. Following the work by Figini and Gorg (2006), I assume that wage and income inequality are the same. Furthermore, in this supply-demand model, workers are homogenous within each skill group. Thus, the source of inequality in this model is the difference between the wages of high-skilled and low-skilled labor. Theoretically, FDI increases demand for relatively skilled labor in both multinational enterprises (MNEs) and domestic firms. Therefore, the demand curve in this model shifts right because of inward FDI. On the supply side, MNEs can help raise the supply of skilled workers in the host country, as MNEs induce skill upgrading via technology spillovers, job training, and individual support for the host country’s educational institutions (Slaughter, 2002). In the long run, FDI promotes skill acquisition because it raises the wage of skilled labor, which encourages people to become skilled labor, and encourages the host country to improve educational quality. Thus, the supply curve also shifts to the right. Since both demand and supply curves shift, the relative wage is not firmly determined. However, political risk comes into play as a friction to the movement of the supply curve. As suggested by Wang and Blomstrom (1992) and Herman et al. (2005), it impedes technology transfer, which is a determinant of the supply of relatively skilled labor. Hence, we should expect that the supply curve is reluctant to shift right. If this effect is significant, it can offset the impact of FDI on the supply of relatively skilled labor, which means the supply curve will not shift right enough to lower the relative wage. Although the relative wage is not firmly determined within this model, the model suggests that the supply curve moves according to the interaction of inward FDI and political risk.

3. Empirical Strategy
Since I use panel data, there are time-invariant variables that make observations not independent of each other. Therefore, an appropriate regression should be a country fixed-effect model so that we can get rid of time-invariant omitted variable bias. It is evident that some demographic factors, namely political risk, education, and population, partly determine inequality and significantly correlate to inward FDI, so we should control for the variables. Hence, the primary regression is:

\[ \text{Gini index} = \beta_0 + \beta_1 \text{FDI} + \beta_2 \text{Interaction term} + \text{controls} + \epsilon \]

2 The other sources of income are capital gains, pensions, dividends, etc.
\[ \text{INEQ}_{it} = \alpha_i + \beta_1 \text{FDI}_{it} + \beta_2 \text{PR}_{it} + \beta_3 \text{FDI}_{it} \times \text{PR}_{it} + \beta_4 \text{EDUC}_{it} + \beta_5 \text{POP}_{it} + u_{it} \]

where \( \text{INEQ}_{it} \) is the income inequality of country \( i \) at time \( t \) measured by the GINI index, \( \alpha_i \) is the specific intercept level of country \( i \), \( \text{FDI}_{it} \) is the amount of FDI inflows to country \( i \) measured by capital stocks provided by foreign investors, \( \text{PR}_{it} \) is the political risk rating with higher value implying lower risk, \( \text{EDUC}_{it} \) measures the percentage of population attaining secondary education, \( \text{POP}_{it} \) is the country's population size, and \( u_{it} \) is the error term.

The expected sign of \( \beta_1 \) is ambiguous because previous literature provides mixed findings and the theoretical model cannot qualitatively determine the effect of FDI on relative wage. \( \beta_2 \) is expected to be negative. Previous studies find that a country with higher political risk faces more severe income inequality; however, the political risk rating is measured in decreasing order, so a positive relationship between political risk and inequality would suggest a negative coefficient. As suggested by the theoretical model, higher political risk impedes the spillover effect of FDI, which makes the supply curve reluctant to shift right and therefore cannot lower the relative wage. This implies a positive correlation between the interaction effect and inequality, and since a lower index represents a higher risk, \( \beta_3 \) should be negative. Previous literature asserts different conclusions on how education affects inequality, so \( \beta_4 \) is ambiguous. Last, there is evidence that population is negatively correlated to inequality (Campante and Do, 2007); thus, the expected value of \( \beta_5 \) is also negative.

### 4. Data Description

To analyze the research question, I use panel data for 28 countries during 2009-2014 with 168 observations in total. There are 13 developing and 15 developed countries in the data. The country classification is based on the “World Economic Situation and Prospects 2016” report by the United Nations. The countries were chosen because they have the most recent and complete data.

The dependent variable used in this research is income inequality, measured by the GINI index. I use the dataset World Income Inequality Database, which is assembled by the United Nations University - World Institute of Development Economic Research. The original dataset may contain more than one observation for each country in each year, because it assembles data from many sources. For each country, I use observations from a single source across years, so the consistency is maintained. Using STATA to plot the data, I observe that the distribution of the GINI index across 28 countries is bimodal. The peaks correspond to those of two classes of countries, namely developing and developed. The distribution of the GINI index in developed countries is right-skewed, while that of developing countries is nearly normal.

The independent variable of interest is FDI. FDI is measured by annual inward FDI stocks to countries obtained from the “Foreign direct investment: Inward and Outward Flows and Stock, annual, 1970-2015” dataset by the United Nations Conference on Trade and Development. Our sample of inward FDI stocks is right-skewed: Many data points in developing group receive small values and do not vary very much, while inward FDI stocks in developed countries are spread more equally across a wide range. Thus, I need to take the logarithm of FDI to normalize the data distribution.

The three control variables used in this paper are political risk, educational attainment, and population size. Among these three variables, we focus on political risk as our regression includes the interaction term between FDI and this variable. Political risk measures the risk that can affect an investment’s return due to a country’s political changes or instability. I use a 100-point index of political risk whose high value implies a more politically stable country. This index is calculated by the PRS Group, a quant-driven firm specializing in political and country risk forecasting and rating, by considering 17 risk components including turmoil, financial transfer, export markets, etc. Political risk data is mildly left-skewed. I use the “Gross enrollment ratio, secondary, both sexes (%)” dataset from the World Bank Indicator (WBI) as a proxy for higher educational attainment. Since half of our observations are in developing countries where many people do not attend higher education, we may not have enough data if we use the higher education dataset. This data measures the total enrollment in secondary education regardless of age over the population of official secondary education age. For population size, I use the “Population, total” dataset from the WBI. This data is calculated by adding up the number of

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3 More details regarding this variable are discussed in the “Data description” section.
4 Values measured this way may be greater than 100% because over-aged and under-aged students are also counted.
residents in each country regardless of their status and citizenship.

5. Regression Analysis
5.1 Results
As I take the logarithm of inward FDI to normalize its distribution, from now on, I refer to the logarithm of FDI as FDI.

I run the country fixed-effect model with two specifications: The first one is the linear regression, and the second one is the regression with the interaction term between FDI and political risk. My regression outcomes show that adding the interaction term does not change the significance of coefficients, but this term makes the effect of the variable of interest on inequality become clearer. A 1% increase in the amount of inward FDI leads to a reduction of 0.03 points in the GINI index, or 0.06 points if we account for the interaction term. Both are statistically significant at the 1% level. The coefficient of the interaction is positive. It disagrees with our prediction in the “Empirical strategy” section. There are two possible explanations. The first one supports the finding: The benefit of inward FDI on inequality is diminishing in the stability level of countries. Therefore, a more stable country enjoys less benefits from inward FDI than a relatively unstable country. It is analogous to the Solow growth model in the sense that a more developed country will converge to the steady state at a slower rate. The second explanation explains why the finding may not be precise: The values of the interaction term, given the dataset, do not vary much over time, so the interaction term has a high collinearity with the fixed effects; as we are using a fixed-effect model, which takes into account time-invariant variables, the multicollinearity causes the estimation of this coefficient to be imprecise. The t-test confirms that the interaction effect is not significant. However, the F-test of FDI and the interaction term suggests that the variables are jointly significant in explaining inequality as the p-value is smaller than the 1% level. The results imply that the effect of FDI on inequality is truly significant, both with or without the interaction term. We should notice that the coefficients on the control variables do not have causal effects as they pick up the effects of demographic properties on the response variable and the variable of interest. The results significantly suggest that inward FDI tends to relieve inequality.

5.2 Robustness
I challenge my results by several methods: using a time-fixed effect model, constructing separate regressions for developing and developed countries, using a quadratic model, and using a different measurement of inward FDI, namely FDI flows. My results are robust to all alternative specifications.

5.3 IV regression
This paper is trying to investigate how the inward FDI affects inequality, but there is the possibility of reverse causality. Although there is no explicit evidence, Im and McLaren (2015) suggest a scenario where the leaders of the host country with unequal income distribution attract FDI sources to maintain their politically privileged status. Hence, we should be aware that the inequality status of the host country may be able to explain or predict the amount of FDI the country receives. This simultaneous causality makes FDI and the regression error no longer independent.

To address the potential threat, I am going to use an IV model besides the primary regression. To run the IV regression, I no longer use the logarithm of FDI. Instead, I use its raw value, as the instrument that I would use is for FDI, not for the logarithm of FDI. According to Figini and Gorg (2006), lagged differences of FDI are valid instruments for FDI stocks. Lagged difference of FDI of country i at year t is defined as follows:

\[ \text{LaggedDiffFDI}_{it} = \text{FDI}_{i,t-1} - \text{FDI}_{i,t-2} \]

I use the two-stage least squares (TSLS) estimator to estimate the coefficient of FDI. Since FDI is also included in the interaction term, we have two endogenous variables. Thus, we need one more instrumental variable to avoid an under-identification problem. Following a method in Wooldridge (2010), I run the first stage of TSLS without the interaction term to obtain the values of FDI explained by the lagged differences, and then interact these fitted values with political risk to generate the second instrument. I regress the TSLS using the package “ivreg2” in STATA. The signs of the estimated coefficients do not change compared to the primary regression. However, Wald tests indicate that my instruments weakly
identify endogenous regressors.\(^5\) There are two possible explanations for why these instruments do not work well. First, my sample is skewed and not big enough, so we cannot observe a strong identification of endogenous variables using the instruments. Second, it may be the case that while the instrument for FDI works quite well, the manually generated instrument for the interaction term is not good enough.

### 5.4 Limitations
First, although the unexpected positive coefficient of the interaction term can be explained by the diminishing return of inward FDI on inequality subject to the level of political stability, there is a possibility of multicollinearity between the interaction term and the fixed effects that may cause an imprecise estimation on the coefficient of the interaction term. This is likely due to the short time span and the small number of countries in the data that the values of the interaction term are not spread enough to see a substantial change across time. Second, there are threats to the internal validity of this research. The data on education is collected via survey, thus we should be aware of its preciseness. There may be measurement error or missing data. For example, some people may randomly misreport their ages, as birth registration is not enforced in some locations. The source from which I retrieved this data, the World Bank Indicator, provides more details of this data’s limitation. The same problem is applicable to the population data. Also, simultaneous causality could occur. While getting rid of missing data is out of this paper’s scope as I use observational data collected by survey, measurement error and simultaneous causality can be resolved with good instrumental variables for inward FDI and the interaction term. However, the instruments that I use, which are the lagged difference of FDI and a manually generated instrument, fail to be strong instrumental variables. Thus, we cannot eliminate the threats entirely, which means the results are only suggestive, not conclusive.

### 6. Conclusion
This paper uses a fixed-effect model to check if the interaction effect between FDI and political risk has any impacts on the relationship between inward FDI and inequality. My main findings suggest that a 1% increase in the amount of inward FDI leads to a reduction of 0.03 points in the GINI index, or 0.06 points if we account for the interaction term, and the interaction term is not significant. The results are robust to several checks.

However, multicollinearity between the interaction term and the fixed effects and threats to internal validity, such as measurement error, missing data, and simultaneous causality, are issues of concern. This paper attempts to use the IV regression as an alternative specification to resolve the threats to internal validity, but the chosen IVs are not strong enough to effectively get rid of the endogeneity.

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5 I consider the Cragg-Donald Wald test, which assumes i.i.d. errors, and the Kleibergen – Paap rk Wald test, which occurs as I request robust standard errors.
References
PRS Group (2016), *Political Risk Index (PRI) Table*, Internet Posting: https://www.prsgroup.com/category/risk-index
The Effect of Immigration on Political Polarization: Evidence from Europe

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ABSTRACT
I examine whether increased immigration in Europe has contributed to rising political polarization. Analyzing self-reported political values of voting-age adults across European countries from 2002 to 2016, I find that an increase in immigration as a share of the population has a bidirectional polarizing political effect. These effects are intensified by high levels of economic insecurity throughout a country, but damped in more highly educated populations. While these relationships are more ambiguous at the regional level, I find that perceived immigration, if not real circumstances, does combine with economic insecurity to induce political polarization across both European countries and regions.
1. Introduction
Europe has experienced a surge of immigration in recent decades, including an unprecedented influx of migrants arriving during the European refugee crisis of just the past few years. This wave of immigration has ignited anxieties regarding the political, social, and economic consequences of such massive inflows. Baker et al. (2016), analyzing newspaper articles, documented a rise in migration-related fears in the wake of the migrant crisis to levels that were three-to-four times higher than the baseline in many European countries. Over the same period, Guiso et al. (2017) and Halla et al. (2015) describe a swelling of extreme political movements, especially in Europe, many of which espouse protection from immigrants.

The impact of economic shocks on political polarization has often been studied but has yet to be fully comprehended. The specific impact of immigration on polarization is even less thoroughly understood in the existing literature. However, casual observation of the current migratory and political climates suggests that there may be a positive relationship between the two. Indeed, it is plausible that increased immigration may function as an economic shock that creates a sense of resource constraints throughout a population. In that case, individuals may be more inclined to engage in group identity politics, adhering to politicians and political parties who they feel will protect their interests. If extreme political sentiments will flourish in the presence of increasing immigration, and if migration to Europe will continue to flow across the Mediterranean, as Hanson and McIntosh (2016) predict, the prospects for social and political accord in Europe may be grim.

In this paper, I examine whether exposure to increased immigration in Europe has contributed to rising political polarization. I begin by estimating the effect of immigrant population shares on political values of voting-age adults across European countries using individual-level survey data from 2002 to 2016. The results suggest that the impact of immigration on polarization is indeed positive and truly bidirectional, rather than ideologically uniform. Next, I investigate several specific cross-country differences in the link between immigration and polarization, and document that higher levels of economic insecurity fortify the politically polarizing effect of immigration, while higher levels of education attenuate this effect across countries. Then, I focus my analysis to the regional level, but find that the political impact of immigration is more ambiguous and difficult to characterize in the data at this degree of geographic granularity. Finally, I examine the effect of perceived, as opposed to real, immigration exposure. The results suggest that perceptions regarding immigration may be more politically polarizing than reality, even at the regional level, and the effect is intensified when economic insecurity pervades a population.

In section 2, I review the relevant literature on this topic. In section 3, I discuss the collection and construction of my data, and present summary statistics of country- and region-level datasets. In section 4, I describe and justify my empirical approach. In section 5, I present my empirical results on the impacts of immigration exposure on political polarization. Section 6 concludes.

2. Literature Review
While various scholarly papers have explored the link between economic shocks and political behavior as well as the determinants of public opinion towards immigration, there is no direct empirical evidence on the relationship between immigration exposure and political polarization. Here, I critically summarize the related literature adjacent to this topic, identify gaps in the knowledge on this issue, and propose an empirical approach to answering some of the remaining questions.

First, it is useful to mention that both Hatton (2016) and Longhi and Markaki (2012) have previously exploited the European Social Survey (ESS), the same dataset I use, to study the determinants of attitudes towards immigration. Hatton (2016) looks at country-level effects from 2002 to 2012 and finds that the immigrant share of a population and the social benefit share of GDP are the two most influential drivers of anti-immigration sentiment. The paper shows that both have a negative impact on public opinion towards immigration, but the effects are not economically large. Longhi and Markaki (2012) study regional effects and also find that larger immigrant shares of the population increase anti-immigrant

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1 This is analogous to the link that Autor et al. (2016) draw between trade exposure and political polarization in the United States from 2000 to 2016.
attitudes, a result mostly driven by non-EU immigrants. Importantly, both studies only test the effect of immigration on attitudes towards immigration. I expand upon these findings by testing the responsiveness of broader political beliefs, which may be more indicative of any greater political divisions caused by immigration.

The literature on the link between trade exposure and political behavior may help shed light on the merits of various measures of political sentiment. The most recent and perhaps most thorough analysis of the effect of trade on political polarization comes from Autor et al. (2016). In their working paper, the researchers find that U.S. local labor markets that are more exposed to Chinese import competition experience greater political polarization as measured by their elected legislators’ voting records. Furthermore, they show that increased trade exposure is associated with a net rightward shift in political beliefs as measured by Republican vote share in presidential elections from 2000 to 2016. However, while the paper leverages exhaustive data on congressional voting records, the use of legislator ideology to measure political polarization may obscure the true underlying public opinion. Furthermore, the result that trade exposure leads to a net rightward political shift is surprising, considering that Republicans tend to support free trade policies.

Indeed, Che et al. (2016) find essentially the opposite, that U.S. counties subject to greater import exposure from China from 1992 to 2010 were more likely to vote for Democrats and be represented by a Democrat. This result seems consistent with the fact that Democrats over this period were more likely to support protectionist or economic assistance policies. Nonetheless, these findings are complicated by the fact that county vote share is a noisy estimate of congressional election winners, since counties and congressional districts do not align exactly, and also by the fact that a net shift toward Democratic leadership may disguise more nuanced shifts in voter ideologies. Dippel et al. (2015), contrarily, find evidence from German administrative districts that only the vote for extreme right-wing parties was responsive to increased trade exposure. Taken together, the scholarship on trade exposure and political polarization suggests that differing levels of geographic granularity and differing proxies for political sentiment (party vote share, legislator ideology, voter ideology) may produce different results when studying the effect of economic shocks on political beliefs.

While no studies directly test the effect of immigration on political polarization, some insights and questions emerge from studies on the determinants of public opinion towards immigration. Citrin et al. (1997) use U.S. National Election Study (NES) surveys to show that restrictionist sentiment is primarily driven by perceptions about the state of the economy, anxiety about taxes, and feelings towards major immigrant groups, but is not related to real-life economic circumstances. These findings are largely consistent with those of Alvarez and Butterfield (2000), who study a 1994 ballot initiative in California (Proposition 187) aimed at curbing immigration and excluding illegal immigrants from public services. They also find that perceptions of financial threat as well as proximity to the immigrant source significantly increased the likelihood of support for Prop. 187. While instructive, these findings may not be generalizable given the specificity of the sample population and ballot measure they study.

Standing in contrast to these papers are the findings of Scheve and Slaughter (2001). Also using data from NES surveys, they find that low-skill workers are significantly more likely than high-skill workers to support restrictive immigration policies. While not a true measure of economic welfare, this skill-preference disparity may suggest that real-life economic circumstances do affect immigration policy opinions, not just perceived circumstances. Perhaps more interesting is that Scheve and Slaughter (2001) find that, if anything, living in an area with increased immigration attenuates the relationship between low skills and support for restrictive immigration policy. The varied findings in the literature studying the determinants of immigration sentiment leaves two important questions: 1) the effect, or lack thereof, of increased exposure (proximity) to immigration and 2) the difference in the effects of real versus perceived circumstances on political sentiments.

To close this knowledge gap in the current literature, I test two main hypotheses using data from Europe from 2002 to 2016, a period during which there was a substantial immigrant influx into many European countries. The first hypothesis is that areas exposed to greater stocks of immigrants as a share of the population experience greater political polarization. Empirically, my measure of political polarization is derived from a self-reported measure of left-right political orientation, a direct representation of public opinion. This proxy for political polarization contributes to the literature which has thus
far focused on party vote share or legislator ideology to measure political beliefs.

Second, I test the hypothesis that countries and regions whose citizens perceive greater exposure to immigration also experience greater political polarization. To test this empirically, I use self-reported estimates of the foreign-born percent share of survey respondents’ country population. By considering the strengths and weaknesses of the existing literature to craft my empirical approach, and by leveraging a rich dataset (described in section 3), I contribute to the scholarly evidence by addressing gaps in the current knowledge of the link between immigration and political polarization.

3. Data

In my analysis, all data come from the ESS. The ESS has been conducted across some 38 European countries biennially from 2002 to 2016. It randomly samples individuals aged 15 and older within private households from participating countries. I use the individual-level survey responses to construct country- and region-level means of several variables of interest to create panel datasets. I thus have two sets of panel data—the first spans 38 European countries over eight waves of data, and the second spans 421 regions within those countries over four waves of data. Both panels are unbalanced since many countries do not participate in every wave of the ESS.

My main dependent variable is a measure of political polarization. In the raw ESS data, each observation contains a self-reported measure of left-right political orientation. I generate a dummy indicating whether an individual is politically polarized, the methodology for which is described in Appendix A. I then collapse the data by country (and again by region) to obtain the mean of this polarization dummy, which represents the percent share of the country (or region) who consider themselves to have polarized political views. In computing these means, I use post-stratification weights provided by the ESS.

My independent variables are constructed in a similar fashion, collapsing individual-level survey data to obtain weighted means for countries and regions. The main independent variable is an estimate of the foreign-born share of the population. The ESS records whether each survey respondent was born in the country where they are being surveyed. I construct a dummy variable equal to 1 if the individual is foreign-born, and obtain a weighted mean representing the percent foreign born share of a country population. As controls, I use measures of economic insecurity, the age of the population, the education of the population, and a dummy that indicates Eurozone countries (since the euro area has been hit harder by both economic hardship and immigration in the past decade). To measure economic insecurity, I draw from Guiso et al. (2017) and use a variable indicating whether an individual is unemployed and seeking work, and another variable indicating whether an individual finds it difficult to live comfortably on present household income. I then find it useful to combine these two measures into a single index of economic insecurity. For age, I simply take the weighted mean for each country and region of the age in years of survey respondents. The education variable, a weighted mean of a dummy variable, measures the share of the population in each country and region who have completed post-secondary education. Summary statistics of the data are reported for countries and regions in Tables 1 and 2, respectively.

Importantly, all my variables are only estimates of country- and region-level statistics. Whereas some of my independent variables, such as foreign-born share and unemployment, might be available as official statistics from other sources, I use only ESS-based estimates to preserve internal consistency with the construction of my dependent variable. The potential...
strength of this approach is that it assumes the ESS sampling practices allow for me to compute meaningful country- and region-level averages. While the smallest number of underlying observations for any country in my panel is still 523 individuals, many regions are represented in the data by as few as two individual survey respondents. The result is that many region-level estimates are skewed by small underlying sample sizes. Thus, I will need restrict my sample to only regions for which there are enough underlying survey respondents to suggest that a regional average is a reasonable estimate of the true value.

4. Empirical Methods
I examine the political consequences of exposure to immigration in several stages. In section 5.1, I test the hypothesis that countries with a greater stock of immigrants have a greater share of the population who are politically polarized. I then consider whether the effect of immigration is truly polarizing, or whether it moves political attitudes in one direction specifically. Next, I further explore the cross-country variation in the political response to immigration exposure by testing the differential effect of various population characteristics. In section 5.2, I extend my analysis to the regional level to examine whether a greater geographic disaggregation shows the same relationship as in the country-level analyses. Across these stages, I estimate equations of the form:

\[ P_{it} = \alpha + \beta * FBS_{it} + \chi_{it} + \gamma_t + \delta_i + \varepsilon_{it} \]

The dependent variable \( P_{it} \) is the political attitude outcome (usually the share of the population that is polarized, but sometimes the share that is left-wing polarized or the share that is right-wing polarized) for wave-year \( t \) in country \( i \) (or region \( i \) in later specifications). The main independent variable is the foreign-born population share \( FBS_{it} \) in country/region \( i \) in wave-year \( t \). The set of control variables \( \chi_{it} \) measure economic insecurity, population age, the share of the population who are highly educated, and euro area membership in country/region \( i \) in wave-year \( t \), as well as interactions between each of those variables and foreign-born population share in some cases. All regressions in sections 5.1 and 5.2 also include \( \gamma_t \) wave-year fixed effects and \( \delta_i \) country fixed effects.

Finally, in section 5.3, I conduct a supplementary analysis of the effect of immigration on political polarization using perceived levels of immigration exposure as opposed to estimates of the real stock of immigrants. These regressions take the same form as equation (1), except that the key independent variable \( FBS_{it} \) instead represents the perceived foreign-born share of the population in country/region \( i \) in wave-year \( t \). Also, wave-year and country/region fixed effects are not included.

5. Results
5.1 Country-level analysis
In my first specification, I examine the country-level impact of immigration exposure on political polarization. The results of this preliminary country-level analysis are reported in Table 3. Column (1) of Table 3 reports the results of a parsimonious OLS regression specification that controls for country and wave-year fixed effects. The coefficient on the foreign-born population share is positive and significant at the 10 percent level. Columns (2) - (5) gradually add country characteristic controls to the original specification. In each case, the coefficient on the foreign-born population share is positive and precisely estimated. The magnitude of the coefficient estimate in column (5) suggests that a one percentage point increase in a country's foreign-born population share leads to a nearly 0.5 percentage point increase in the politically polarized population share, controlling for economic insecurity, age, and education of the population, as well as euro area membership. This result is consistent with the hypothesis that increased exposure to immigration may create a sense of resource competition, whether real or perceived, that drives voters to engage in group identity politics.

However, the relationship observed between foreign-born population share and this measure of political polarization is not by itself evidence of a polarizing effect of immigration. Rather, the underlying movement of political beliefs in response to immigration may be one-directional. It may simply be the case that countries with a greater immigrant share have only a greater share of right-wing voters or of left-wing voters but not both, a trend which is masked by grouping the two tails of the political spectrum together in my dependent variable. Indeed, a one-directional effect of immigration
that pushes political attitudes uniformly to the right might be plausible, since conservative parties and politicians tend to promote stronger restrictions on immigration than their liberal counterparts. To test this possibility, I modify my specification to test right-wing and left-wing share of the population as separate dependent variables. The results of this analysis are reported in Table 4.

Column (1) of Table 4 reports the results from the same full specification that was shown in column (4) of Table 3, for comparison. Columns (2) and (3) of Table 4 show the results when the dependent variable of politically polarized population share is replaced with the left-wing and right-wing polarized shares, respectively. In both columns, the coefficient on the foreign-born population share is positive, but not significant. Though this test of bidirectional, as opposed to one-directional, polarization is inconclusive due to the lack of statistical significance, it is at least not inconsistent with a bidirectional polarization effect of immigration. That the coefficient on foreign-born population share is positive in both columns (2) and (3) suggests that immigration may cause an increased share of both left- and right-wing voters, though these effects are not precisely measured. Nonetheless, the coefficient on foreign-born population share in column (3) is nearly three times that of the coefficient in column (2), suggesting that the right wing may capture more of the shift in political attitudes than the left—but, again, these estimates are not statistically significant.

To more specifically characterize the impact of exposure to immigration on political polarization, it may be useful to examine how the effect differs based on country-specific characteristics. For example, countries with a higher incidence of economic insecurity may become comparatively more polarized by immigration, perhaps because the notion of resource competition is heightened by economic hardship. On the other hand, populations that are more highly educated may be more open-minded towards immigrants because they better understand the potential economic and social benefits of immigration. Thus, these populations may be less likely to react to immigration by turning to divisive politics, and so the increase in political polarization caused by immigration may be lower in more highly educated countries. To test these hypotheses, I add to the regression specification interaction terms between foreign-born population share and each of the four country-characteristic variables: economic insecurity, age, education, and euro area membership. The results of this analysis are reported in Table 5.

Table 5 mimics Table 4, with the addition of the interaction terms as regressors. Column (1) of Table 5 reports the results from the full estimation equation regression where the dependent variable is the politically polarized share of the population. The coefficient on the interaction between economic insecurity and foreign-born population share is positive and statistically significant at the five percent level. This is consistent with the hypothesis that more pervasive economic insecurity amplifies the effect of immigration on the polarization of political attitudes. Conversely, the coefficient on the interaction between education and foreign-born population share is negative and statistically significant at the five percent level. This is again consistent with the previously proposed hypothesis—that a more educated population is less likely to be politically polarized by an increase in immigration. Furthermore, the coefficient on the uninteracted foreign-born share term becomes statistically insignificant with the addition of interaction terms to the regression. This suggests that the cross-country variation in the effects of immigration on polarization is primarily driven by the ways that immigration is viewed through the lenses of economic insecurity and education.

Column (3) of Table 5 reports the results of the same specification, but with the right-wing polarized share as the dependent variable. The coefficients on the two interaction terms of interest—economic insecurity and education—are again positive and negative, respectively. Both are precisely measured and of approximately the same magnitude as the corresponding coefficients in column (1). The corresponding coefficient estimates in column (2), where left-wing polarization is the dependent variable, are small in magnitude and statistically insignificant. These results seem to indicate that more economic insecurity throughout a country intensifies a rightward shift in political attitudes caused by immigration, but has no detectable outsized impact on left-wing voting; similarly, education dampens the right-wing polarization effect of immigration, but does not differentially affect the left wing.

5.2 Region-level analysis
In this section, I attempt to examine the relationship between immigration and political polarization at a more disaggregated
level, using European regions rather than countries as the unit of analysis. Beginning with the fifth wave of the survey in 2010, the ESS has coded respondents geographically at the regional level.\(^5\) Studying regions as opposed to countries may have two advantages: 1) regions are, by design, closer in population size than countries and may therefore be more apt for comparison, and 2) there may exist effects of immigration at the regional level that are obscured when they are aggregated at the country level. Thus, I specify the same regressions of political polarization on immigration as before, but with regions as the unit of analysis. The results are presented in Table 6.

These results do not support the same relationship between immigration and political polarization that was found in the country-level analysis. In column (1), the coefficient on the foreign-born share population share is negative, small in magnitude, and statistically insignificant. Furthermore, the statistical insignificance of this coefficient estimate does not change in column (2) after adding interaction terms, nor in columns (3) or (4), after removing regions whose variable estimates were constructed from a small underlying survey sample.\(^6\) The coefficients on the interaction terms in column (5) and (6) suggest some differential impacts of immigration on political polarization based on regional characteristics: the impact of immigration is greater on the left wing in economically insecure regions, slightly greater on the right wing in regions with older populations, and lesser on the right wing in regions in euro area countries. These results are smaller in magnitude and largely inconsistent with the previous country-level findings.

It is perhaps surprising that the results from the country-level analysis do not hold at the regional level since, if anything, we might expect the opposite—that effects of immigration at the regional level were washed out at the cruder country level. That the reverse was found is puzzling, and is perhaps due to the nature of the construction of the data. After all, the validity of this study relies on the representative power of the ESS to estimate country- and region-level statistics. This may be less reliable at the region level, where fewer individual observations are meant to give a sound approximation of the regional average. At this finer level of geographic variation, imperfectly random survey sampling may interfere more with the results than in the country-level analyses.

### 5.3 Analysis of perceived immigration exposure

Another explanation for the mixed results on the relationship between immigration exposure and political polarization is the potentially important distinction between real and perceived immigration. As discussed in section 2, per both Citrin et al. (1997) and Alvarez and Butterfield (2000), individuals' perceptions of economic and immigration circumstances, rather than real circumstances, are more reliable predictors of immigration attitudes. The same may well be true for the effect of real versus perceived immigration on political polarization. The first and seventh waves of the ESS include a rotating module which asks several questions about immigration, including an estimate of the foreign-born population share of the respondent's country.\(^7\) This question allows me to construct a measure of the perceived immigrant exposure, as opposed to the measure of real immigrant exposure that has been the independent variable of interest up to this point in the paper. Figure 1 plots these two values, along with a fitted line, for each country in 2002 and 2014. It appears that people tend to overestimate the share of immigrants living in their country. In fact, in the country-level data, perceived foreign-born share is invariably greater than real foreign-born share.\(^8\) Figure 2 shows the same relationship at the regional level in 2014. Especially at low to medium levels of real foreign-born population share, and aside from a handful of outliers, people almost always overestimate the share of immigrants living in their country relative to the true share living in their region. These figures suggest that substituting perceived immigration for real immigration in the previous country- and region-level regressions may yield different results. To that end, I modify the fully specified estimation equations from before—including all controls and interactions—by replacing foreign-born population share with perceived foreign-

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5 The ESS uses the nomenclature of territorial units for statistics (NUTS) regions.

6 Especially at the finer regional level, the validity of the data depends on the statistical power of the ESS to estimate region-level statistics, so dropping small regions with weak statistical power is essential

7 The survey question asks, “Out of every 100 people living in [country], how many do you think were born outside [country]?”

8 The “perceived foreign-born share” data are only available at the country level in waves 1 and 7 of the ESS when the rotating module was included, and only available at the regional level in wave 7, since regions are not encoded in the wave 1 data.
born population share as the key independent variable and in the four interaction terms. I then estimate the modified econometric model at the country- and region-level, with each of polarized share, left-wing share, and right-wing share as the dependent variable. The results of this analysis are reported in Table 7.

Indeed, the results in Table 7 provide some key insights. Columns (1) - (3) show the estimates of the country-level effect of perceived immigration on political polarization, on left-wing polarization, and on right-wing polarization. Compared to Table 5, the coefficients on the interaction terms in columns (1) - (3) of Table 7 tell a similar story of the country-specific differences in the political impact of immigration. While the smaller sample size in these regressions limits my statistical power to detect a precise estimate of the coefficient on the education interaction term, the coefficient on the interaction between economic insecurity and perceived immigration in column (1) of Table 7 is positive and highly significant at the one percent level. Also consistent with previous results, the coefficient on this economic insecurity interaction term in column (2) is insignificant and in column (3) is positive and significant at the five percent level, suggesting that the differential effect of economic insecurity is mostly driven by moderates moving to the right wing but does not necessarily impact the share of left-wing voters.

Whereas the effects of foreign-born population share on political polarization were mostly indeterminable in section 5.2, columns (4) and (6) of Table 7 show that the coefficients on the interaction between economic insecurity and perceived foreign-born population share are positive and statistically significant at the five percent level. While economic insecurity was not shown to have a differential effect on the political impact of real immigration at the regional level, these results suggest that regions undergoing greater economic hardship do become disproportionately more polarized by perceived country-level immigration. The increased polarization is entirely driven by movement to the right, with no detectable differential impact on the left wing. Furthermore, the coefficients on the interaction between economic insecurity and perceived foreign-born population share in columns (4) and (6) are more than twice the magnitude of those in columns (1) and (3), suggesting that the differential effect of economic insecurity with respect to perceived immigration at the regional level may be doubly impactful than at the country level. It must be noted that the sample sizes in the regressions shown in Table 7 are much smaller than in all previous analyses, so the validity of the results should not be overstated. Nonetheless, this supplemental piece of analysis suggests that, especially at the regional level, perceived immigration may be a more salient, polarizing political factor than true immigration.

6. Conclusion

The simultaneous and precipitous rise of both immigration and divisive politics in Europe in recent years begs the question of whether there is a causal relationship between the two. My contribution in this paper is to show that countries with a larger share of immigrants find themselves more politically polarized, and that this is driven by pervasive economic insecurity and attenuated by high levels of education. While these associations are harder to detect at finer levels of geographic granularity, I show that perceptions of immigration exposure, if not real circumstances, do indeed combine with economic hardship to propel political polarization across both European countries and regions.

That an economic shock such as immigration produces a clear response in political sentiments may be unsurprising. However, that the response is bidirectional and not uniformly skewed towards the right wing, with its tendency to embrace restrictive immigration policies, is non-obvious. Policies that bolster economic growth and promote higher education in immigrant-heavy communities may counteract the potential for political vehemence. Future studies might also focus on explaining the disconnect between real and perceived immigration, given that they appear to be often misaligned and both salient political factors.

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9 Due to limited data, none of the regressions in Table 7 include country or wave-year fixed effects.
References


Appendices

Robustness tests

In addition to the analyses presented in section 5, I perform three tests of the robustness of my country-level results. First, I run the same regression specified in column (1) of Table 5 while dropping each country from the sample, one at a time. In all cases, the coefficients of interest on the interactions terms are of roughly the same magnitude and statistical significance, suggesting that the results are not biased by any one country. Second, I perform a similar test by dropping each wave of the ESS from the sample sequentially. Again, the results appear not to be biased by any one wave-year of the data. Finally, I check the sensitivity of my results to the definition of polarization described in Appendix A. Toggling the liberal-moderate and conservative-moderate cutoffs by 1 in either direction does in fact lead to statistical insignificant estimates of the coefficients of interest in country-level regressions. This suggests that, while my core numerical definition of political polarization in the data may be reasonable, my results are highly sensitive to this definition.

Figure 1. Real vs. perceived foreign-born share across European countries, 2002 and 2014

Figure 2. Real vs. perceived foreign-born share across European regions, 2014
Table 1. Summary statistics for key dependent and independent variables of country-level data

<table>
<thead>
<tr>
<th>Variable</th>
<th>Obs.</th>
<th>Mean</th>
<th>Std. Dev.</th>
<th>Min</th>
<th>Max</th>
</tr>
</thead>
<tbody>
<tr>
<td>Foreign-born share</td>
<td>157</td>
<td>0.091</td>
<td>0.068</td>
<td>0.003</td>
<td>0.345</td>
</tr>
<tr>
<td>Perceived foreign-born share</td>
<td>34</td>
<td>0.204</td>
<td>0.087</td>
<td>0.071</td>
<td>0.400</td>
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<tr>
<td>Polarized share</td>
<td>157</td>
<td>0.267</td>
<td>0.079</td>
<td>0.148</td>
<td>0.594</td>
</tr>
<tr>
<td>Economic insecurity index</td>
<td>157</td>
<td>0.355</td>
<td>0.293</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>Age</td>
<td>157</td>
<td>46.4</td>
<td>2.23</td>
<td>38.2</td>
<td>52.5</td>
</tr>
<tr>
<td>Education</td>
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<td>0.311</td>
<td>0.122</td>
<td>0.111</td>
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<tr>
<td>Eurozone</td>
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<td>0.433</td>
<td>0.497</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>No. underlying survey respondents</td>
<td>157</td>
<td>1603.7</td>
<td>432.3</td>
<td>459</td>
<td>2891</td>
</tr>
</tbody>
</table>

Note: units of observations are European countries from 2002 to 2016. Variable values are weighted means of individual-level survey data, not official statistics. Perceived foreign-born share data are only available for 2002 and 2014.

Table 2. Summary statistics for key dependent and independent variables of region-level data

<table>
<thead>
<tr>
<th>Variable</th>
<th>Obs.</th>
<th>Mean</th>
<th>Std. Dev.</th>
<th>Min</th>
<th>Max</th>
</tr>
</thead>
<tbody>
<tr>
<td>Foreign-born share</td>
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<td>0.075</td>
<td>0</td>
<td>0.613</td>
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<tr>
<td>Perceived foreign-born share</td>
<td>250</td>
<td>0.192</td>
<td>0.076</td>
<td>0.041</td>
<td>0.375</td>
</tr>
<tr>
<td>Polarized share</td>
<td>1128</td>
<td>0.269</td>
<td>0.112</td>
<td>0</td>
<td>0.865</td>
</tr>
<tr>
<td>Economic insecurity index</td>
<td>1128</td>
<td>0.309</td>
<td>0.237</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>Age</td>
<td>1128</td>
<td>47.8</td>
<td>3.90</td>
<td>28.1</td>
<td>67.2</td>
</tr>
<tr>
<td>Education</td>
<td>1128</td>
<td>0.330</td>
<td>0.149</td>
<td>0</td>
<td>0.863</td>
</tr>
<tr>
<td>Eurozone</td>
<td>1128</td>
<td>0.496</td>
<td>0.500</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>No. underlying survey respondents</td>
<td>1128</td>
<td>137.3</td>
<td>166.4</td>
<td>2</td>
<td>2327</td>
</tr>
</tbody>
</table>

Note: units of observations are European NUTS regions from 2010 to 2016. Variable values are weighted means of individual-level survey data, not official statistics. Perceived foreign-born share data are only available for 2014.
### Table 3. The effect of country-level immigration exposure on political polarization in Europe from 2002 to 2016

<table>
<thead>
<tr>
<th>Variables</th>
<th>(1) Polarized Share</th>
<th>(2) Polarized Share</th>
<th>(3) Polarized Share</th>
<th>(4) Polarized Share</th>
<th>(5) Polarized Share</th>
</tr>
</thead>
<tbody>
<tr>
<td>Foreign-born share</td>
<td>0.394**</td>
<td>0.473**</td>
<td>0.472**</td>
<td>0.473**</td>
<td>0.484**</td>
</tr>
<tr>
<td></td>
<td>(0.188)</td>
<td>(0.205)</td>
<td>(0.211)</td>
<td>(0.209)</td>
<td>(0.212)</td>
</tr>
<tr>
<td>Economic insecurity</td>
<td>-0.032</td>
<td>-0.032</td>
<td>-0.038</td>
<td>-0.038</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.023)</td>
<td>(0.023)</td>
<td>(0.024)</td>
<td>(0.024)</td>
<td>(0.025)</td>
</tr>
<tr>
<td>Age</td>
<td>-0.0001</td>
<td>-0.0005</td>
<td>-0.0006</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.003)</td>
<td>(0.002)</td>
<td>(0.002)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Education</td>
<td>0.048</td>
<td></td>
<td>0.047</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.045)</td>
<td></td>
<td>(0.044)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Eurozone</td>
<td></td>
<td></td>
<td>0.021*</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(0.012)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>0.224***</td>
<td>0.232***</td>
<td>0.238*</td>
<td>0.239*</td>
<td>0.233*</td>
</tr>
<tr>
<td></td>
<td>(0.020)</td>
<td>(0.020)</td>
<td>(0.135)</td>
<td>(0.118)</td>
<td>(0.118)</td>
</tr>
<tr>
<td>Observations</td>
<td>157</td>
<td>157</td>
<td>157</td>
<td>157</td>
<td>157</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.907</td>
<td>0.909</td>
<td>0.909</td>
<td>0.910</td>
<td>0.910</td>
</tr>
</tbody>
</table>

Note: all regressions include country and wave-year fixed effects, and are weighted by relative country population size. Standard errors corrected by clustering on country are reported in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

### Table 4. The effect of country-level immigration exposure on political polarization in Europe from 2002 to 2016

<table>
<thead>
<tr>
<th>Variables</th>
<th>(1) Polarized Share</th>
<th>(2) Left-Wing Polarized Share</th>
<th>(3) Right-Wing Polarized Share</th>
</tr>
</thead>
<tbody>
<tr>
<td>Foreign-born share</td>
<td>0.484**</td>
<td>0.128</td>
<td>0.356</td>
</tr>
<tr>
<td></td>
<td>(0.212)</td>
<td>(0.153)</td>
<td>(0.272)</td>
</tr>
<tr>
<td>Economic insecurity</td>
<td>-0.038</td>
<td>0.027*</td>
<td>-0.065**</td>
</tr>
<tr>
<td></td>
<td>(0.025)</td>
<td>(0.014)</td>
<td>(0.030)</td>
</tr>
<tr>
<td>Age</td>
<td>-0.0006</td>
<td>-0.005**</td>
<td>0.004*</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.002)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>Education</td>
<td>0.0465</td>
<td>0.082***</td>
<td>-0.036</td>
</tr>
<tr>
<td></td>
<td>(0.044)</td>
<td>(0.022)</td>
<td>(0.040)</td>
</tr>
<tr>
<td>Eurozone</td>
<td>0.021*</td>
<td>0.022***</td>
<td>-0.0009</td>
</tr>
<tr>
<td></td>
<td>(0.012)</td>
<td>(0.007)</td>
<td>(0.010)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.233*</td>
<td>0.298***</td>
<td>-0.066</td>
</tr>
<tr>
<td></td>
<td>(0.118)</td>
<td>(0.101)</td>
<td>(0.133)</td>
</tr>
<tr>
<td>Observations</td>
<td>157</td>
<td>157</td>
<td>157</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.910</td>
<td>0.880</td>
<td>0.920</td>
</tr>
</tbody>
</table>

Note: all regressions include country and wave-year fixed effects, and are weighted by relative country population size. Standard errors corrected by clustering on country are reported in parentheses.

*** p<0.01, ** p<0.05, * p<0.1
<table>
<thead>
<tr>
<th>Variables</th>
<th>Polarized Share</th>
<th>Left-Wing Polarized Share</th>
<th>Right-Wing Polarized Share</th>
</tr>
</thead>
<tbody>
<tr>
<td>Foreign-born share</td>
<td>-2.41</td>
<td>-1.88</td>
<td>-0.533</td>
</tr>
<tr>
<td></td>
<td>(2.11)</td>
<td>(1.49)</td>
<td>(1.47)</td>
</tr>
<tr>
<td>Economic insecurity</td>
<td>-0.100***</td>
<td>0.015</td>
<td>-0.114***</td>
</tr>
<tr>
<td></td>
<td>(0.026)</td>
<td>(0.019)</td>
<td>(0.020)</td>
</tr>
<tr>
<td>Age</td>
<td>-0.009*</td>
<td>-0.009**</td>
<td>-0.0002</td>
</tr>
<tr>
<td></td>
<td>(0.005)</td>
<td>(0.004)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>Education</td>
<td>0.233***</td>
<td>0.094</td>
<td>0.139**</td>
</tr>
<tr>
<td></td>
<td>(0.082)</td>
<td>(0.059)</td>
<td>(0.068)</td>
</tr>
<tr>
<td>Eurozone</td>
<td>0.0368**</td>
<td>0.0330**</td>
<td>0.00381</td>
</tr>
<tr>
<td></td>
<td>(0.016)</td>
<td>(0.016)</td>
<td>(0.017)</td>
</tr>
<tr>
<td>Economic insecurity x Foreign-born share</td>
<td>1.38**</td>
<td>0.328</td>
<td>1.05**</td>
</tr>
<tr>
<td></td>
<td>(0.555)</td>
<td>(0.559)</td>
<td>(0.411)</td>
</tr>
<tr>
<td>Age x Foreign-born share</td>
<td>0.076*</td>
<td>0.042</td>
<td>0.035</td>
</tr>
<tr>
<td></td>
<td>(0.045)</td>
<td>(0.033)</td>
<td>(0.033)</td>
</tr>
<tr>
<td>Education x Foreign-born share</td>
<td>-2.73**</td>
<td>-0.054</td>
<td>-2.68***</td>
</tr>
<tr>
<td></td>
<td>(1.02)</td>
<td>(0.857)</td>
<td>(0.767)</td>
</tr>
<tr>
<td>Eurozone x Foreign-born share</td>
<td>-0.231</td>
<td>-0.237</td>
<td>0.006</td>
</tr>
<tr>
<td></td>
<td>(0.255)</td>
<td>(0.254)</td>
<td>(0.254)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.598**</td>
<td>0.496**</td>
<td>0.102</td>
</tr>
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<td>(0.226)</td>
<td>(0.204)</td>
<td>(0.119)</td>
</tr>
<tr>
<td>Observations</td>
<td>157</td>
<td>157</td>
<td>157</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.921</td>
<td>0.887</td>
<td>0.932</td>
</tr>
</tbody>
</table>

Note: all regressions include country and wave-year fixed effects, and are weighted by relative country population size. Standard errors corrected by clustering on country are reported in parentheses.

*** p<0.01, ** p<0.05, * p<0.1
Table 6. The cross-regional effects of immigration exposure on political polarization in Europe from 2010 to 2016

<table>
<thead>
<tr>
<th>Variables</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Foreign-born share</td>
<td>-0.032</td>
<td>1.32</td>
<td>0.005</td>
<td>-0.723</td>
<td>0.502</td>
<td>-1.23</td>
</tr>
<tr>
<td></td>
<td>(0.102)</td>
<td>(1.13)</td>
<td>(0.079)</td>
<td>(0.739)</td>
<td>(0.892)</td>
<td>(0.825)</td>
</tr>
<tr>
<td>Economic insecurity</td>
<td>0.047</td>
<td>0.039</td>
<td>0.049</td>
<td>0.036</td>
<td>0.019</td>
<td>0.017</td>
</tr>
<tr>
<td></td>
<td>(0.056)</td>
<td>(0.057)</td>
<td>(0.043)</td>
<td>(0.050)</td>
<td>(0.038)</td>
<td>(0.037)</td>
</tr>
<tr>
<td>Age</td>
<td>0.002</td>
<td>0.004**</td>
<td>0.003**</td>
<td>0.002</td>
<td>0.005**</td>
<td>-0.003</td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td>(0.002)</td>
<td>(0.002)</td>
<td>(0.002)</td>
<td>(0.002)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>Education</td>
<td>0.023</td>
<td>-0.063</td>
<td>0.100*</td>
<td>0.074</td>
<td>0.046</td>
<td>0.028</td>
</tr>
<tr>
<td></td>
<td>(0.044)</td>
<td>(0.063)</td>
<td>(0.056)</td>
<td>(0.088)</td>
<td>(0.051)</td>
<td>(0.069)</td>
</tr>
<tr>
<td>Eurozone</td>
<td>0.012</td>
<td>-0.004</td>
<td>-0.023</td>
<td>-0.006</td>
<td>0.0009</td>
<td>-0.007</td>
</tr>
<tr>
<td></td>
<td>(0.030)</td>
<td>(0.034)</td>
<td>(0.021)</td>
<td>(0.023)</td>
<td>(0.018)</td>
<td>(0.015)</td>
</tr>
<tr>
<td>Economic insecurity x</td>
<td>0.077</td>
<td>0.323</td>
<td>0.691*</td>
<td>-0.368</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Foreign-born share</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.336)</td>
<td>(0.518)</td>
<td>(0.367)</td>
<td>(0.312)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Age x Foreign-born share</td>
<td>-0.039*</td>
<td>0.012</td>
<td>-0.020</td>
<td>0.033**</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.023)</td>
<td>(0.014)</td>
<td>(0.018)</td>
<td>(0.016)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Education x Foreign-born</td>
<td>0.873**</td>
<td>0.403</td>
<td>0.651</td>
<td>-0.247</td>
<td></td>
<td></td>
</tr>
<tr>
<td>share</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.424)</td>
<td>(0.539)</td>
<td>(0.411)</td>
<td>(0.367)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Eurozone x Foreign-born</td>
<td>0.270</td>
<td>-0.231**</td>
<td>-0.063</td>
<td>-0.169*</td>
<td></td>
<td></td>
</tr>
<tr>
<td>share</td>
<td></td>
<td></td>
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<td></td>
</tr>
<tr>
<td></td>
<td>(0.191)</td>
<td>(0.106)</td>
<td>(0.102)</td>
<td>(0.098)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>0.146*</td>
<td>0.073</td>
<td>0.012</td>
<td>0.082</td>
<td>-0.158</td>
<td>0.240**</td>
</tr>
<tr>
<td></td>
<td>(0.075)</td>
<td>(0.098)</td>
<td>(0.075)</td>
<td>(0.092)</td>
<td>(0.110)</td>
<td>(0.099)</td>
</tr>
<tr>
<td>Observations</td>
<td>1,128</td>
<td>1,128</td>
<td>457</td>
<td>457</td>
<td>457</td>
<td>457</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.333</td>
<td>0.356</td>
<td>0.555</td>
<td>0.569</td>
<td>0.472</td>
<td>0.709</td>
</tr>
<tr>
<td>Sample</td>
<td>All</td>
<td>All</td>
<td>N&gt;117</td>
<td>N&gt;117</td>
<td>N&gt;117</td>
<td>N&gt;117</td>
</tr>
</tbody>
</table>

Note: all regressions include country and wave-year fixed effects, and are weighted by relative country population size. Standard errors corrected by clustering on region are reported in parentheses. Regressions in columns (3) - (6) include only regions for which variable estimates were calculated using more than 117 underlying individual survey respondents (117 is five percent of the largest region sample size).

*** p<0.01, ** p<0.05, * p<0.1
### Table 7. The effects of perceived immigration exposure on political polarization in European countries and regions in 2002 and 2014

<table>
<thead>
<tr>
<th>Variables</th>
<th>(1) Country Polarized Share</th>
<th>(2) Country Left-Wing Polarized Share</th>
<th>(3) Country Right-Wing Polarized Share</th>
<th>(4) Region Polarized Share</th>
<th>(5) Region Left-Wing Polarized Share</th>
<th>(6) Region Right-Wing Polarized Share</th>
</tr>
</thead>
<tbody>
<tr>
<td>Perceived foreign-born share</td>
<td>2.82</td>
<td>-0.275</td>
<td>3.09</td>
<td>0.467</td>
<td>1.75</td>
<td>-1.28</td>
</tr>
<tr>
<td></td>
<td>(1.81)</td>
<td>(1.22)</td>
<td>(2.47)</td>
<td>(2.91)</td>
<td>(1.72)</td>
<td>(3.42)</td>
</tr>
<tr>
<td>Economic insecurity</td>
<td>-0.098*</td>
<td>0.049</td>
<td>-0.147*</td>
<td>-0.374</td>
<td>0.113</td>
<td>-0.487***</td>
</tr>
<tr>
<td></td>
<td>(0.051)</td>
<td>(0.038)</td>
<td>(0.071)</td>
<td>(0.226)</td>
<td>(0.131)</td>
<td>(0.176)</td>
</tr>
<tr>
<td>Age</td>
<td>0.001</td>
<td>-0.008</td>
<td>0.009</td>
<td>0.0006</td>
<td>0.013**</td>
<td>-0.013</td>
</tr>
<tr>
<td></td>
<td>(0.005)</td>
<td>(0.006)</td>
<td>(0.008)</td>
<td>(0.009)</td>
<td>(0.006)</td>
<td>(0.012)</td>
</tr>
<tr>
<td>Education</td>
<td>0.487</td>
<td>0.312</td>
<td>0.175</td>
<td>0.442</td>
<td>0.312</td>
<td>0.130</td>
</tr>
<tr>
<td></td>
<td>(0.519)</td>
<td>(0.193)</td>
<td>(0.452)</td>
<td>(0.326)</td>
<td>(0.252)</td>
<td>(0.434)</td>
</tr>
<tr>
<td>Eurozone</td>
<td>-0.238**</td>
<td>0.072</td>
<td>-0.310***</td>
<td>-0.259***</td>
<td>-0.048</td>
<td>-0.210***</td>
</tr>
<tr>
<td></td>
<td>(0.091)</td>
<td>(0.069)</td>
<td>(0.089)</td>
<td>(0.072)</td>
<td>(0.053)</td>
<td>(0.072)</td>
</tr>
<tr>
<td>Economic insecurity x Perceived</td>
<td>1.22***</td>
<td>0.138</td>
<td>1.08**</td>
<td>2.69**</td>
<td>0.328</td>
<td>2.36**</td>
</tr>
<tr>
<td>foreign-born share</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.260)</td>
<td>(0.236)</td>
<td>(0.386)</td>
<td>(1.26)</td>
<td>(0.715)</td>
<td>(0.926)</td>
</tr>
<tr>
<td>Age x Perceived foreign-born share</td>
<td>-0.058*</td>
<td>0.018</td>
<td>-0.075</td>
<td>-0.022</td>
<td>-0.036</td>
<td>0.014</td>
</tr>
<tr>
<td></td>
<td>(0.032)</td>
<td>(0.026)</td>
<td>(0.046)</td>
<td>(0.054)</td>
<td>(0.031)</td>
<td>(0.059)</td>
</tr>
<tr>
<td>Education x Perceived foreign-born</td>
<td>-2.50</td>
<td>-1.23*</td>
<td>-1.27</td>
<td>-1.76</td>
<td>-0.698</td>
<td>-1.06</td>
</tr>
<tr>
<td>share</td>
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<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(2.08)</td>
<td>(0.644)</td>
<td>(1.88)</td>
<td>(1.60)</td>
<td>(1.20)</td>
<td>(1.90)</td>
</tr>
<tr>
<td>Eurozone x Perceived foreign-born</td>
<td>0.926**</td>
<td>-0.171</td>
<td>1.10**</td>
<td>1.15***</td>
<td>0.410*</td>
<td>0.742**</td>
</tr>
<tr>
<td>share</td>
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<td></td>
<td></td>
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<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.445)</td>
<td>(0.283)</td>
<td>(0.413)</td>
<td>(0.311)</td>
<td>(0.218)</td>
<td>(0.308)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.141</td>
<td>0.316</td>
<td>-0.174</td>
<td>0.278</td>
<td>-0.624*</td>
<td>0.902</td>
</tr>
<tr>
<td></td>
<td>(0.282)</td>
<td>(0.262)</td>
<td>(0.429)</td>
<td>(0.532)</td>
<td>(0.371)</td>
<td>(0.717)</td>
</tr>
<tr>
<td>Observations</td>
<td>34</td>
<td>34</td>
<td>34</td>
<td>111</td>
<td>111</td>
<td>111</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.688</td>
<td>0.510</td>
<td>0.716</td>
<td>0.388</td>
<td>0.320</td>
<td>0.470</td>
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Note: In columns (1) - (3), standard errors corrected by clustering on country are reported in parentheses. In columns (4) - (6), robust standard errors are reported in parentheses. Columns (1) - (3) use country-level data constructed from ESS waves 1 and 7, while columns (4) - (6) use region-level data constructed from ESS wave 7 only. Regressions in columns (3) - (6) include only regions for which variable estimates were calculated using more than 117 underlying individual survey respondents (117 is five percent of the largest region sample size).

*** p<0.01, ** p<0.05, * p<0.1
Returns to Schooling in Indonesia: A Household Consumption Approach

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ABSTRACT
This paper uses household consumption to estimate the returns to schooling in Indonesia. I use school availability, proxied by the distance to the nearest junior high school, as an instrument to overcome endogeneity concerns over schooling decisions. I find that an additional year of schooling is associated with a 5.0 - 9.3% increase in consumption. The richness of the household data also allows me to explore the effect of assortative matching, where couples of similar educational qualifications form unions, on evaluating the returns to schooling. I find evidence suggesting its significance, with higher educated individuals experiencing a stronger effect. The effect of education on the components of household consumption is highly heterogeneous, ranging from -19.3% (alcohol and tobacco) to 28.3% (transportation).

1 Acknowledgements: The author would like to thank his supervisor, Dr Daniel Reck, for his patient guidance and critical feedback, as well as Dr Matthew Levy, Dr Rachael Meager, Dr Judith Shapiro, Benjamin Toh Jun Hui, and all EC331 seminar participants for their time, input and kind words of encouragement.
1. Introduction

Education has been widely acknowledged as an important instrument for sustainable poverty alleviation (Ribich, 1968; Jung and Thorbecke, 2003). As compared to transfer programs, promoting access to education is not mired by disincentivisation side effects, such as reduced employment (Lemieux and Milligan, 2008). This paper contributes to the literature in the following ways. First, it provides an estimate for the return to schooling in Indonesia using an alternative proxy for welfare -- household per capita consumption. Next, I will examine the use of school availability, in particular distance to the nearest junior high school, as an instrument for education. Third, the availability of spousal data grants me the opportunity to examine the effect of educational assortative matching on consumption, a relatively unexplored area in the returns to schooling literature. Lastly, I look at how increased years of education affects the consumption of various components of household consumption.

Compared to existing estimates between 6-11%, the OLS estimate for my data is lower at 5.0%. Controlling for ability had little effect on the estimate. Using an IV model, on the other hand, produced an estimate of 9.3%, almost double of the OLS estimate. I also found that assortative matching has a significant effect on welfare, and its effect is stronger for educated individuals who tend to marry other highly-educated individuals. The effect of education on the components of consumption is extremely heterogeneous, ranging from -20.1% (alcohol and tobacco) to 28.5% (transportation).

2. Existing Literature

There is a wealth of literature dedicated to verifying the return to schooling. The majority uses income as a direct measure for welfare, with the estimated return to schooling ranging from 6% to 11% (see Angrist and Krueger, 1991; Ashenfelter and Krueger, 1994; Harmon and Walker, 1995). In the context of Indonesia, Duflo (2001) examined how the construction of over 61,000 primary schools in the 1970s affected education and income. She estimated the economic return to schooling to be between 6.8% and 10.6%. However, income is considerably susceptible to transitory shocks. As a result, temporarily high or low incomes may not be representative of the true position of the household when borrowing or saving is allowed to smooth the stream of consumption (Blundell and Preston, 1998).

3. Conceptual Framework

In this section, I present a simple theoretical model linking consumption and education using the Mincer earnings function, Friedman's permanent income hypothesis, and Modigliani's life-cycle hypothesis.

The classic approach to verifying the returns to schooling is the use of the canonical Mincer earnings function (1974) where $w$ is the income received, $s$ is the years of schooling, $x$ is the years of labour experience and $\varepsilon$ is a conditional mean zero residual $E(\varepsilon|s,x) = 0$.

$$\ln w(s,x) = \alpha_0 + \rho s + \beta_0 x + \beta_1 x^2 + \varepsilon$$

On the other hand, Friedman's permanent income hypothesis (1957) states that an individual’s income can be treated as a sum of two components: a permanent component $y_p$ and a transitory component $y_t$, which can be expressed as:

$$y = y_p + y_t$$

The permanent component, which I shall call the permanent income, captures the effects of the individual’s characteristics, including human capital and work experience. The transitory component can be interpreted as absorbing the effects of all other factors which occurred at 'random', such as weather shocks and cyclical fluctuations in economic activity. The Mincer earnings function can then be applied to approximate the relationship between the permanent income and years of schooling:

$$\ln y_p = \alpha_0 + \rho s + \beta_0 x + \beta_1 x^2$$

Next, I apply Modigliani and Brumberg’s life-cycle hypothesis (1954) by assuming that there is a perfect credit market which allows saving and dis-saving, that all households practice consumption smoothing perfectly, and that their consumption...
decisions are based solely on their permanent income. Consumption can then be expressed as a function of the permanent income i.e. \( c = f(y_p) \). Assuming a linear relationship between consumption and income for simplicity, we can then express the relationship between consumption and schooling as:

\[
\ln c = f(s, x) = \alpha + \beta s + \gamma_1 x + \gamma_2 x^2
\]

Acknowledging that this is an over-simplified model of the relationship between consumption and schooling, this paper does not seek to pin down the exact mechanisms through which schooling affects consumption levels. Instead, the focus is to identify the effects of education on welfare levels, as measured by the level of household consumption.

### 3.1 Context: education in Indonesia

The focus of my research will be on Indonesia, which has more than 60 million students and almost 4 million teachers in over 340,000 educational institutions. Over the past two decades, the country has recorded strong economic growth accompanied by reduced output volatility and relatively stable inflation.

### 3.2 Identification Strategy

This paper has two main objectives. The first is to estimate the effect of education on household consumption. The second is to estimate the effect of education on the composition of household consumption. The main estimating equation for the effect of education on household consumption is:

\[
\ln c_i = \alpha + \beta s_i + \gamma_1 x_i + \gamma_2 x_i^2 + b_i' \delta + \epsilon_i
\]

where \( c_i \) is the household per capita consumption (PCE) for individual \( i \), \( x_i \) is the potential labour experience (age - years of schooling - 6), \( b_i' \) is a vector of controls, \( \delta \) is a vector of the coefficients and \( \epsilon_i \) is the error term.

### 4. Data

#### 4.1 Indonesia Family Life Survey

My paper uses data from the Indonesia Family Life Survey (IFLS), an on-going longitudinal survey in Indonesia conducted by RAND. The sample is representative of about 83% of the population and contains over 30,000 individuals stratified on provinces. 13 out of the 27 provinces in Indonesia were included in the sample. As of 2017, 5 waves of study have been conducted, with each wave consisting of a household survey and a community survey. The household survey contains detailed information on consumption and education at the household and individual levels. Each individual and household is tagged with a unique ID, which allows me to link them across datasets and waves. The location ID allows me to link individuals with data on their communities, a useful feature for an IV strategy.

As the survey does not contain information on parental consumption, I have to manually match parents with children who were sampled in the survey. This is done by accessing the household roster dataset, which includes information about the household members. Each member is listed as one of seventeen possible household members, which include head of the household, spouse to the head of the household, and child. I used IFLS1/1993 (wave 1) to define ‘parents’ and ‘child’. The head of the household and his/her spouse in wave 1 are defined as the parents. I do not make distinctions between a male and a female household head for wave 1. For convenience, I would label the household head as the father and the spouse as the mother in the regression tables. I then extracted the educational information of the parents and their household consumption information. The figures are then re-based to 2007 prices using CPI data obtained from Badan Pusat Statistik, the government body responsible for conducting statistical surveys. Next, I filtered out the children from each household in wave 1. The children are then linked with their parents’ consumption and educational data using the unique household IDs from wave 1. Next, I use IFLS4/2007 (wave 4) for the children’s consumption and educational data. I limited my sample of children to those above the age of 25, which is the age at which the number of years of the schooling is expected to stabilise.
4.2 Educational data

Three issues arose when preparing the education data. The first is the distinction between secular and non-secular schools. In the household survey, household members are asked the highest level of school attended as well as the type of school. As non-secular schools follow the same structure and curriculum of the general education system, I treated the education received from both types of school as equal. In addition to general education schools, there are also vocational schools. The base specification treats the education received from both types of schools as equal. Alternative specifications allow for different effects from attending different types of schools. Due to the small number of respondents who attended master’s and above levels of schooling, the top coding for education is undergraduate. Adult education, open university and special education are not coded as contributing to the years of education. Table 1 presents the summary statistics for the main variables.

Table 1. Summary Statistics

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
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<td>N</td>
<td>mean</td>
<td>sd</td>
<td>min</td>
<td>max</td>
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<tr>
<td>Consumption</td>
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<td>572,737</td>
<td>543,625</td>
<td>49,733</td>
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<td>Education (Years)</td>
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<td>16</td>
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<td>Primary = 1</td>
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<td>Junior High = 1</td>
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<td></td>
<td></td>
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<td>Senior High = 1</td>
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<td></td>
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<td></td>
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<tr>
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<td>320.916</td>
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<td>5,337,556</td>
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<td>Father’s Education</td>
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<td>Ability Test Score (Out of 15)</td>
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<td>3.10</td>
<td>0</td>
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<td>IV Model</td>
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<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
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<td>2.22</td>
<td>2.50</td>
<td>0</td>
<td>22.5</td>
</tr>
</tbody>
</table>

*a Consumption figures are at the individual monthly level and in IDR at 2007 prices.
*b Vocational and non-vocational dummies are mutually exclusive sets.

5. Results

5.1 OLS

The first regression model that I run is an OLS. In this model, I assume that schooling decisions are exogenous conditional on household and parental characteristics:

$$\ln c_i = \alpha + \beta s_i + \gamma_1 x_i + \gamma_2 x_i^2 + b_i \delta + \epsilon_i$$

where $c_i$ is the household per capita consumption for individual $i$, $x_i$ is the potential labour experience (age-years of schooling-6), $b_i$ is a vector of individual, parental, and province controls, and $\delta$ is a vector of the coefficients. The controls include the natural log of parents’ per capita household consumption, which gives the coefficient on the variable an elasticity interpretation and parents’ education levels. I also included dummy variables for each of the 13 provinces surveyed to account for provincial differences. The coefficient on education in the regression with controls is 5.0%, suggesting the presence of positive omitted variable bias. The coefficient on education is relatively stable to the inclusion of province
dummies, suggesting provincial differences do not account for a considerable part of the variation in consumption. The elasticity between parents’ consumption and children’s consumption is estimated at 0.16. Compared to existing estimates of inter-generational correlation of wealth which used income data (0.27 in Denmark (Adermon et. al, 2016), 0.37 in the U.S. (Charles and Hurst, 2003), the estimate shows greater inter-generational mobility. This means that parental consumption level only has a small effect on an individual’s level of consumption. The effect of household size on consumption is also statistically significant. Adding an additional household member is associated with a 12% fall in household per capita consumption, which can be explained by household economies of scale in consumption (Nelson, 1988).

5.2 Correcting for ability bias
Differences in individual abilities may violate the exogenous schooling assumption. To proxy for ability, I use cognitive tests administered as part of the survey. The model can then be modified to account for the differences in individual ability:

$$\ln c_i = \alpha + \beta s_i + \gamma_1 x_i + \gamma_2 x_i^2 + b_i'\delta + \delta A_i + \epsilon_i$$

where $A_i$ is the standardised test score of individual $i$. Following the inclusion of this measure of ability, there is a moderate fall in the coefficient of education which suggests the presence of positive ability bias, in line with predictions. A one standard deviation increase in score is associated with a 3.6% increase in consumption. The small number of questions on the ability test (only 13) and possible selection into taking the test since the test is not compulsory for survey respondents may threaten both the internal and external validity of the results. In view of such a possibility, I do not include the test scores as a control in the subsequent sections. In the absence of a control for ability, I would expect an upward bias in my estimates for the returns to schooling.

5.3 School availability as an instrument
5.3.1 Distance to the nearest junior high school
In the same vein as Card’s (1993) use of proximity to college as an instrument for educational attainments, I will be exploiting variations in accessibility to junior high education across communities as an instrument for years of schooling.

The motivation behind choosing junior high school as an instrument instead of other grade levels has three dimensions. First, junior high school is a relatively easy transition from primary school in terms of academic difficulty. As a result, the decision to attend junior high school is less likely to be contaminated by ability bias as compared to other grade levels (e.g. from senior high school to university). Second, completing junior high school provides a statistically significant increase in consumption. Thus, variations in the decision to attend junior high school are likely to impact consumption levels. Lastly, most people in the sample who attended high school achieved senior high school and above levels of qualification. Only a relatively small proportion drops out at junior high school level. Attending junior high school may thus have a disproportionate effect on an individual’s schooling outcomes, as compared to other grade levels.

As there is no information on the distance between the nearest junior high school and each individual household, I am exploiting the variation in access to junior high school at the community level. The IFLS community survey provides the distance between the nearest junior high school and the community office. Since there are only an average of 20 households in each of the 312 communities, I would not expect large variations in access to junior high school within a community. Thus, I would argue that this distance is a good proxy for an individual household’s access to education. Additionally, even if the households are geographically scattered within a community, the control for household characteristics should capture most of the differences (e.g. richer households are located nearer to village centres where the office is located).

5.3.2 IV model
The data used for the IV strategy originate from the IFLS1/1993 community survey. The heads of each of the 312 communities were asked about the distances between the village head office and the schools. The IV model is as follows:

First stage:

$$s_i = \pi_0 + \pi_1 d_i + \pi_2 x_i + \pi_3 x_i^2 + b_i'\delta + \mu_i$$
Reduced form:

\[ \ln c_i = \beta_0 + \beta_1 d_i + \gamma_1 x_i + \gamma_2 x_i^2 + b_i ' \rho + \epsilon_i \]

where \( c_i \) is the household per capita consumption for individual \( i \), \( d_i \) is the distance to the nearest school from the village head office, \( x_i \) is the potential labour experience (age - years of schooling - 6), \( b_i ' \) is a vector of controls, \( \delta \) and \( \rho \) are vectors of the coefficients to the controls in the first stage and reduced form respectively. In addition to the controls from the previous sections, I also included controls for the number of siblings and gender due to potential causal effects on educational decisions in the first stage (see Butcher and Case, 1994 and Klasen, 2002).

### 5.3.3 Results

The results are shown in Table 2. The first stage regression to check for relevance produced a F-statistic of 7.2, lower than the heuristic of F-statistic=10. I also employed the Anderson-Rubin test (1949) as a further weak instrument check, which produced a confidence interval of [0.0172, 0.169]. Even though the coefficient for education is within the confidence interval, the wide interval suggests that my estimate for the effect of education is quite imprecise.

The first stage regression shows that a one standard deviation increase in distance to the nearest junior high school is associated with 0.32 fewer years of schooling. Compared with the OLS estimate of approximately 4.9% in column (1), the IV produced an estimate double of that. The IV estimate, however, is imprecise and is only significant at 10% level.

In an attempt to improve the precision of my estimates, I limited my sample to those with ≥6 years of education in columns (5) - (8), which excludes approximately 400 observations out of a total of about 3,600. According to Imbens and Angrist (1994), the 2SLS identifies the Local Average Treatment Effect (LATE), which is the average effect of distance to school on the compliers (i.e. those who change their decision from non-enrolment to enrolment due to the close proximity to school). Under the reasonable assumption that there are no defiers (ie. those who do not enrol when they are near to school and enrol when they are far from school), the useful variation in years of schooling (signal) originates solely from the compliers. The unhelpful variation (noise), on the other hand, is contributed by the never-takers (ie. those who would not enrol no matter what), and the always-takers (ie. those who would always enrol). Assuming that those with <6 years of schooling are the never-takers, removing them from the sample would increase the precision of my estimates by reducing the noise without introducing selection bias.

The OLS regression in column (5) shows a significantly higher return to schooling for the sub-sample (5.96% vs 4.89%), which is due to the different interpretation of the coefficient on education. Instead of estimating the effect from an additional year of education averaged over 16 years, the new coefficient in columns (5) and (8) reflects the effects from an additional year of post-primary education. Running the first stage in column (6) produced a comparable estimate for the effect of distance on education as that in column (2). The statistical significance remains at 5% significance level. Obvious differences can be observed in the reduced form and 2SLS regressions. There is an increase in the effect of distance on consumption, and education on consumption. The coefficient on distance becomes more statistically significant at 5% significance level in the sub-sample. Similarly, the coefficient on education in column (8) increases in statistical significance to 1%, from the previous significance level of 10% in column (4). There are two possible reasons for the differences: the reduction in noise from the first stage as the never-takers are removed from the sample, and the partialling-out of the effect of additional primary education from the average return to schooling.
5.3.4 Discussions of the validity of the instrument

The key identifying assumption for the use of distance to the nearest junior high school as an instrument is that its effect on household consumption is solely through educational decisions. This assumption could be violated if parents choose to live in communities nearer to junior high schools and if they have children who display higher academic abilities which affect their earning potential. As discussed earlier, the 2SLS estimates the LATE. The compliers are most likely to be students from poorer socio-economic backgrounds where distance to school would constitute a significant cost in assessing the net value of education. For example, students from more well-off households would be able to circumvent long distances by paying for transportation. I would expect the compliers, who come from poor economic backgrounds, to have limited flexibility in the choice of location. Additionally, parents who would move nearer to school obviously place a high value on education. It is not far-fetched to predict that their children would be the always-takers, given that an individual’s early life educational decisions are largely dictated by their parents. Thus, parental attitude is unlikely to affect the schooling decision for the compliers and bias the estimate for the effect of education on consumption.

Assuming that the instrument is valid, the higher 2SLS estimates relative to the OLS estimates suggest that students from poorer socio-economic backgrounds enjoy greater returns from schooling than the average individual. On the other hand, if students from poorer socio-economic backgrounds in fact experience the same returns to schooling as an average individual, the doubling of the estimate for the return to schooling in the 2SLS suggests significant endogeneity in schooling decisions, which results in a negative bias for the OLS estimates.

5.4 Assortative matching

Assortative matching is the phenomenon where individuals choose partners based on either the similarities (positive assortative matching) or differences (negative assortative matching) in their characteristics. I formulated a model which
attempts to separate out the effect of assortative matching on the estimates for the returns to schooling. Rather than providing an indicative estimate of the effect of assortative matching, the purpose of this model is aimed at verifying the importance of the assortative matching channel in influencing household consumption. In particular, I am looking for evidence of positive assortative matching, as suggested by Becker (1974) and Mare (1991). The general specification of my model is as follows:

$$\ln c_i = \alpha + \beta_1 \text{education}_i + \beta_2 \text{spouse education}_i + \beta_3 \text{education}_i \times \text{spouse education}_i + \gamma_1 x_i + \gamma_2 x_i^2 + b'_i \delta + \epsilon_i$$

$\beta_1$ and $\beta_2$ are the effects of the individual’s education and spouse’s education on household consumption respectively. To simplify the analysis, I categorised individuals and their spouses into two groups: high-educated, where the individual completed senior high school and above level of schooling, and non-high-educated, where the individual completed below senior high school level of schooling. The variables education and spouse education are dummy variables which turn on if the individual falls into the high-educated category. The variable of interest is $\beta_3$, as it indicates the marginal effect of marrying an equally-educated spouse on consumption, after controlling for both the individual’s and spouse’s individual effects on consumption.

After controlling for an individual’s level of schooling, marrying an equally-educated spouse has a two-fold effect: the higher earning ability of the spouse, and the interaction between an individual and his/her spouse’s high education level. Summing the three coefficients gives $0.47$, higher than the estimate obtained in the regression with only the individual’s education level. Overall, the results lend support for the importance of the assortative matching channel when examining the returns to education for high-educated individuals. Highly-educated couples experience significant gains from assortative matching. Returns to schooling studies which do not account for this channel may understate the welfare returns from education, especially for high-educated individuals.

5.5 Examining the effects of education on the components of household consumption

In the previous sections, I have examined the effects of education on household welfare, as measured by changes in household per capita consumption. In this section, I aim to explore the effects of education on specific components of household consumption. I grouped household consumption into two main categories: “Food” and “Non-food”.

The estimating model is as follows:

$$f_{gi} = \alpha_g + \beta_g s_i + \theta_g x_i + \theta_g x_i^2 + b'_i \delta + \epsilon_{gi}$$

where $f_{gi}$ is the log-consumption of item $g$ for individual $i$, $s_i$ the years of schooling for individual $i$, $x_i$ is the number of years of potential labour experience for individual $i$, and $b'_i$ is a vector of controls for individual characteristics. The same controls from the original OLS regression are included (household and parental characteristics as well as province controls). $\beta_g$ can be interpreted as a rough estimate for the income elasticity of demand for item $g$. The results are summarised in charts.

The results in food items are shown in Figure 1. Staple food is viewed as an inferior good, while dairy, vegetables, meat, 1 The average years of schooling in my sample is 9.5 years.
5.5.1 Food Consumption

Figure 1. Approximate % Change in Food Consumption Associated with an Additional Year of Schooling

Fish, dried food, and eating out are viewed as normal goods. In particular, considering their relatively large magnitudes of increase in consumption, dairy products and eating out could be luxury goods whose consumption increase more than proportionately with respect to income.

5.5.2 Non-food consumption

Figure 2. Approximate % Change in Frequent Non-food Consumption Associated with an Additional Year of Schooling
We observe in Figure 2 that an additional year of schooling is associated with an approximately 8.7% increase in consumption of frequently purchased non-food goods, significantly higher than the increase for the “Food” category (2.9%). The results for infrequently purchased non-food goods and services are summarised in Figure 3. An additional year of schooling is associated with an approximately 7.9% increase in total consumption in this category.

**Figure 3.** Approximate % Change in Infrequent Non-food Consumption Associated with an Additional Year of Schooling

6. Conclusion
This paper studies the effect of education on individual welfare, as measured by household per capita consumption. Building on the Mincer earnings function, Friedman's permanent income hypothesis, and Modigliani's life-cycle hypothesis, I constructed a simple linear model of the effect of education on household consumption. I found that the OLS model produced a smaller estimate as compared to existing literature, which uses income as a measure of welfare. Overall, I estimate the return to schooling in Indonesia to be in the range of 5.0 - 9.3%, where the IV estimate is double that of the OLS estimate. My estimates are slightly lower as compared to Duflo's (2001) study of Indonesia in the 1990s which produced estimates between 6.8 - 10.6%. I also explored the assortative matching channel for return to schooling and found it to have a positive effect on consumption as equally-educated individuals form unions. Lastly, I found significant heterogeneity in the effects of education on various components of household consumption. The increase in consumption of food items is relatively modest (2.9% for an additional year of education) as compared to the increase in transportation (28.3%) and recreation (19.6%) expenditures.

The paper, however, has its limitations. I can only construct a simple linear model linking education with consumption. Becker (1962, 1965, 1966, 1974, 1975) found household production to be a complex process, which my model does not capture. This is an area where further research is needed, especially given the significance of the assortative matching channel. Additionally, the use of self-reported consumption data raises concerns of measurement errors, which may result in complications if the errors are correlated with an individual's level of education. I would suggest the use of alternative datasets to verify the extent of this error. In my paper, I assume homogeneity in the quality of education. In reality, this assumption rarely holds due to significant heterogeneity in school funding and quality of teachers. An extension of this research is to include a control for the quality of schools.

Nevertheless, the paper adds to the limited literature of using consumption data to estimate the returns to schooling. I found consumption to be a viable alternative, especially when there is an absence of high-quality income data. Also, developing countries usually have a significant proportion of their labour forces employed in the informal sector, which makes income data especially susceptible to transitory shocks. Under such a scenario, consumption data may be a better proxy for welfare than income data.
References
Blackburn, McKinley L., and David Neumark. “Omitted-ability bias and the increase in the return to schooling.” *Journal of labor economics* *11* *(3) (1993): 521-544.*


Appendices of the Carroll Round Proceedings
APPENDIX A:  
Presentation Schedule

Friday, April 20, 9:00 – 11:50 am  
Session 1A  
Healy 104  
Chair: Robert Cumby (Professor, Georgetown University)

David Alzate (Georgetown University)  
Poor Targeted Social Programs and Labor Informality in Latin America: The Case of Mexico’s Progresa CCT  
Discussant: Ryan Mather

Ryan Mather (University of Minnesota)  
Determinants of Diversification: A Study of Ecuadorian Exports  
Discussant: Connor Regan

Connor Regan (Dartmouth College)  
Credit Ratings and the Pricing of Sovereign Default Risk: A Retreat to Moderation?  
Discussant: Christina Cheung

Christina Cheung (University of British Columbia)  
Eliminating the Penny in Canada: An Economic Analysis of Penny-Rounding on Grocery Items  
Discussant: David Alzate

Session 1B  
Healy 105  
Chair: Rodney Ludema (Professor, Georgetown University)

Duy Mai (Georgetown University)  
Lobbying Preferences and International Environmental Agreements  
Discussant: Sebastian Senlle

Sebastian Senlle (University of Buenos Aires)  
Public Capital Investment: Does Infrastructure Spur Growth?  
Discussant: Tuyet-Anh Tran

Tuyet-Anh Tran (Macalester College)  
FDI, Political Risk, and Inequality in Host Countries  
Discussant: Duy Mai

Friday, April 20, 2:00 – 4:00 pm  
Session 2A  
Maguire 101  
Chair: Marko Klasnja (Associate Professor, Georgetown University)
Alexander Weinberg (Dartmouth College)
The Effect of Immigration on Political Polarization: Evidence From Europe
Discussant: Stephan Hobler

Stephan Hobler (University of Warwick)
Disappearing Jobs and Displaced Workers: Does Education Matter?
Discussant: Aditya Pande

Aditya Pande (Georgetown University)
Measuring Returns to Innovation: Patent Quality and Federal R&D Spending
Discussant: Alex Weinberg

Session 2B
Maguire 102
Chair: Shareen Joshi (Assistant Professor, Georgetown University)

Quan Xue (London School of Economics and Political Science)
Returns to Schooling in Indonesia: A Household Consumption Approach
Discussant: John Thiemel

John Thiemel (Georgetown University)
The Health and Migration Effects of Rising Groundwater Salinity in Coastal Bangladesh
Discussant: Wessam Kanes

Wessam Kanes (Georgetown University in Qatar)
Returns to Education on Poverty Reduction & Labor Market Outcomes in Rural China
Discussant: Quan Xue

Saturday, April 21, 9:00 – 11:00 am
Session 3A
Healy 103
Chair: Olga Timoshenko (Assistant Professor, George Washington University)

Arjun Krishnan (Georgetown University)
The Effect of Aid on Transparency: IDA Threshold-Crossing as a Quasi-Experiment
Discussant: Jonah Kelly

Jonah Kelly (Dartmouth College)
Jewish Network Effects in Israeli Trade
Discussant: Tristan Byrne

Tristan Byrne (University of Warwick)
Gentrification and Displacement
Discussant: Arjun Krishnan

Session 3B
Healy 104
Chair: Dan Cao (Assistant Professor, Georgetown University)
Daniel Sanchez-Ordonez (Stanford University)
International Monetary Policy Spillover in Colombia: An SVAR Analysis
Discussant: Sitong Ding

Sitong Ding (London School of Economics and Political Science)
Bounded Rationality in Rules of Price Adjustment and the Phillips Curve
Discussant: May Nguyen

May Nguyen (Dartmouth College)
Herd Behavior Among Hedge Funds and Its Effects on Funds’ Returns and Survival Profiles
Discussant: Daniel Sanchez-Ordonez

Saturday, April 21, 1:00 – 3:00 pm
Session 4A
Healy 103
Chair: Christopher Griffin (Research Director, Access to Justice Lab, Harvard Law School)

Olivia Bisel (Georgetown University)
Do Renewable Energy Commitments Benefit Shareholders? A Quasi-Experimental Event Study
Discussants: Darya Labok & Filip Drazdou

Darya Labok and Filip Drazdou (Stockholm School of Economics in Riga)
Raspberries vs Wheat: Economic Sophistication as a New Predictor of Income Volatility
Discussant: Jake Owen

Jake Owen (Lancaster University)
Substituting Matchday Attendance for TV Broadcasts: The Case of the English Football League
Discussant: Olivia Bisel

Session 4B
Healy 104
Chair: Charles Udomsaph (Associate Teaching Professor, Georgetown University)

Beatriz Garcia Quiroga (Universitat Pompeu Fabra)
The Effects on Social Welfare of Emergency Low Emissions Zones: An Application to Barcelona
Discussant: Jonathan Kaufmann

Jonathan Kaufmann (American University)
The Economic Impact of Psychological Distress on Former Child Soldiers
Discussant: Van-Anh Le

Van-Anh Le (Macalester College)
Does Agglomeration Account for Process Innovation in Small and Medium Enterprises in Vietnam?
Discussant: Beatriz Garcia Quiroga
APPENDIX B:

Past Speakers

First Annual Carroll Round
(April 5-7, 2002)
Roger Ferguson, Federal Reserve Board of Governors
Donald L. Kohn, Federal Reserve Board of Governors
Lawrence Lindsey, Assistant to the President and National Economic Council
Edwin M. Truman, Institute for International Economics
John Williamson, Institute for International Economics

Second Annual Carroll Round
(April 11-13, 2003)
R. Glenn Hubbard, Council of Economic Advisers and Columbia University
Donald L. Kohn, Federal Reserve Board of Governors
John Williamson, Institute for International Economics

Third Annual Carroll Round
(April 15-18, 2004)
Donald L. Kohn, Federal Reserve Board of Governors
John F. Nash, Jr., Princeton University (1994 Nobel Laureate)
Peter R. Orszag, The Brookings Institute

Fourth Annual Carroll Round
(April 22-24, 2005)
Ben S. Bernanke, Federal Reserve Board of Governors
William Easterly, New York University
Maurice Obstfeld, University of California, Berkeley
Edwin M. Truman, Institute for International Economics

Fifth Annual Carroll Round
(April 28-30, 2006)
Kemal Dervis, United Nations Development Programme
Thomas C. Schelling, University of Maryland (2005 Nobel Laureate)

Sixth Annual Carroll Round
(April 19-22, 2007)
Grant D. Aldonas, Center for Strategic and International Studies
François Bourguignon, Chief Economist and Senior Vice President of the World Bank
Randall Kroszner, Federal Reserve Board of Governors

Seventh Annual Carroll Round
(April 17-20, 2008)
Susan C. Athey, Harvard University
Philip I. Levy, American Enterprise Institute
Steven Radelet, Senior Fellow at the Center for Global Development
Eighth Annual Carroll Round  
(April 16-19, 2009)  
Eric S. Maskin, Princeton University (2007 Nobel Laureate)  
Nassim Nicholas Taleb, Universa Investments and New York University

Ninth Annual Carroll Round  
(April 22-25, 2010)  
Philip I. Levy, American Enterprise Institute  
Lant Pritchett, Harvard Kennedy School

Tenth Annual Carroll Round  
(April 14-17, 2011)  
Jagdish Bhagwati, Columbia University  
Joseph Stiglitz, Columbia University (2001 Nobel Laureate)

Eleventh Annual Carroll Round  
(April 19-22, 2012)  
Jonathan Levin, Stanford University  
Gene Sperling, Director of National Economic Council

Twelfth Annual Carroll Round  
(April 18-21, 2013)  
John Taylor, Stanford University  
Janet Currie, Princeton University

Thirteenth Annual Carroll Round  
(April 10-13, 2014)  
Peter Diamond, Massachusetts Institute of Technology (2010 Nobel Laureate)  
Martin Ravallion, Georgetown University

Fourteenth Annual Carroll Round  
(April 16-19, 2015)  
Rajiv Shah, United States Agency for International Development  
George Akerlof, Georgetown University (2001 Nobel Laureate)

Fifteenth Annual Carroll Round  
(April 21-24, 2016)  
Daniel Kaufmann, President and CEO of Natural Resource Governance Institute  
Rodney Ludema, Chief Economist at the Department of State and Georgetown University

Sixteenth Annual Carroll Round  
(April 20-23, 2017)  
Jason Furman, Harvard University Kennedy School of Government  
Nobuhiro Kiyotaki, Princeton University
Seventeenth Annual Carroll Round
(April 19-22, 2018)
George Akerlof, Georgetown University (2001 Nobel Laureate)
Augusto Lopez Carlos, World Bank and Senior Fellow at Georgetown University
APPENDIX C:
Former Steering Committees

First Annual Carroll Round  
(April 5-7, 2002)  
Christopher L. Griffin, Chair (SFS ’02)  
William Brady (SFS ’02)  
Cullen Drescher (COL ’04)  
Meredith L. Gilbert (COL ’04)  
Joshua Harris (SFS ’02)  
Andrew T. Hayashi (SFS ’02)  
Mark Longstreth (SFS ’04)  
Kathryn Magee (SFS ’02)  
Ryan Michaels (SFS ’02)  
J. Brendan Mullen (SFS ’02)  
Scott E. Pedowitz (SFS ’02)  
Waheed Sheikh (SFS ’04)

Second Annual Carroll Round  
(April 11-13, 2003)  
Seth Kundrot, Chair (SFS ’03)  
Nada Abdelnour (SFS ’03)  
Maria Arhancet (SFS ’04)  
Victoria Bembenista (SFS ’03)  
Michael Callen (SFS ’05)  
Eric Fischer (SFS ’03)  
Daphney Francois (SFS/GRD ’04)  
Meredith L. Gilbert (COL ’04)  
Jeffrey Harris (COL ’03)  
Robert S. Katz (COL ’04)  
Marina Lafferriere (SFS ’06)  
Lu Shi (SFS ’03)  
Stacey Tsai (SFS ’03)  
Robert Wrobel (SFS ’03)  
Erica Yu (COL ’05)

Third Annual Carroll Round  
(April 15-18, 2004)  
Meredith L. Gilbert, Chair (COL ’04)  
Héber Delgado-Medrano (SFS ’06)  
Ryan Fraser (SFS ’04)  
Tetyana Gaponenko (SFS ’07)  
Yun jung Cindy Jin (SFS ’05)  
Sarah Knupp (SFS ’04)  
Robert S. Katz (COL ’04)  
Marina Lafferriere (SFS ’06)  
Alia Malik (SFS ’04)  
Susan Work (SFS ’04)  
Beatka Zakrzewski (SFS ’04)

Fourth Annual Carroll Round  
(April 22-24, 2005)  
Erica Yu, Chair (COL ’05)  
Jasmina Beganovic (SFS ’05)  
Lucia Franzese (SFS ’07)  
Dennis Huggins (SFS ’05)  
Yun jung Cindy Jin (SFS ’05)  
Jonathan Kirschner (SFS ’05)  
Susan Kleiman (SFS ’05)  
Yousif Mohammad (SFS ’06)  
Amy Osekowsky (SFS ’07)  
Daniel Schier (SFS ’05)

Fifth Annual Carroll Round  
(April 27-30, 2006)  
Marina Lafferriere, Chair (SFS ’06)  
Irmak Bademli (SFS ’06)  
Stephen Brinkmann (SFS ’07)  
Héber Delgado-Medrano (SFS ’06)  
Lucia Franzese (SFS ’07)  
Yasmine Fulena (SFS ’08)  
Jen Hardy (SFS ’06)  
Michael Kunkel (SFS ’08)  
Yousif Mohammed (SFS ’06)  
Emily Reimao (SFS ’06)  
Tamar Tashjian (SFS ’06)

Sixth Annual Carroll Round  
(April 19-22, 2007)  
Stephen Brinkmann, Chair (SFS ’07)  
Lucia Franzese (SFS ’07)  
Nicholas Hartman (SFS ’07)  
Ian Hinsdale (COL ’09)  
Alexander Kostura (SFS ’09)  
Jennifer Noh (SFS ’07)  
Amy Osekowsky (SFS ’07)  
Allison Phillips (SFS ’07)  
Sun Yi (SFS ’07)

Seventh Annual Carroll Round  
(April 17-20, 2008)  
Yasmine Fulena, Chair (SFS ’08)  
Sue Bai (SFS ’08)  
Stacey Droms (COL ’08)  
Brandon Feldman (COL ’08)
LiJia Gong (SFS ’08)
Kory Katenga (SFS ’10)
Sung Kim (SFS ’08)
Michael Kunkel (SFS ’08)
Fuyang Zhang (SFS ’10)

**Eighth Annual Carroll Round**
*(April 16-19, 2009)*
Rebecca Heide, Chair (SFS ’09)
James Arnold (SFS ’11)
Amanda B. Delp (SFS ’12)
Henry Gillam (SFS ’10)
Tom Han (SFS ’10)
Anna Klis (SFS ’10)
Daniel Leonard (SFS ’09)
Daniel Lim (SFS ’11)
Arjun Pant (SFS ’09)
Benjamin Simmons (COL ’09)
Ariell Zimran (SFS ’10)

**Ninth Annual Carroll Round**
*(April 22-25, 2010)*
Ariell Zimran, Chair (SFS ’10)
Michael Counihan (SFS ’11)
Amanda B. Delp (SFS ’12)
Katherine Donato (SFS ’10)
Tom Han (SFS ’10)
Michael Karno (SFS ’11)
Allison Kern (SFS ’10)
Anna Klis (SFS ’10)
Daniel Lim (SFS ’11)
H. Jess Seok (SFS ’12)
Matthew Shapiro (SFS ’11)

**Tenth Annual Carroll Round**
*(April 14-17, 2011)*
Amanda B. Delp, Chair (SFS ’12)
James Arnold (SFS ’11)
Albert Chiang (SFS ’13)
Malin Hu (SFS ’11)
Katrina Kosner (SFS ’12)
Nancy Lee (SFS ’11)
Doug Proctor (SFS ’12)
Vivek Sampathkumar (SFS ’11)
Monica Scheid (SFS ’11)
Matthew Shapiro (SFS ’11)

**Eleventh Annual Carroll Round**
*(April 19-22, 2012)*
Katrina Kosner, Chair (SFS ’12)
Albert Chiang (SFS ’13)
Amanda B. Delp (SFS ’12)
Nhaca Le (SFS ’13)
Doug Proctor (SFS ’12)
Glenn Russo (COL ’13)
H. Jess Seok (SFS ’12)
Anusuya Sivaram (SFS ’12)
Meredith Strike (SFS ’14)
Shuo Yan Tan (SFS ’12)

**Twelfth Annual Carroll Round**
*(April 18-21, 2013)*
Glenn Russo, Chair (COL ’13)
Albert Chiang (SFS ’13)
Meredith Strike (SFS ’14)
Natalie Nah (SFS ’15)
Brian Goggin (SFS ’14)
Heather Hedges (SFS ’14)
Dawn Chan (SFS ’14)
Edward Hedke (SFS ’13)
Elena Malik (SFS ’14)
Stephen McDonald (SFS ’13)
Emily Oehlsen (SFS ’13)

**Thirteenth Annual Carroll Round**
*(April 10-13, 2014)*
Heather Hedges, Chair (SFS ’14)
Brian Goggin (SFS ’14)
Dawn Chan (SFS ’14)
Elena Malik (SFS ’14)
Natalie Nah (SFS ’15)
Jill Ni (SFS ’14)
Kristen Skillman (SFS ’16)
Meredith Strike (SFS ’14)
Christopher Stromeyer (SFS ’14)
Rachel Szymanski (SFS ’14)

**Fourteenth Annual Carroll Round**
*(April 16-19, 2015)*
Kristen Skillman, Chair (SFS ’16)
Natalie Nah (SFS ’15)
Thomas Christiansen (SFS ’16)
Geeva Gopalkrishnan (SFS ’15)
Elle Kang (SFS ’15)
Grace Kim (SFS ’17)
Cheryl Lau (SFS ’16)
Eve Lee (SFS ’15)
Shom Mazumder (SFS ’15)
Morgan Snow (COL ’16)
MaryAnne Zhao (SFS ’16)

Fifteenth Annual Carroll Round
(April 21-24, 2016)
MaryAnne Zhao, Chair (SFS ’16)
Olivia Bisel (SFS ’18)
Audrey Chambers (COL ’19)
Felicia Choo (SFS ’16)
Alexander Colyer (SFS ’17)
Serena Gobbi (SFS ’16)
Elizabeth Johnson (COL ’16)
Grace Kim (SFS ’17)
Duy Mai (SFS ’18)
Harry Rosner (SFS ’18)
Kristen Skillman (SFS ’16)

Sixteenth Annual Carroll Round
(April 21-24, 2017)
Grace Kim, Chair (SFS ’17)
Olivia Bisel (SFS ’18)
Audrey Chambers (SFS ’19)
Tina Cheesman (SFS ’19)
Sang Jun Chun (SFS ’19)
Griffin Cohen (SFS ’17)
Alexander Colyer (SFS ’17)
Grady Killeen (SFS ’18)
Duy Mai (SFS ’18)
Alyson Matthews (SFS ’17)
Harry Rosner (SFS ’18)
Elinor Walker (SFS ’20)

Seventeenth Annual Carroll Round
(April 19-22, 2018)
Olivia Bisel, Chair (SFS ’18)
Shine Aung (SFS ’21)
Grady Killeen (SFS ’18)
Arjun Krishnan (SFS ’18)
Joshua Levy (SFS ’20)
Duy Mai (SFS ’18)
Parker Malarkey (SFS ’19)
Meggie Underwood (SFS ’19)
Elinor Walker (SFS ’20)
Jacob Witt (SFS ’20)
Crystal Zhu (SFS ’19)

Eighteenth Annual Carroll Round
(April 11-14, 2019)
Margaret Underwood, Chair (SFS ’19)
Shine Aung (SFS ’21)
Yi Bao (SFS ’19)
Sang Jun Chun (SFS ’19)
Joshua Levy (SFS ’20)
Chloe Li (SFS ’21)
Victor Li (COL ’20)
Peter Liu (SFS ’22)
Jacob Witt (SFS ’20)
Yuou Wu (SFS ’21)
Alice Ye (COL ’21)
Crystal Zhu (SFS ’19)
Appendix D:

Members of the Advisory Panel

Meredith Gilbert Ballotta, UCSF Medical Center
Christopher L. Griffin, University of Arizona College of Law
Andrew T. Hayashi, The University of Virginia
Mitch Kaneda, Georgetown University
Robert S. Katz, Amazon
J. Brendan Mullen, American College of Cardiology
Scott E. Pedowitz, Arlington Chamber of Commerce
Erica Yu Wright, Bureau of Labor Statistics
Appendix E:

Past Participants

First Annual Carroll Round
(April 5-7, 2002)
Azhar Abdul-Quader, Columbia University
Santosh Anagol, Stanford University
William Brady, Georgetown University
Daniel Braun, Oberlin College
Jacqueline Bueso, University of Pennsylvania
Karla Campbell, The University of Virginia
Benn Eifert, Stanford University
Courtney Fretz, University of Pennsylvania
Carlos Galvez, Stanford University
Aniruddha Gopalakrishnan, Duke University
Christopher Griffin, Georgetown University
Casey Hanson, Lehigh University
Joshua Harris, Georgetown University
Andrew Hayashi, Georgetown University
Marco Hernandez, Massachusetts Institute of Technology
Katia Hristova, Illinois-Wesleyan University
Maria Jelescu, Massachusetts Institute of Technology
Fadi Kanaan, Yale University
Avinash Kaza, Stanford University
Vinay Kumar, Duke University
Anisha Madan, Illinois-Western University
Kathryn Magee, Georgetown University
Ryan Michaels, Georgetown University
Jack Moore, Stanford University
Brendan Mullen, Georgetown University
Andrei Muresianu, Brown University
Scott Orleck, Duke University
Scott Pedowitz, Georgetown University
Jonathan Prin, University of Pennsylvania
Jeremy Sandford, Illinois-Western University
Deborah Slezak, Illinois-Western University
Conan Wong, Brown University

Second Annual Carroll Round
(April 11-13, 2003)
Nada Abdelnour, Georgetown University
Amanda Barnett, Emory University
Andrea Bell, Wellesley College
Patrick Byrne, University of Colorado
David Chao, Cornell University
Sylvia Ciesluk, Lehigh University
Adam Doverspike, Georgetown University
Benn Eifert, Stanford University
Adam Engberg, Georgetown University
Alexandra Fiorillo, Connecticut College
Eric Fischer, Georgetown University
Zlata Hajro, Wellesley College
Samina Jain, Georgetown University
Avinash Kaza, Stanford University
Eric Kim, The George Washington University
Seth Kundrot, Georgetown University
Lada Kyi, Rice University
Lee Lockwood, Northwestern University
Sunil Mulani, New York University
Holly Presley, Vanderbilt University
Duncan Roberts, University of California, Berkeley
Lu Shi, Georgetown University
Shanaz Taber, Barnard College
Jiang Wei, University of Michigan

Third Annual Carroll Round
(April 15-18, 2004)
Jeffrey Arnold, Dartmouth College
Julia Berazneva, Mt. Holyoke College
Mehmet Cangul, Georgetown University
Richard Carew, The University of Virginia
Ashley Coleman, Vanderbilt University
Dilyana Dimova, Stanford University
Fernando Galeana, Stanford University
M. Blair Garvey, Emory University
Meredith Gilbert, Georgetown University
Adam Greeney, Oberlin College
Asim Gunduz, The University of Virginia
Marc Hafstead, Northwestern University
Andrew Hayashi, University of California, Berkeley
Katherine Howitt, McGill University
Sohini Kar, Columbia University
Josh Lewis, Illinois-Western University
Alexis Manning, Illinois-Western University
Sara Menker, Mt. Holyoke College
Elizabeth Mielke, Vanderbilt University
Stratos Pahis, Dartmouth College
Alicja Pluta, Georgetown University
David Chao, Cornell University
Sylvia Ciesluk, Lehigh University
Adam Doverspike, Georgetown University
Benn Eifert, Stanford University
Adam Engberg, Georgetown University
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Lada Kyi, Rice University
Lee Lockwood, Northwestern University
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Shanaz Taber, Barnard College
Jiang Wei, University of Michigan

Appendix E:

Past Participants

First Annual Carroll Round
(April 5-7, 2002)
Azhar Abdul-Quader, Columbia University
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Maria Jelescu, Massachusetts Institute of Technology
Fadi Kanaan, Yale University
Avinash Kaza, Stanford University
Vinay Kumar, Duke University
Anisha Madan, Illinois-Western University
Kathryn Magee, Georgetown University
Ryan Michaels, Georgetown University
Jack Moore, Stanford University
Brendan Mullen, Georgetown University
Andrei Muresianu, Brown University
Scott Orleck, Duke University
Scott Pedowitz, Georgetown University
Jonathan Prin, University of Pennsylvania
Jeremy Sandford, Illinois-Western University
Deborah Slezak, Illinois-Western University
Conan Wong, Brown University

Second Annual Carroll Round
(April 11-13, 2003)
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Seth Kundrot, Georgetown University
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Lee Lockwood, Northwestern University
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Fernando Galeana, Stanford University
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Meredith Gilbert, Georgetown University
Adam Greeney, Oberlin College
Asim Gunduz, The University of Virginia
Marc Hafstead, Northwestern University
Andrew Hayashi, University of California, Berkeley
Katherine Howitt, McGill University
Sohini Kar, Columbia University
Josh Lewis, Illinois-Western University
Alexis Manning, Illinois-Western University
Sara Menker, Mt. Holyoke College
Elizabeth Mielke, Vanderbilt University
Stratos Pahis, Dartmouth College
Alicja Pluta, Georgetown University
David Chao, Cornell University
Sylvia Ciesluk, Lehigh University
Adam Doverspike, Georgetown University
Benn Eifert, Stanford University
Adam Engberg, Georgetown University
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Samina Jain, Georgetown University
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Eric Kim, The George Washington University
Seth Kundrot, Georgetown University
Lada Kyi, Rice University
Lee Lockwood, Northwestern University
Sunil Mulani, New York University
Holly Presley, Vanderbilt University
Duncan Roberts, University of California, Berkeley
Lu Shi, Georgetown University
Shanaz Taber, Barnard College
Jiang Wei, University of Michigan

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Fourth Annual Carroll Round
(April 22-24, 2005)
Lidia Barabash, Dartmouth College
Jasmina Beganovic, Georgetown University
Xun Bian, Illinois-Wesleyan University
Michael Furchtgott, Columbia University
Michael Gechter, Pomona College
Kevin B. Goldstein, Dartmouth College
Michael Haase, University of Copenhagen
Dennis Huggins, Georgetown University
Michael Insel, Claremont McKenna College
Jonathan Kirschner, Georgetown University
Shiying Lee, Duke University
James Liao, Dartmouth College
Brian Lichter, Washington University in St. Louis
Wee Lee Loh, Cornell University
Alice Luo, Duke University
Katharine Mullock, University of Western Ontario
Jose Mustre del Rio, The Ohio State University
Leah Nelson, Georgetown University
Ee Cheng Ong, Wellesley College
Matthew Phan, Columbia University
Nina Rendelstein, Washington University in St. Louis
David Rogier, Washington University in St. Louis
Ana Maria Romero, Illinois-Wesleyan University
Nathan Saperia, Dartmouth College
Bogdan Tereshchenko, Georgetown University
Olga Timoshenko, University of Western Ontario
Tom Vogl, Princeton University
Kenneth Ward, The University of Chicago
Jonathan Wolfson, Washington University in St. Louis
Suzanne Zurkiya, Emory University

Fifth Annual Carroll Round
(April 27-30, 2006)
Sarah Carroll, Stanford University
Ruth Coffman, Georgetown University
Dubravka Colic, Wellesley College
Pratik Dattani, University of Warwick
Jennifer Dawson, Illinois-Wesleyan University
Héber Delgado-Medrano, Georgetown University
Sherri Haas, Illinois-Wesleyan University
Jen Hardy, Georgetown University
Lauren Iacocca, University of California, Los Angeles
Salifou Issoufou, University of Wisconsin-Madison
Stella Klemperer, Brown University
Daniel Kurland, Dartmouth College
Corinne Low, Duke University
Shanthi Manian, Georgetown University
Michael Monteleone, The University of Chicago
John Nesbitt, Georgetown University
Natasha Nguyen, University of California, Berkeley
Oyebanke Oyeyinka, Carleton College
Evgeniya Petrova, Dartmouth College
Emy Reimao, Georgetown University
Svetoslav Roussanov, Columbia University
Vikram Shankar, Georgetown University
Juan Carlos Suarez, Trinity University
Austin Vedder, Dartmouth College
David Wiczer, Carleton College
Geoffrey Yu, Carleton College
Xiaoti Zhang, University of Warwick

Sixth Annual Carroll Round
(April 19-22, 2007)
Matthew Adler, Oberlin College
Marion Aouad, Princeton University
Stephen Brinkmann, Georgetown University
Erik Eggum, University of Warwick
Lucia Franzese, Georgetown University
Tanja Groth, University of St. Andrews
Ashley Halpín, Dartmouth College
Nicholas Hartman, Georgetown University
Adrienna Huffman, Washington University in St. Louis
Abdulla Humaidan, University of Warwick
Mohammad Huq, Georgetown University
Nedko Kyuchukov, Dartmouth College
Zachary Mahone, New York University
R. Priya Mathew, Washington University in St. Louis
Yana Morgulis, The University of Chicago
Jennifer Noh, Georgetown University
Andrew O’Brien Penney, Georgetown University
Jessica Oliveri, Monash University
Matthew Pech, Dartmouth College
Allison Phillips, Georgetown University
Angelica da Rocha, University of Warwick
Sören Radde, University of Bayreuth
Heleri Rande, New York University
Elena Spatoulas, University of Michigan
Yi Sun, Georgetown University
Bennett Surajat, Carleton College
Freddy Tsai, University of British Columbia
David Wolff, Dartmouth College
Jennifer Xi, Dartmouth College
Cynthia Yim, Princeton University

Seventh Annual Carroll Round
(April 17-20, 2008)
Karl Andres, University of Warwick
Cecil Ang, The University of Virginia
Alaina Antonucci, The Pennsylvania State University
Sue Bai, Georgetown University
Marinella Boyadzhiev, Oberlin College
Quentin Brummet, Illinois-Wesleyan University
Brendan Cooper, Carleton College
Gerard DiPippo, Dartmouth College
Stacey Droms, Georgetown University
Varun Dutt, Macalester College
Yasmine Fulema, Georgetown University
Amish Gandhi, University of Warwick
Katherine Gordon, Mt. Holyoke College
Yi Kang, Wesleyan College
Michael Kunkel, Georgetown University
Han Youp Lee, Georgetown University
Claudio LoCascio, Dartmouth College
Olivia Lynch, Georgetown University
Amr Moubarak, The George Washington University
Simone Nitsch, University of Warwick
Saurabh Pant, New York University
Carson Sherwood, University of Western Ontario
Tadashi Shirai, University of Warwick
Dominique Shure, Georgetown University
William Slater, Vanderbilt University
Shyam Sundaram, Brown University
Poh Lin Tan, Princeton University
Dorothy Voorhees, Georgetown University
Kris Walsh, Georgetown University
Monica Yu, Dartmouth College

Eighth Annual Carroll Round
(April 16-19, 2009)
Jennifer Cairns, Calvin College
David Childers, Georgetown University
Vaska Dimitrova, American University in Bulgaria
Rebecca Freeman, Smith College
Georg Graetz, London School of Economics and Political Science
Markus Gstoettner, London School of Economics and Political Science
Arpit Gupta, The University of Chicago
Frederick Haney, New York University
Rebecca Heide, Georgetown University
Gregory Howard, The University of North Carolina at Chapel Hill
Jacqueline Iwata, The George Washington University
Anders Jensen, London School of Economics and Political Science
William Kafoure, The George Washington University
Elira Kuka, Wellesley College
Daniel Leonard, Georgetown University
Chris Lim, Dartmouth College
Juan Ignacio Elorrieta, Maira University of Chile
Nick Marchio, Macalester College
Hekuran Neziri, American University in Bulgaria
Casey Oswald, Georgetown University
Arjun Pant, Georgetown University
Caitlin Pierce, Dartmouth College
Isra Salim, Macalester College
Keval Sangani, University of Warwick
Pronita Saxena, University of California, Berkeley
Benjamin Simmons, Georgetown University
Maximilian Sirianni, Macalester College
Seitaro Takarabe, Wesleyan University
Fabien Thayamballi, Georgetown University
Rachel Winograd, Dartmouth College
Woan Foong Wong, Oberlin College

Ninth Annual Carroll Round
(April 22-25, 2010)
Jorge Aponte, Georgetown University
Benjamin Arnold, University of Michigan
Courtney Blair, Harvard University
Vera Chau, New York University
Nick Chantraporn, University of San Francisco
Antonina Davydenko, American University in Bulgaria
Katherine Donato, Georgetown University
Yang Du, Dartmouth College
Siddharth Eapen George, London School of Economics and Political Science
Takuma Habu, University of Warwick
Kelsey Hample, Illinois-Wesleyan University
Tom Han, Georgetown University
Rob Harris, University of Warwick
Sarah Hinkfuss, Harvard University
Peter Hull, Wesleyan University
Michael Karno, Georgetown University
Todd Kawakita, Dartmouth College
Allison Kern, Georgetown University
Anna Klis, Georgetown University
Birgit Leimer, New York University
Daniel Lim, Georgetown University
Benjamin Morley, University of Warwick
In Un Flora Ng, Dartmouth College
Katharine Ng, University of San Francisco
Xing Cong Ong, London School of Economics and Political Science
Hang Qian, Dartmouth College
Paul Unanue, Princeton University
Ahmad Wahdat, Oberlin College
Ariell Zimran, Georgetown University

Bulgaria
Daniel Boada, Harvard University
Gustavo Camilo, New York University
Daniel Chan, United States Naval Academy
Meryl Ching, University of Warwick
Kimberly Conlon, University of Minnesota
Tess DeLean, Wellesley College
Max Gelb, Dartmouth College
Ben Guttman-Kenney, University of Warwick
Malin Hu, Georgetown University
Kilian Huber, London School of Economics and Political Science
Tomas Jagelka, Dartmouth College
Shorena Kalandarishvili, Smith College
Hideto Koizumi, Soka University of America
Krisjanis Krustins, Stockholm School of Economics in Riga

Benjamin Langworthy, Macalester College
Nancy Lee, Georgetown University
Daniel Lim, Georgetown University
Van Nguyen, Washington and Lee University
Nikita Orlov, University of Warwick
Anselm Rink, London School of Economics and Political Science
Vivek Sampathkumar, Georgetown University
Monica Scheid, Georgetown University
Markus Schwedeler, Maastricht University
Matthew Shapiro, Georgetown University
Zane Silina, Stockholm School of Economics in Riga

Anusuya Sivaram, Georgetown University
David Thomas, University of Oxford
Maximilian Thomann, London School of Economics and Political Science

Eleventh Annual Carroll Round
(April 19-22, 2012)
Madara Bogdane, Stockholm School of Economics in Riga
Paul Byatta, Harvard University
Nikhil Dugal, New York University
Vladimir Epuri, American University in Bulgaria
Samuel Evans, University of Warwick
Evan Friedman, Brown University
Fabian Gunzinger, University of Bern
Taras Ignashchenko, Lancaster University

Tenth Annual Carroll Round
(April 14-17, 2011)
Dimitri Avramov, American University in Bulgaria

Hang Qian, Dartmouth College
Paul Unanue, Princeton University
Ahmad Wahdat, Oberlin College
Ariell Zimran, Georgetown University
Katrina Koser, Georgetown University
Nhaca Le, Georgetown University
Wanyi Li, Macalester College
Elitsa Nacheva, American University in Bulgaria
Anastasija Oleinika, Stockholm School of Economics in Riga
Carlo Pizzinelli, Dartmouth College
Thomas Preston, University of Warwick
Doug Proctor, Georgetown University
Julian Richers, Columbia University
Christopher Roth, University of Warwick
Andrea Ruiz, The George Washington University
Kaivan Sattar, New York University
Mark Schmidt, Georgetown University
H. Jess Seok, Georgetown University
Kenichi Shimizu, Soka University of America
Anusuya Sivaram, Georgetown University
Shuo Yan Tan, Georgetown University
Anna Weber, Georgetown University
Edie Wu, Dartmouth College
Qianyi Yang, Macalester College

Twelfth Annual Carroll Round (April 18-21, 2013)
Nikola Andreev, American University in Bulgaria
Matthew Bailey, University of Warwick
Albert Chiang, Georgetown University
Bayarkhuu Chinzorigt, American University in Bulgaria
Hadi Elzayn, Columbia University
Yi Jie Gwee, London School of Economics and Political Science
Rosa Hayes, Wesleyan University
Asher Hecht-Bernstein, Columbia University
Edward Hedke, Georgetown University
Hannah Hill, Georgetown University
Sasha Indarte, Macalester College
Mohandass Kalaichelvan, Dartmouth College
Phoebe Kotlikoff, United States Naval Academy
Weiwen Leung, Singapore Management University
Shawn Lim, University College London
Michael Lopesciolo, Georgetown University
Sara Marcus, Dartmouth College

Stephen McDonald, Georgetown University
Leyla Mocan, University of Pennsylvania
Preston Mui, Georgetown University
Emily Oehlsen, Georgetown University
Igors Pasuks, Stockholm School of Economics in Riga
Nicolas Powidayko, University of Brasilia
Michael Reher, Georgetown University
Glenn Russo, Georgetown University
Eduards Sidorovics, Stockholm School of Economics in Riga
Fabian Trottnner, London School of Economics and Political Science
Ilyas Zhukenov, University of Warwick

Thirteenth Annual Carroll Round (April 10-13, 2014)
Eric Aldenhoff, University of Maryland, College Park
Russell Black, Oxford University
Thomas Bumberger, University of Cambridge
Dawn Chan, Georgetown University
Kyle Coombs, Macalester College
Rob Dent, The University of Virginia
Brian Goggin, Georgetown University
Heather Hedges, Georgetown University
Alyssa Huberts, Georgetown University
Johnny Huynh, Pomona College
Nikhil Kalathil, Oberlin College
Matthew Klein, The University of Chicago
Samsun Knight, Oberlin College
Vincent La, Dartmouth College
Nataliya Langburd, Yale University
Katherine Loosley, London School of Economics and Political Science
Soumyajit Mazumder, Georgetown University
Russell Morton, Princeton University
Jill Ni, Georgetown University
Jonathan Pedde, Dartmouth College
Viktoria Pilinko, Stockholm School of Economics in Riga
Andrei Romancenco, Stockholm School of Economics in Riga
Saugata Sen, London School of Economics and Political Science
Benjamin Shoesmith, The University of North Carolina at Wilmington
Meredith Strike, Georgetown University  
Chris Stromeyer, Georgetown University  
Rachel Syzmanski, Georgetown University  
Josh Walker, Lancaster University

**Fourteenth Annual Carroll Round**  
**(April 16-19, 2015)**  
Levi Boxell, Taylor University  
Hameem Races Chowdhury, University of Warwick  
Thomas Christiansen, Georgetown University  
Mathison Clore, Georgetown University  
Chenbo Fang, University of California, Berkeley  
Ryan Su-Shien Go, University of California, Berkeley  
Aaron Goodman, Dartmouth College  
Geeva Gopalkrishnan, Georgetown University  
Sankalp Gowda, Georgetown University  
Thomas Gutierrez, Harvard University  
Dora Heng, Cornell University  
Samuel Huang, London School of Economics and Political Science  
Kenan Jusufovic, London School of Economics and Political Science  
Eve Lee, Georgetown University  
Michael Lee, The University of Texas at Austin  
Karlis Locmelis, Stockholm School of Economics in Riga  
Shom Mazumder, Georgetown University  
Jonathon McClure, Georgetown University  
Michael McGrath, Georgetown University  
John McKeon, Boston University  
Virginia Minni, University of Warwick  
Natalie Nah, Georgetown University  
Emily Reeves, Dartmouth College  
Lea Rendell, Vassar College  
Daniel Roeder, Duke University  
Raphael Small, Haverford College  
Jack Wiloughby, Duke University  
Nancy Wu, Dartmouth College  
Yingtong Xie, Macalester College

**Fifteenth Annual Carroll Round**  
**(April 21-24, 2016)**  
Mihaly Abel, University of Warwick  
Ahwaz Akhtar, Georgetown University in Qatar  
Rachel Anderson, Duke University  
Lukas Bolte, London School of Economics and Political Science  
Lilja Chobanova, American University in Bulgaria  
Felicia Choo, Georgetown University  
Griffin Cohen, Georgetown University  
Emily Corning, Columbia University  
Dashnajil Enkhbayar, Georgetown University  
Michael Gill, University of Warwick  
Serena Gobbi, Georgetown University  
Elizabeth Johnson, Georgetown University  
Anastasiya Kazhar, Stockholm School of Economics in Riga  
Stephanie Kestelman, Swarthmore College  
Olena Kuzan, Stockholm School of Economics in Riga  
Jia Jun Lim, University of Warwick  
Omeed Maghzian, Columbia University  
Querida Qiu, The University of Chicago  
Mason Reasner, Vanderbilt University  
Tim Rudner, Yale University  
Robert Scales, Dartmouth College  
Joycelyn Su, The University of North Carolina at Chapel Hill  
Yichuan Wang, University of Michigan  
Matthew Waskiewicz, American University  
Danny Watson, Georgetown University  
Vanessa (Wenye) Xiao, Vanderbilt University  
MaryAnne Zhao, Georgetown University

**Sixteenth Annual Carroll Round**  
**(April 20-23, 2017)**  
Mohammad Ahmad, Georgetown University in Qatar  
Olivia Bisel, Georgetown University  
Matthew Carl, Washington and Lee University  
Mathison Clore, Georgetown University  
Gabriella Cohen, University of Cape Town  
Griffin Cohen, Georgetown University  
Alexandra Colyer, Georgetown University  
Christian Fernado, Georgetown University  
Robert Garcia, Universitat Pompeu Fabra  
Ty Greenberg, Georgetown University
Seventeenth Annual Carroll Round  
(April 19-22, 2018) 
David Alzate, Georgetown University  
Olivia Bisel, Georgetown University  
Tristan Byrne, University of Warwick  
Christina Cheung, University of British Columbia  
Sitong Ding, London School of Economics and Political Science  
Filip Drazdou, Stockholm School of Economics in Riga  
Beatriz Garcia Quirog, Universitat Pompeu Fabra  
Stephan Hobler, University of Warwick  
Wessam Kanes, Georgetown University in Qatar  
Jonathan Kaufmann, American University  
Arjun Krishnan, Georgetown University  
Darya Labok, Stockholm School of Economics in Riga  
Van-Anh Le, Macalester College  
Duy Mai, Georgetown University  
Ryan Mather, University of Minnesota  
May Nguyen, Dartmouth College  
Jake Owen, Lancaster University  
Aditya Pande, Georgetown University  
Connor Regan, Dartmouth College  
Daniel Sanchez-Ordonez, Stanford University  
Sebastian Senlle, University of Buenos Aires  
Jack Thiemel, Georgetown University  
Tuyet-Anh Tran, Macalester College  
Alexander Weinberg, Dartmouth College  
Quan Xue, London School of Economics and Political Science