BREAD AND RIOTS: ASSESSING THE EFFECT OF FOOD SECURITY ON POLITICAL STABILITY

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Christopher S. Adams, B.A.

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Christopher S. Adams, B.A.

Thesis Advisor*:* Matthew H. Fleming, Ph.D.

ABSTRACT

Policymakers routinely argue that food security undergirds political stability within a country. Indeed, this linkage motivates much of US foreign food assistance. However, the broader literature does not substantiate this conclusion, finding most internal conflicts arise not from common grievances but from narrow avarice. Though some researchers have found empirical evidence that food security and political stability might be linked, these studies are burdened by data and methodological issues. Using novel data sources and a unique conceptual model, this paper seeks to address these potential problems. We find, in contradiction to expectations, that increases in absolute levels of food security significantly increase the frequency of events of political unrest in a country. This finding is robust to different specifications of our model, lending credence to its validity. However, we also find that increases in the relative levels of food security significantly reduce political instability. If true, these findings suggest that the US government might wish to reconsider certain assumptions that underlie its food security policy and consider alleviating relative declines in food availability in addition to absolute decreases.

The research and writing of this thesis
are dedicated to everyone who helped along the way.

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Introduction

History suggests that, like the proverbial army, the state marches on its stomach. Roman emperors famously subsidized bread prices to keep the *hoi polloi* contented in the twilight years of the empire. In Paris in 1789 and Saint Petersburg in 1917, bread riots precipitated by poor harvests culminated in uprisings that toppled monarchs. In modern times, observers connect food insecurity and political instability more explicitly, holding that rising food prices and empty stomachs rob states of output legitimacy. The leadership appears helpless as the public starves, abetting political opposition to the government. Increasing food prices can also drive inflation, effectively impoverishing the public and potentially robbing the state of legitimacy (Fam and Kassem 2011). Indeed, the Arab Spring uprisings that spread across the Middle East and North Africa in 2011 (and continue in Syria to this day) appear to stem in part from diminished food security and the resultant food riots becoming politicized.[1] Along similar lines, others have argued that the frustrations facing coalition forces in Afghanistan to create lasting political stability result from an inability to provide meaningful food security (Cordesman and Mausner, 2010).

However, food prices themselves may seem like a common culprit because they are so frequently volatile, both across seasons and years, that an occasional correlation between food security and political stability might be purely coincidental. For instance, during the immense spike in food prices in 2008, equivalently large as the one that precipitated the 2011 Arab uprisings, UN Secretary General Ban Kim Moon warned of mass unrest and instability. The 2008 price increase led to numerous food riots across Africa and the remainder of the developing world, but did not produce political consequences as significant as the more recent Arab uprisings (Topping 2008).

[1] See for instance, Ariana Eunjung Cha. "Spike in global food prices contributes to Tunisian violence." *Washington Post.* January 14, 2011; Zoe Flood. "At Least Twenty Killed in Economic Protests in Tunisia and Algeria." *The Daily Telegraph*. January 10, 2011; and Caroline Henshaw. "The Food Politics of Egypt." *The Wall Street Journal*. February 1, 2011.

Likewise, observers have connected the so-called color revolutions that spread across the post-Soviet world during the first half of the twentieth century to a wide variety of causes, but few argue that these upheavals were triggered by food insecurity or rising food prices. Indeed, the relevant literature on civil wars and civil unrest suggest that it is the abundance of profitable resources, like diamonds or oil, that fuels violent dissident groups, rather than a dearth of necessary resources, like basic foodstuffs.

Nonetheless, the imputed connection between food security and political stability remains persuasive amongst policymakers. In the wake of the 2008 price spike, the leaders of the world's largest economies committed billions of dollars of assistance for food aid in a statement that explicitly links food insecurity with political unrest (G8 2009). Similarly, US officials have defended "Feed the Future," the president's 3.8 billion dollar interagency food security initiative, along similar lines, arguing that food assistance quells instability abroad and thus improves American national security.[2]

Therefore, understanding the true nature of the relationship between food insecurity and political instability remains a policy relevant problem. This paper intends to test the proposition that the former causes the latter, using food security data from the Food and Agriculture Organization (FAO) and a database of individual instances of political unrest, maintained by the Cline Center for Democracy at the University of Illinois.[3] We find, contrary to US policy but in line with the prevailing literature, that increases in absolute levels of food security appear to increase overall political instability but increases in relative levels of food security appear to diminish political instability. Before detailing these results, though, we hope to limn the concept of food security and

[2] See, for instance, this post by Jonathan Shrier, the then acting Special Representative for Global Food Security for the State Department: Jonathan Shrier."Food Security Contributes to National Security." US Department of State. October 28, 2011. Accessed April 4, 2013. http://blogs.state.gov/index.php/site/entry/food_national_security

[3] Accessible from http://faostat3.fao.org/home/index.html and http://www.clinecenter.illinois.edu/research/speed.html, respectively

briefly the outline the competing literature on the effect of food and other resource abundance on political violence and unrest. From these, we shall build a complimentary conceptual framework for how food security can influence political unrest (and vice versa), which will be the bedrock of the analytical model we will test. After summarizing the results of these tests, we intend to explore their implications and offer a few tentative suggestions to future researchers and policymakers alike.

Background

The concept of food security has evolved significantly in the past 30 years. The term originated in the 1970s to describe a nation's food supply, whether the state in question produced or imported enough food to feed its citizenry, as well as whether the aggregate of all states produced enough food to feed the world. However, as the focus of food crisis prevention and remediation shifted from boosting the capacities of national governments to targeting specific populations in need, this definition proved inadequate. Thus, observers like the Food and Agriculture Organization have gradually adapted their definitions to the level of individual households and, as a result, expanded beyond a simple measure of food supply.[4]

The US Department of Agriculture (USDA) defines food security domestically as a combination of the *availability* of sufficient and nutritionally adequate food and the ability to *access* that available food in socially acceptable ways (USDA 2013). In the international context, a third component, *utilization*, is frequently added (USAID 1995). *Availability* refers primarily to the total food produced, either internationally or domestically, and can be improved by boosting agricultural yields relative to population growth. *Access* concerns the dispersion of food throughout the international market and within a country and whether those individuals have the means to purchase

[4] For a discussion of all the evolving definitions of food security used by the FAO over the past forty years, see FAO. "Food Security." FAO Policy Briefs. June 2006, Issue 2: p.1ftp://ftp.fao.org/es/ESA/policybriefs/pb_02.pdf

sufficient food, even if it is available. This can typically be improved through a variety of measures including reducing barriers to trade, improving transportation within a country, and increasing individual incomes within a country. *Utilization* concerns human behavior, whether, given sufficient nutritious food is available and accessible, it is purchased and consumed by the public. Food *utilization* can be potentially improved through nutrition education programs or subsidies for specific types of foods. FAO has recently included a fourth dimension, *stability*, which measures the constancy of the above three over time (FAO 2006).

Improvements to one element of food security can sometimes conflict with attempts to improve another (for instance, a shift towards cash crops for export reduces yields of staple crops within food insecure countries) and there is some disagreement both within academia and amongst practitioners as to which element is most predictive of true food security within a specific country. Nonetheless, there is widespread agreement that all three (or four) elements are important factors, independent of their relative importance, in determining food security.

Literature Review

The prevailing view in the policy world holds that food insecurity precipitates political instability. Vice President Joe Biden, in speech awarding Bill Gates and Warren Buffet for their work fighting global hunger, outlines this view:

> As Pope Paul VI once said, "development is the new word for peace." And the reality is that, in many countries, food security and political stability are closely linked.
>
> Investments made to ward off food insecurity and prevent its recurrence can prevent the vicious cycles of rising extremism, armed conflict and state failure than can require far larger commitments of resources down the road.
>
> When food prices spiked three years ago, riots or demonstrations broke out in dozens of countries because people could no longer feed their children. Many of these protests turned violent.
>
> In Sudan, the Darfur crisis, which seized the world's attention for much of the past decade, was sparked, in part, by a competition for arable land—a competition later used to justify unspeakable atrocities by the Janjaweed militia. The crisis in Darfur is man-made. But it is also true that with dwindling supplies of water

> and arable land, often exacerbated by climate change, the conditions were ripe for conflict— because people were forced to compete for resources they once shared (Biden 2011).

Unfortunately, the predominant literature does not corroborate this model. Researchers investigating the influence of natural resources on political instability explain the linkage through two competing motivations: greed and grievance.[5] Proponents of the former argue that an abundance of valuable, portable resources, like minerals, oil, or cash crops enable self-interested groups to exploit internal conflict to harvest and sell these goods on the international market, or at very least leverage their sale to fuel conflicts instigated by other factors (*e.g.* Ross 1999). Grievance-based explanations, on the other hand, cite a dearth of vital resources, like water, livestock, and staple crops, as instigating conflicts either between groups or by marginalized groups against the government over their lack of these resources (*e.g.* Diamond 2005). Traditionally, comparative studies have found greed-centric explanations and an abundance of valuable natural resources a more persuasive explanatory factor for political conflict, at least with regard to civil war (Collier and Hoeffler, 2000; Fearon and Laiton 2003). Wars require significant financial and organizational investments to sustain and are especially risky endeavors for both rebels and the government; this usually necessitates a significant potential reward to sustain. Grievances can facilitate opportunistic actors in widening conflicts, but a more potent motive is often necessary. This emphasis on greed, however, runs directly contrary to current US policy regarding food security, which holds that deprivation leads to insecurity by instigating grievances against the government.

However, civil war is only one end of the spectrum of political instability, and a number of scholars have proposed that resource scarcities, specifically of food or water, can precipitate a broad range of disorders that do not rise to the level of outright conflict. Homer-Dixon, for instance, argues that environmental pressures do and increasingly will lead to diminished agricultural yields

[5] This dichotomy stems primarily from Paul Collier and Anke Hoeffler, "Greed and Grievance in Civil War," World Bank Policy Research Working Paper No. 2355, May 2000

and gross domestic products in the developing world, which in turn disrupt the social order within states (1991). He proposes a model for how changing environmental factors, principally climate change, population growth, and the diminishment of arable land, can instigate population displacement, authoritarianism, and conflict, and predicts that food crises will be one of the key determinants of political violence in the twenty-first century. Conversely, both Rotberg (2005) and Bates (2008) reverse the causality on these trends, attributing famines, diminished yields, and environmental disasters to rapacious, short-sighted political elites and the internal conflicts their actions spawn. From this perspective, food insecurity is merely symptomatic of greed and dysfunction at the highest levels, rather than causing political instability in and of itself. Cohen and Pinstrup-Anderson (1999) synthesize previous studies and anecdotal evidence to paint a more nuanced picture, pointing to instances where unrest appears to precipitate hunger as well as the reverse.

Unfortunately, quantitative investigations of this issue are sparse and unpersuasive. Hendrix and Salehyan (2010), for instance, link extreme hydrological events with instances of political unrest using panel data separated by country, but do not attempt to use their findings on hydrology to model how food shocks specifically would influence political unrest. This is especially notable as Lagi *et al.* (2011) find no correlation between the instance of extreme weather conditions and international food prices. In a separate but related paper, Lagi *et al.* (2011) instead correlate the international food price index calculated by the FAO with instances of political instability globally. Intriguingly, they look specifically at food riots to increase the likelihood that changes in food prices influenced the dependent variable in question. However, doing so limits the explanatory power of their findings; it does not seem likely that one can extrapolate this relationship to political unrest more generally. Additionally, the researchers lump very different events in the same category, making no distinction between a minor protest in Bangladesh and the Syrian civil wars, considering

them both an example of a food riot despite the disparity in severity. Finally, there is little accounting for causality: they do not control for the possibility that food prices could have been independently increased by global instability.

Other researchers have attempted more sophisticated models to elucidate the direction of causality with these interconnected phenomena. For instance, Bellemare (2011) looks specifically at the relationship between food riots and food prices, like Lagi *et al.*, but also instruments the independent variable using recorded instance of natural disasters that might influence food prices (like floods, droughts, or insect infestations) to control for endogeneity. Bellamare finds a strong positive relationship between international food prices in a given month and the predicted number of food riots in that month, indicating that the similar relationship uncovered by Lagi *et al.* might have been correct. However, like Lagi *et al.*, Bellemare only looks at food riots, rather than all instances of political unrest, which makes his findings hard to generalize beyond the obvious. He also only examines change at the international level, using time series data for monthly international food prices, and thus fails to control for country level effects.

Arezki and Bruckner (2011), with the International Monetary Fund, address both of these issues. They use a fixed effects model to demonstrate that a country-specific food price index correlates with various measures of political instability within that country beyond just food riots. However, their estimation method for the within-country food price index (a simple function of international food prices for five commodities combined with the country's status as an importer or exporter for those commodities) excludes a number of factors that dictate prices in a particular country, most notably government interventions like tariffs, export taxes, and subsidies. Cohen and Pinstrup-Anderson, for instance, point out that changes in Zambia's food subsidies led to riots that ultimately ended President Kuanda's regime and ushered democratic elections in that country. More importantly, Arezki and Bruckner do not account for issues of endogeneity in their model and,

unlike Bellamare, cannot persuasively demonstrate that food prices precipitate instability rather than the reverse (as Rotberg and Bates had theorized). Indeed, when Arezki and Bruckner looked specifically at the effect on instability of the previous year's food prices, the significance of the relationship disappeared, suggesting that the linkage they noticed was not causal. Finally, Arezki and Bruckner, like the remainder of the above papers, only consider international food prices, rather than a measure of the food situation within the country. An examination of country-specific food security on political instability within that country would be able to better reconcile the differing theoretical arguments about the linkage between food shortages and political unrest.

Conceptual Framework

This paper seeks to build on these previous works. In accordance with theory on state capacity and current US development policy, we hypothesize a positive relationship exists between food security and political stability, with greater food security enhancing political stability. As with previous empirical studies, we anticipate that deficiencies in food security—in terms of access and availability—worsen the well-being of the average citizen, which in turn robs states of output legitimacy. This decline in legitimacy could induce the members of the public to seek extra-political measures to influence the government or even to turn to rival sub-state actors to replace the government. A diminished food security could influence elite behavior as well, as it forces governments to seek out other forms of legitimation independent of the public well-being. For instance, governments could instead retreat to appeals to specific ethnic identities or familial ties to keep support for the government strong, or attempt to rally the public against foes foreign and internal. All of these actions would be expected to increase political instability in a country and to be more acute the closer the average citizen is to starvation. Food security can influence political stability through less direct channels as well. For instance, rising food prices due to a diminished

supply could trigger more general price inflation, which would have far broader economic effects than simply in the agricultural sector, further robbing the state of legitimacy. Alternatively, rising food prices have a regressive effect on income, with food consisting of a greater proportion of the consumption basket for those who are less well off, which could hasten the growth of inequality and, from there, political unrest.

However, a problem arises with this simple model: the causality need not flow solely from food security to political stability. Indeed, as Cohen and Pinstrup-Anderson outlined, political unrest frequently precipitates food crises within countries. Due in part to the capricious actions of repressive regimes or the realities of intrastate conflict, growing political unrest disrupts trade internationally and internally, limiting both the availability and access elements of food security. It also frequently displaces whole groups of people, deracinating them from their homes and traditional food sources, further diminishing the average citizen's access to food. This apparent effect of political unrest on food security requires us to consider the potential for reverse causality in our models, a consideration that previous quantitative studies have undertaken only sparingly.

One must consider other confounding variables as well. For instance, the type of regime may influence the relationship between food security and political unrest in a particular country. If food insecurity acts primarily by robbing governments of output legitimacy, then one would expect the resultant political unrest to be more likely in authoritarian regimes, where the governing legitimacy rests more exclusively on outputs and where no peaceful means of expressing discontent exist (Acemoglu and Robinson 2001). Additionally, as Amartya Sen and others have argued, authoritarian states are themselves more likely to experience famines, due to unequal distributions of food within states and the insularity of the governing regimes (Sen 1981). Therefore, it will be incumbent to consider the effects of regime type and behavior, as more autocratic regimes would be presumed to experience more food insecurity and greater upheaval across equivalent levels of food

insecurity. Likewise, though we speculated above that generalized price inflation could be a conduit through which food insecurity intensifies political instability, an increase in inflation rates independent of food shocks would be expected to worsen both food security (through diminished food access due to heightened food prices) and political stability (through decreased output legitimacy also due to heightened prices more generally). Similarly, wealthier states would be more capable of weathering food crises and would have fewer individuals at or near starvation, though both of these effects would be expected to diminish as states grow wealthier. Finally, states with terrains that are more arid or more difficult will face problems producing and distributing sufficient food in a crisis, which can also amplify the effect of food shortages. These considerations will need to be controlled for as well.

Data and Methods

As compared to previous studies, this paper intends to use novel and hopefully more accurate data sources to measure both food security and political instability within a particular country. For country-level data on food security, we intend to use a direct measure of in-country food supply, specifically the aggregate food supply measures collected by the FAO, as opposed to inferring this data from international food prices. These data are collated from self-reported yield figures for a wide swath of crops and livestock and then modified using similar data on agricultural imports and exports to calculate the total foodstuffs available within the country in that particular year. FAOStats (2012) reports this information in kilocalories per capita per day, based on the estimated caloric content of the foodstuff in question (e.g., a bushel of corn). We multiplied these data by the FAO's country-year population estimates to approximate the daily food supply in the country. This paper will also pair this measure with an estimate of food quality, calculated as the percentage of calories of a country's daily food supply that comes from either fats or proteins

(suggestive of a higher quality and more variegated diet), as caloric content alone does not provide a complete accounting of malnourishment.[6] As the FAO only began tracking such information in 1979, this year will serve as the starting point for all panel data compiled as part of this paper (FAOStats 2012).

Predicating our measure of food security on country-level food supply data rather than international food prices offers several distinct advantages relative to previous studies. Most notably, unlike Bellamare or Lagi *et al.*, doing so allows us to use country-year panel data for all variables rather than time series data using international averages. This enables our inquiry to control for country-level changes in political and macroeconomic indicators, as well as country-specific fixed effects, in estimating the effect of food security on political stability. It also ensures that the two are spatially and temporarily aligned, so that a famine in Bangladesh is not counted as linked to a civil war in Liberia simply because they happened to occur in the same year. Additionally, unlike Arezki and Bruckner, who extrapolated country-level prices based on international food prices and tariffs on imports within the country, our data capture a finer grain on food availability: the extrapolation method used by Arezki and Bruckner would have missed completely the aforementioned Zambian uprising, as it was predicated on a change in internal subsidies to farmers. Our data have the additional advantage of capturing actual statistics, rather than imputed measures.

However, this points to a significant potential disadvantage in using FAO's food supply data rather than those for international food prices. While the latter are predicated on prices observed by FAO on the international market, FAO relies on official government sources to construct their country-level food supply data (FAOStats 2012). This leads to reliability issues as individual governments may have an incentive to portray their food supply as more or less abundant than it in

[6] See Appendix 1 for a complete accounting for how we constructed our food security variables.

fact is. Additionally, some countries do not respond to these surveys at all, either out of dysfunction or pique, which could potentially further bias our results. Fortunately, FAO does correct for instances where the data are unavailable or unreliable, but these corrections just shift the locus of the problem from the countries to the FAO itself (FAOStats 2012). Indeed, some countries with a history of instability (like Somalia and Afghanistan) or with negligible internal food production (like Qatar and Bahrain) do not report food supply data to the FAO at all and have thus been removed from our sample for all years (FAOStats 2012).[7]

An additional complication arises as the food supply data (along with those for international food prices) only capture the first of the three components that make up food security. All of these indicators are measures of food *availability*, whether there is sufficient food in-country or globally to feed those in need, but are blind to issues of food *access*, which encompasses the distribution of food within the country and the public's ability to purchase it, and food *utilization*, which encapsulates whether individual buy and use the available and accessible food properly. As each of the components builds sequentially—the inclination of the public to use certain foods means little if that food is never produced or never reaches them—food availability is likely the best proxy for true food security and certainly the easiest to measure effectively. However, other components may perpetuate undernourishment and food insecurity even in countries awash with foodstuffs. It should be noted that FAO uses these same measures of in-country food supply in determining its own estimates of food insecurity across the globe, suggesting that the above caveats are not sufficient to dismiss measures of food supply as irreparably flawed (FAOStats 2012).

We likewise rely on a novel data source to approximate political stability within a particular country. Arezki and Bruckner, for instance, proxy a country's political stability through its degree of democratic governance combined with the intensity of intra-state wars in its borders. But this

[7] See Appendix 1 for a complete accounting of the countries removed from our sample

metric is both too broad and too narrow: it misses instances of political violence that do not rise to the level of civil war and at the same time considers stable autocratic states to be unstable. Instead, like the many of the previous empirical studies, this paper will use event data to proxy political stability, but, unlike those studies, our data are not restricted solely to instances of food riots, which partially presumes the conclusion, and are linked specifically to the country of origin, allowing again for country-year comparisons to be employed.

Specifically, our data come from the Social, Political, and Economic Event Database (SPEED), collated by Dr. Peter Nardulli and others at the Cline Center for Democracy at the University of Illinois.[8] Using automated search protocols, Nardulli *et al.* combed through the complete archives of the New York Times and the Wall Street Journal from 1946 to 2009 for instances of a range of political or social instabilities (for instance: coups, group violence, anti-regime protests). Finding these sources lacking in international coverage, the researchers included reports from the Foreign Broadcast Information Service and the Summary of World Broadcasts, each of which contain English language summaries of tens of thousands of newspapers from every country across the globe, collated by the CIA and BBC, respectively. The researchers used algorithms to extract a wide swath of information from the news reports about the events, including, for instance, the number of deaths, injuries, and arrests that resulted from the event, with human following up to clean up and fill out the computer outputs (Nardulli 2012). We collapsed the data from SPEED into simple counts of events per country-year to be used as our primary dependent variable. As with the independent variable, our data begin in 1979 and extend to 2009. On the personal advice of Dr. Nardulli, we removed the United States from our sample, as the reports from the New York Times and Wall Street Journal are heavily weighted towards domestic coverage, complicating comparisons between the United States and other countries.

[8] Many thanks to Dr. Nardulli for graciously providing the data and offering helpful tips on its coding and uses.

Analysis Plan

To test the hypothesis that food security positively affects political stability, we intend to use regression analysis to predict events of political unrest in a country given levels of food security. As Gould (2011) argues, the Poisson regression model is most appropriate for attempting to predict count data, especially since many countries within our sample did not have events occurring every year, leading to a high concentration of null observations relative to positive ones (see Figure 1).[9] Poisson regressions rely on the Poisson distribution, which is used to predict the likelihood of a certain number of discrete events, given a set small mean. Unlike with the normal distribution, a Poisson distribution can only vary across one parameter, the conditional mean; the variance is assumed to be a simple function of the conditional mean (Wooldridge 2009).

Figure 1: Distribution of Event Data

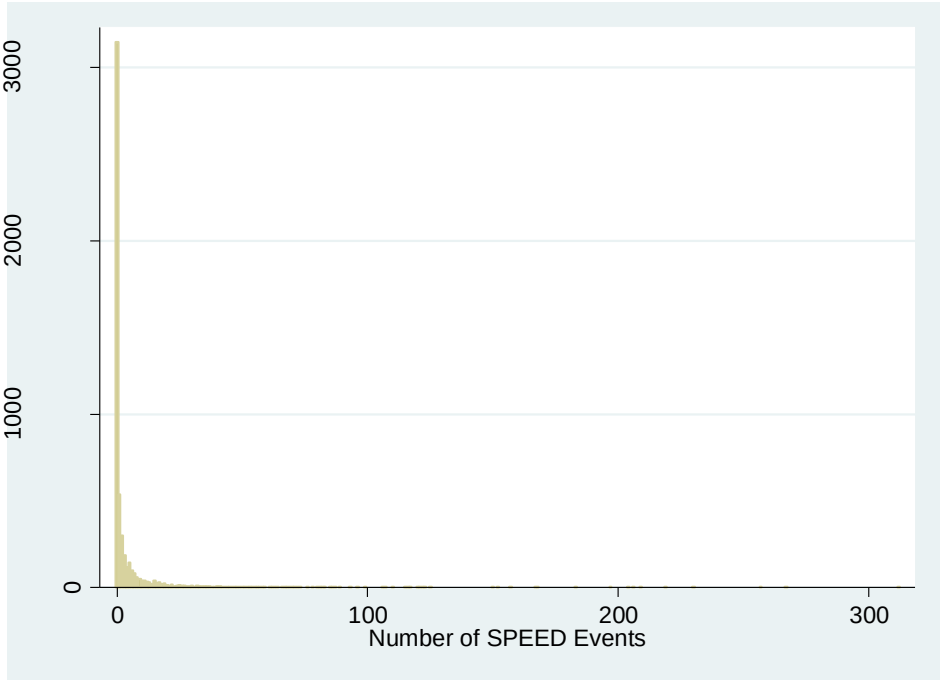

Figure 1: Frequency graph of the number of events recorded for each observation. Over 70 percent of the observations have zero events and, of those that have at least one event, roughly 60 percent had only one event recorded.

[9] The Poisson regression assumes that the expected value of the dependent variable is a function of *e* raised to the combination of the independent variables and their beta coefficients and is thus, in that respect, similar to but not identical to taking the natural log of the dependent variable. It is the difference between E(ln(y)) and ln(E(y)), but in situations with strong skew and large numbers of zeroes, as observed with most count variables including our own, the latter is more appropriate (Wooldridge 2009).

However, this is often an unrealistic assumption for a given set of data. For instance, it would require that, if Uzbekistan had a large number of events in one year, then it would be no more likely than random chance to have a similar number the following year. Some methodologists, like Wooldridge and Gould, assert that this overdispersion can be easily corrected for, but the majority of researchers have moved towards correcting for overdispersion using a negative binomial regression instead (e.g. Krause 1994; Beth and Elkins 2004; Krause, Suzuki, and Witmer 2006). A negative binomial regression relies on the negative binomial distribution instead of the Poisson distribution and thus allows for the estimated conditional variance to exceed the conditional mean.[10] In effect, it introduces an additional source of randomness to the Poisson model, multiplying the Poisson-determined conditional mean by a gamma distributed random variable with an expected value of one (Krause 1994).[11] As the greater variance enabled by the negative binomial regression better approximates our data, we intend to primarily use negative binomial regression models to predict our event count variable. However, we will include the results of Poisson regressions with corrected standard errors as well as standard ordinary least squares (OLS) estimates of our primary models for the sake of comparison (See Appendix 2).

Our initial specification will use event counts per country-year as the dependent variable and food supply measured in two different ways: kilocalories of food, as well as the percentage of fat and protein in that food, all per day per country-year. Measuring caloric content alone offers only a narrow view of potential diet deficiencies and cannot capture the potential malnutrition or discontentment that arises with food supplies that are above starvation levels but consist purely of cheap cereals. This specification offers the secondary benefit of allowing for us to capture how

[10] The negative binomial distribution refers to the number of successes one is expected to receive in a series of Bernoulli trials before a set number of failures obtain. For instance, one could use the negative binomial distribution to determine the likelihood of surviving various numbers of rounds of Russian roulette with 2 bullets in the chamber.
[11] As Poisson regressions assume the dependent variable is equal to $e^{(x\beta)}$ (see footnote 8), multiplying this by a random variable is equivalent to adding a random error term to the model (i.e. $e^{(x\beta)}\delta = e^{(x\beta + \varepsilon)}$ where $\delta = e^{\varepsilon}$ and ε is uncorrelated with all x's). In this respect, the negative binomial regression better approximates the assumptions of OLS than does Poisson regression.

shifts not only in the quantity but the quality of foodstuffs alter stability, as foods high in protein and fat tend to be more expensive and indicative of a diversified diet. Finally, as we expect the positive effects of food security on political stability to diminish as more of the public moves further from starvation and deprivation, all specifications of our models will include the natural logarithm of food supply rather than the linear metric itself.

We also intend to use fixed effects for both country and years in our models. Using fixed-effects allows us to control for a host of unobserved country- and year-specific effects, so that, for instance, the potentially unrelated weather in 1991 (when a large number of unrest events occurred) does not overly bias our results. Doing so also allows us to control for many of the country-invariant structural variables we alluded to earlier. Both of these adjustments leverage the large number of observations in our dataset (over 5000) to avoid issues with the decreased precision they entail. However, some researchers have cautioned against using fixed effects with negative binomial regressions, as doing so interferes with the independent specification of the conditional variance and can either incorrectly lead to estimates identical to those obtained by a Poisson model or can result in nonzero estimates for the time-invariant variables (Allison and Waterman 2002). This complication could obviate the entire point of using a fixed effects model, wherein, by demeaning all regressors and the dependent variables by the within country mean, the effects of unobserved country-specific variables would disappear (Wooldridge 2009). To adjust for this potential problem, we specified an additional models for all regression that uses simple unconditional country and year dummies rather than rely on the conditional estimates generated by a fixed effects model.

It should also be noted that fixed effects models can only control for country-specific elements that remain constant over time or, conversely, time-specific elements that are constant across states. A few determinants of both food security and political stability, including many secondary elements of our conceptual model, vary over time within a country and will need to be

controlled for. Most notably, we anticipate that a country's population will govern both the overall food supply in a country (which we derived from a per capita value to begin with) and the number of events observed. Therefore, we will need to include a measure of population per country-year in all models.[12] Likewise, a country's wealth, measured as gross national product (GDP), will have to be controlled for, so that the wealthy, stable, and well-fed countries in Europe and North America do not overly influence our findings.[13] As with food supply, the effect of GDP would be hypothesized to diminish as it increases suggesting that a log transformed variable would be more appropriate to include in all models. Additionally, as we stated in our conceptual model, rising food prices could influence more generalized inflation or vice versa, leading to political unrest, which means inflation rates per country year will need to be accounted for.[14] We also argued previously that regime type, measured as more or less democratic, could influence both food security and political stability and is obviously not constant across states or years and thus would need to be accounted for in any model.[15] All four of these factors will need to be included as separate independent variables in all models specified.[16]

With all of these considerations in place, our primary model for predicting events of political unrest is specified as such:

$$\text{Events}_{it} = \beta_0 + \beta_1 \ln(\text{FoodSupply}_{it}) + \beta_2 \text{FoodQuality}_{it} + \beta_3 \text{Democracy}_{it} +$$

$$\beta_4 \ln(\text{GDP}_{it}) + \beta_5 \text{Inflation}_{it} + \beta_6 \text{Population}_{it} + \alpha_t + \alpha_i + \eta_{it}$$

However, to ensure the robustness of the model, we intend to test a number of alternate specifications as well, to control for the possibility that our findings are sensitive to certain

[12] Population measured in thousands of people per country per year, via FAOStats (2012)
[13] GDP per capita measured in constant (2005) international dollars and normalized for purchasing power parity, via the World Bank, http://databank.worldbank.org/ddp/home.do
[14] Inflation rates per country measured yearly based on consumer prices, also via the World Bank, http://databank.worldbank.org/ddp/home.do
[15] Regime type measured on a -10 to 10 scale, where -10 is maximally autocratic and 10 is maximally democratic, via Polity IV, http://systemicpeace.org/polity/polity4.htm
[16] See Appendix 1 for additional information on how we arrived at our four control variables.

assumptions implicit in the above model. Firstly, we must address the issue of reverse causality broached in our conceptual framework. The high likelihood that political unrest could cause a decrease in food security, in addition to or instead of the reverse, means our primary model is insufficient as specified, as the true effect of food security on political stability could be masked by the effect of the later on the former. We intend to address this possibility using two different methods. In the first method, we will simply lag the variables for food supply and food quality by one year, in effect attempting to predict the current year's level of political unrest with the previous year's level of food security. However, this approach requires that the current year's measures cannot cause the previous year's, a reasonable though not necessary assumption. Therefore, we also intend to do an additional regression using instruments for food supply. This entails we first predict food supply using new variables highly correlated with food supply but not with political unrest. These predicted values are then included in the original model instead of the actual values for food supply, where we can now assume no reverse causation between them and our dependent variable.[17]

We intend to predict food supply using two variables for a country's agricultural potential: the concentration of tractors in a country-year and the percentage of arable land that country. A good instrument must satisfy two conditions: it must be highly correlated with the variable it intends to predict (the inclusion restriction) while remaining independently uncorrelated with the dependent variable (the exclusion restriction). Both tractor density and the abundance of arable land serve as fantastic predictors of food supply ($t = -11.46$ and 14.42, respectively, when regressed with all other independent variables), making them strong candidates for use as instruments. Their joint significance had an F-value of 143.81, well above the traditional cutoff of 10 for an effective joint instrument (Wooldridge 2009). Likewise, we find it hard to fathom how either variable and

[17] As per Hardin, Schmiediche & Carroll (2003), we in fact performed this process in using one command, "qvf," which allows for the use of instrumental variables using a negative binomial regression. As "qvf" does not allow for fixed effects models to be specified, only the results for the country and year dummy models will be reported.

especially their combination could be correlated with political instability except through their effect on food supply. Thus, we conclude that these variables together represent worthwhile instruments to control for reverse causation in our model, though we admit, given the impossibility of proving a negative, that we can never know for sure that we satisfied the exclusion restriction.

In addition, the nature of our dependent variable, as a simple sum of discrete events, gives equal weighting to all events with no regard for their individual magnitude. Though this specification accords with that of Bellemare, Lagi *et al.*, and Hendrix and Salehyan, we find it plausible that not only the frequency but the intensity of events increase with a decline in food security. Presumably, an aggregate of all individuals killed in a country in a given year from events of political unrest, a statistic similarly calculated from the SPEED database, would approximate the seriousness of the observed events in addition to their quantity. As the number of individuals killed is a count variable and has the same highly skewed distribution as the original event count data, a negative binomial regression remains appropriate for predicting this data. Therefore, we intend to specify an alternate model that approximates individuals killed rather than events observed for a given country-year as the principal dependent variable.

We could likewise have incorrectly specified our measures of food security. In our primary model, we measured absolute levels of food supply and quality, in keeping with both the previous quantitative literature and US food security policy, which tend to use metrics like absolute food prices, total individuals malnourished, or total food production to estimate food security. However, our conceptual model highlighted that spikes in food prices or declines in food availability were speculated to deteriorate the regime's output legitimacy and thus precipitate political unrest. This framework suggests that changes in food security, rather than their absolute levels, could represent the true cause of our dependent variable. Indeed, previous literature in psychology and sociology suggest a link between relative, rather than absolute, deprivation and crime or other social ills (*e.g.*

Walker and Mann 1987; Kawachia, Kennedy, and Wilkinson 1999; Bossert, D'Ambrosio, and Peragrine 2007). We shall thus test whether year-on-year percentage changes in our two food security variables better predict events of political unrest than do their absolute levels.[18]

Descriptive Statistics

As Table 1 makes clear, our data are measured and distributed very differently. Our two measures of food security are distributed roughly normally, each with a weak positive skew. Interestingly, looking at the base per capita data for these measures, two trends are apparent (Table 2). While the mean overall calories per day were roughly in line with USDA recommendations, the levels for fat supply were well below those recommendations and those for protein were well above those same recommendations (USDA and USHHS 2010). This suggests that fat supply could be a key limiting factor for diets globally. Additionally, all three variables trend up noticeably over time, further validating the inclusion of year controls in our models.

As mentioned earlier, the number of events per country-year is distributed highly irregularly, with a massive positive skew. Though the majority of observations saw no events occur, some countries saw more than 300 events in a single year. The alternative measure of political unrest that incorporates event intensity demonstrates a similar distribution. This is in part due to the uneven distribution of population, which the data in Table 1 suggests has a similarly strong skew. Also note, from Table 2, that observed events of political unrest decreased steadily over time, barring a spike between 1999 and 2003. The striking decline in the period from 2004 to 2009 is in large part due to weaknesses in data availability as we approach the present and should not bias our results too strongly due to the controls for the effects of time.

[18] Percentage change here defined as the difference between the current year and the previous year's value, divided by the previous year's value, see Appendix 1.

Among the political and economic controls included in the model, the distribution of the Polity IV score indicates that the bulk of the observations in the model are of democracies and anocracies (countries whose institutions are too weak or illiberal for Polity IV to consider them democratic), with a smaller number of strongly autocratic countries weighing down the mean. The economic variables not unreasonably have a marked positive skew, with most countries, for instance, experiencing mild rates of inflation, but some countries experiencing either hyperinflation or occasionally deflation in some years. Likewise, the two metrics used to predict food supply are distributed irregularly and with a positive skew, though tractor density appears much more disperse than does the percentage of arable land in a country. Also note that, while most variables have between 4,000 and 5,000 observations, tractor density has less than 3,000, dramatically reducing the precision of any estimates derived from these data.

Table 1: Descriptive Statistics

Years	1979-2009
Countries	172
Total Observations	5,054

Variable	Mean Value	Median Value	Standard Deviation	Range	Observations
Food Supply (1000s of kcal/day)	83,600	15,800	318,000	97.8 – 3,970,000	5006
Food Quality (% kcal fat/protein)	0.356	0.349	0.088	0.153 – 0.555	5006
Polity IV Score (-10 to 10)	1.7	5	7.2	-10 – 10	4305
GDP (Billions of 2005 Int$)	196	23.7	514	0.111 – 8,480	4353
Inflation (annual % change)	33.4	6.6	287.8	-13.1 – 11,749	4105
Population (1000s of people)	31,867.6	6,528	123,600.2	41 – 1,365,580	5006
Events (count/year/country)	4.9	0	16.4	0 – 312	5240
Deaths (count/year/country)	3,698.0	1	108,392	0 – 4,500,028	5240
Arable Land (% total land area)	14.7	10.4	13.8	0 – 72.1	4969
Tractor Density (per 100km^2 arable land)	537.6	122.8	1779.1	0.754 – 20,000	2986

Sources: *Food and Agriculture Organization (FAO)*, *Polity IV*, *World Bank, Cline Center for Democracy*

Note: *Number of countries reported represents the most countries in a single year (2006-2009). The true number fluctuates as a function of time. A number of countries were removed from the dataset for having insufficient data or for skewing event count data. Food supply data collected by FAO from country governments. Missing values estimated by FAO. Polity IV scores are distributed on a range from -10 to 10, where -10 represents pure autocracy and 10 represents pure democracy. GDP measured in 2005 international dollars, which represents the purchasing power parity of the US dollar in the United States, benchmarked to 2005. Inflation rates calculated based on year-on-year percentage change in consumer prices in the given country.*

Table 2: Distribution of Key Variables over Time

Year Range	Total Events	Mean Food Supply (kcal/capita/day)	Mean Fat Supply (g/capita/day)	Mean Protein Supply (g/capita/day)
1979 – 1983	5,591	2,532	69.0	69.4
1984 – 1988	6,797	2,571	71.6	70.8
1989 – 1993	3,444	2,574	73.1	71.0
1994 – 1998	3,735	2,606	74.0	72.6
1999 – 2003	4,773	2,671	77.3	74.7
2004 – 2009	1,090	2,763	81.9	77.5

Results

Our key results for our primary model do not corroborate the literature that suggested a negative relationship between food security and events of political unrest. Instead, using a negative binomial model that controlled for country and year specific effects, we found a significant positive influence of a greater overall food supply on the number of events of political unrest experienced in a country in that year ($p < 0.001$, see Table 3.3).[19] This positive effect of food security on political unrest remains, albeit slightly attenuated, when only country level effects are held constant ($p < 0.001$, see Table 3.1). To control for the potential that using two-way fixed effects biased our estimates when using a negative binomial regression, we also used a simple dummies for country and year, but the effect remained equivalently significant ($p < 0.001$, see Table 3.2). Further, an increase the quality of food also appears to significantly increase the predicted number of events of political instability, though this effect only obtains when we account for two-way fixed effects ($p = 0.007$, see Table 3.3). When we avoid using a fixed effects specification, the effect of quality appears negative, though it is far from significant ($p = 0.710$).

To help comprehend the magnitude of the effect of food supply on instability, we produced several projected outcomes in which we varied the key food supply variable while keeping all other regressors at their means. Using the country/year dummy model (Table 3.2), this coefficient suggests that an increase of food supply from the 25th percentile to the median would increase the predicted number of events in a given country fivefold, while when going from the median to the 75th percentile the increase is almost tenfold (holding all else at their means).[20] Using the country/year fixed effects model (Table 3.3), this method predicts an increase of roughly half an

[19] For all pooled regression models and those using fixed effects with OLS or Poisson regression models, please consult Appendix 2. As stated earlier, we do not think these models are appropriate for predicting event count data with the distribution our data have. However, the use of these models instead do not alter our findings significantly.
[20] Predicted events –25th percentile: 0.00390; 50th percentile: 0.0200; 75th percentile: 0.130

event from the 25th to 50th percentiles and a similar increase from the 50th to 75th percentiles.[21] Keeping in mind that 70 percent of country-years observed no events and the majority of the rest observed only one, the effect of food supply on events of insecurity appears substantively positive in both specifications, wholly contrary to our expectations.

As argued earlier, we had strong reason to believe that food security and political stability were intimately related and we suspected that the potential effect of political unrest on food security could be skewing our results. However, when we looked instead at the effect of the previous year's food metrics (both quality and quantity) on the number of predicted events of political unrest in the given year, the results appeared virtually identical to those from comparable models using the given year's food data (See Table 4.1 and 4.2). We also used data on the number of tractors per 100 square kilometers of arable land and the percentage of total land that is arable (two metrics presumably much less correlated with political unrest than food security) to predict the overall food supply in a given country-year and then used those predicted values in our original regressions (see Table 4.3).[22] Even here, the effect of the instrumented food supply on the predicted number of events of political instability remained positive and significant. The inclusion of the instrumented food supply measure does not seem to have affected the effect of food quality on events of political stability, which remains insignificant.

As these results conflicted with both our own conceptual model and previous quantitative studies, we wanted to ensure that our particular specifications did not overly influence our results. Therefore, we specified similar models that predicted the total number of individuals killed, across all events of political unrest, in a given country year (see Table 5.1 and 5.2). This would potentially

[21] Predicted events –25th percentile: -4.254; 50th percentile -3.884; 75 percentile: -3.433 (the negative values likely arise from the aforementioned issues with specifying fixed effects models using a negative binomial regression)
[22] As stated earlier, we actually performed both steps in one motion, via the "qvf" extension for Stata (Hardin, Schmiediche & Carroll 2003). However, the math and the concept are easier to comprehend as two separate steps and we thus describe it as such.

mitigate idiosyncratic deficiencies in our original dependent variable and introduce an approximation of event intensity, rather than simply event frequency. Despite this, the effects of the overall food supply on the number of deaths remained positive and significant, with a comparable magnitude to the effect of food supply on the number of events ($p = 0.009$ and $p > 0.001$ see Table 5.1 and 5.2, respectively). Interestingly, though, we found the effect of food quality on the predicted number of deaths from political unrest to be the reverse of the previous regressions. The dummy variable model now finds a significant positive effect for food quality on political unrest ($p = 0.02$) and the effect in the fixed effects model is no longer significant ($p = 0.073$).

We also tested a different set of specifications for the two food security variables (see Tables 5.3-5.6). We did so to investigate whether our initial choice of a level variable instead of a change variable to measure the effect of changes in food security was appropriate. As individuals might use their past experience as a reference point to judge their well-being, it would not be unreasonable to assume that one's relative food security would weigh more heavily than one's overall food security. Indeed, when we measured the year-on-year percentage change in food supply instead of overall levels of food supply, we found this variable had a significantly negative effect on events of political unrest across both the factor dummy and fixed effects specifications ($p = 0.003$ and $p = 0.025$, see Tables 5.3 and 5.4, respectively). Interestingly, in neither case was the effect of the second derivative of food quality on political unrest significant. In the interest of comparing these disparate effects, we tested one more set of models that included both the level and the change variables for both food supply and food quality (see Tables 5.5-5.6).[23] Little changed between these models and those previous. The effect of absolute levels of food supply remains equally positive and significant ($p < 0.001$ for both) and the effect of change in food supply remains equally negative and significant ($p <$

[23] Do note that these combined models, insofar as they attempt to vary a level while holding the change variable constant (and vice versa), do not accurately reflect the partial effects of either and their specifics should be interpreted with caution. We included them simply to note that the differential *ceteris paribus* effects of absolute and relative food security hold up even when controlling for the other.

0.001 and p = 0.012, respectively). The effect of both the absolute levels of and changes in food quality still appears ambiguous and infrequently significant.

The estimated effects of the other macroeconomic independent variables remained roughly in keeping with expectations, which increases our confidence in our results. Across most models, we found that increases in year-on-year inflation rates significantly increased the number of predicted events of political instability. Similarly, increases in the overall wealth of a country tended to decrease these predicted events, though not often significantly, perhaps due to the reasonably high correlation between country GDP and country population (r = 0.623). Our estimates for the effect of democratic governance appeared more erratic. Some models suggested a significant positive effect on political unrest while others suggested the effect was negative and some even found no significant effect at all. The *ceteris paribus* effect of a greater population on political unrest was likewise ambiguous and frequently insignificant.

Table 3: Primary Negative Binomial Regression Models

	(1) Country FE	(2) Country/Year Dummy	(3) Country/Year FE
ln(Food Supply) (kcal/day)	0.327* (4.61)	1.813* (4.92)	0.440* (5.90)
Food Quality (% kcal fat/protein)	1.106 (1.72)	-0.713 (-0.37)	1.828* (2.70)
Polity IV Score (-10 to 10)	-0.0208* (-3.92)	-0.0262* (-2.98)	0.00914 (1.66)
ln(GDP) (1000s of 2005 Int$)	-0.0584 (-0.99)	-1.281* (-4.48)	-0.0135 (-0.22)
Inflation (annual % change)	0.000206* (5.13)	0.000125 (1.46)	0.000148* (3.72)
Population (1000s of people)	-0.000000811* (-3.48)	0.00000203 (1.13)	-0.000000572* (-2.28)
Country Effects	*	*	*
Year Effects		*	*
N	3177	3300	3177
chi^2	104.8	2699.5	349.2
p-value	2.44e-20	0	1.89e-53

t statistics in parentheses
* $p < 0.05$

Table 4: Regressions for Endogeneity

	Lagged Models		Instrumented Model
	(1) C/Y Dummy	(2) C/Y FE	(3) C/Y Dummy
ln(Food Supply) (Lagged one year)	2.169* (6.04)	0.467* (6.29)	
Food Quality (Lagged one year)	0.241 (0.13)	2.048* (3.06)	
Food Quality (% kcal fat/protein)			0.618 (0.25)
Polity IV Score (-10 to 10)	-0.0282* (-3.20)	0.00891 (1.62)	-0.0211 (-1.86)
ln(GDP) (1000s of 2005 Int$)	-1.525* (-5.30)	-0.0356 (-0.57)	-1.778* (-3.48)
Inflation (annual % change)	0.000130 (1.52)	0.000149* (3.72)	0.0000926* (2.13)
Population (1000s of people)	0.00000206 (1.15)	-0.000000597* (-2.38)	-0.00000157 (-0.80)
ln(Food Supply) (Predicted)			4.625* (3.24)
Country Effects	*	*	*
Year Effects	*	*	*
N	3296	3175	2009
chi^2	2715.5	352.9	
p-value	0	3.48e-54	

t statistics in parentheses
* $p < 0.05$

Table 5: Regressions Using Alternate Specifications

	Models using Individuals Killed Instead of Events		Models using Year-on-Year Percentage Change in Food Supply			
	(1) C/Y Dummy	(2) C/Y FE	(3) C/Y Dummy	(4) C/Y FE	(5) C/Y Dummy	(6) C/Y FE
ln(Food Supply) (kcal/day)	3.190* (2.62)	0.354* (4.32)			2.02* (5.48)	0.454* (6.06)
Food Quality (% kcal fat/protein)	13.58* (2.33)	-1.227 (-1.80)			-0.367 (-0.18)	1.86* (2.74)
Polity IV Score (-10 to 10)	-0.00729 (-0.23)	0.0368* (5.43)	-0.0267* (-2.99)	0.00322 (0.60)	-0.027* (-3.07)	0.00871 (-1.30)
ln(GDP) (1000s of 2005 Int$)	-3.903* (-4.33)	0.0253 (0.39)	-0.968* (-3.65)	0.291* (10.98)	-1.47* (-5.10)	-0.0278 (-0.44)
Inflation (annual % change)	-0.000222 (-1.11)	0.0000381 (0.46)	0.000133 (1.54)	0.000142* (3.59)	0.000122 (1.46)	0.000148* (3.65)
Population (1000s of people)	0.00000780 (1.63)	-0.000000697* (-2.71)	0.00000175 (0.95)	0.00000155 (0.75)	0.00000189 (1.05)	-0.00000584* (-2.33)
Δ Food Supply (annual % change)			-3.08* (-3.00)	-1.894* (-2.24)	-3.73* (-3.65)	-2.12* (-2.52)
Δ Food Quality (annual % change)			-1.38 (-1.34)	-0.953 (-1.19)	-1.39 (-1.31)	-1.05 (-1.30)
Country Effects	*	*	*	*	*	*
Year Effects	*	*	*	*	*	*
N	3300	2878	3296	3175	3296	3175
chi²	1393.1	329.7	2688.7	316.3	2719.67	356.29
p-value	2.80e-202	2.75e-67	0	5.09e-47	0	0

t statistics in parentheses
* $p < 0.05$

Discussion

Our results suggest that increases in the overall food supply within a country in fact increases the predicted number of events of political instability, in contradiction with both our initial hypothesis and similar studies. They also suggest, much more ambiguously, that improvements in food quality have a similarly positive effect on the incidence of political unrest in a country.

The mechanism for such an unintuitive effect is unclear. One possible explanation stems from the greed hypothesis, that greater levels of food availability empower dissident groups to fund themselves through food sales on the international market. One complication with this explanation stems from the bulk of food products relative to their price, especially compared to more commonly exported commodities, like diamonds, metals, or oil. For instance, a previous study found no significant effect of the endowment of timber resources, a similarly bulky commodity, on the incidence of civil war (Ross 1999). Alternatively, a greater food supply could have more internal benefits for potential dissidents, by perhaps providing the capacity to feed guerrilla forces or sustain localized opposition to the central government. The additional rents that governments can collect from greater agricultural production, importation, and exportation could provide another motivation to explain the predicted increased frequency of politically destabilizing events. The resultant greater reward for holding reins of power could induce a higher frequency of coup d'états or power struggles between regional and national governments. Furthermore, the greater availability of resource rents from agriculture could affect the incentives for those currently in power. As Bates (2008) articulates in his work on state failure, elites in developing states face two options for how to exercise their power: they can use their positions to improve their country's wellbeing, with the expectation that doing so will increase both their likelihood to remain in power and the rewards for holding power, or they can exploit their positions for short-term personal gain, though with more severe longer term consequences for themselves and their country. The present availability of

higher agricultural returns could induce more leaders towards the latter approach, which would presumably diminish their countries' political stability. Further research that disaggregates the kinds of political instability linked to an increased food supply and investigates the relationship between both of these factors and the rents governments receive from agriculture would help distinguish between these phenomena.

Our results can be partially reconciled with the previous quantitative literature if one considers the *ceteris paribus* effect of change in the food supply in addition to the effect of absolute levels of the same. Our models predict that decreases in food supply within a country, independent of the level at which one starts, would significantly increase the observed events of political unrest. This finding in part preserves the grievance-based explanations for the linkage between political unrest and food security, though it shifts the emphasis from absolute deprivation to relative deprivation. It also coheres with Hendrix and Salehyan's findings that increased volatility in rainfall precipitate riots, though in doing so, directly contradicts Bellemare's conclusion that the level and not the volatility of food prices predicts increases in unrest. It also undermines the objectives of US food aid policy, which tend to seek to alleviate absolute hunger rather than increased hunger relative to a previous baseline. Further sociological and psychological research into the weight of relative deprivation with respect to hunger in motivating activism or anti-regime attitudes might help clarify this effect.

However, we offer a few caveats for those researchers and policymakers hoping to build off our results. Our data were not entirely pristine, with a number of countries removed from the sample for entirely lacking food supply data. These countries included those, like Afghanistan or Somalia, that have high levels of political unrest and that lacked data precisely because of that unrest. Further, we systematically missed economic or political data from a selection of small or disorganized countries, especially from those observations further in the past, leading to a number of

observations and occasionally whole countries to be removed from our dataset or regression models. All of these factors open the possibility of selection bias in our results. Further, since we aggregated our main dependent variable from a collection of observed events, biases inherent in that dataset could have also skewed our results. For instance, as all of the sources of the original database come from either North America or Europe, we might have overestimated the events in countries from those regions and underestimated those from elsewhere. This specific possibility is especially problematic when one considers that Europe and North America tend to have the highest levels of food security in the world. In addition, we made no attempt to control for the potential for autocorrelation in our model as a result of our choice to specify a negative binomial regression instead of standard OLS. We think this trade off worthwhile and we avoided making conclusions based on results of spurious significance ($0.05 < p < 0.1$) in part to compensate, but this issue remains nonetheless. Finally, though we attempted to control for the most obvious time-variant confounding variables, others inevitably exist and thus could bias any or all of our estimates. The similar potential also exists for our instruments to have in truth failed the exclusion condition. Further studies using alternate datasets or additional variables might lessen these issues and discover if our results still hold.

That these results differ so strongly from the previous literature and official US policy certainly opens the possibility that we have erred at some stage, but it should not be surprising that we arrived at such unexpected results. Most previous quantitative studies looked specifically at the relationship between international food prices and international political unrest over time, with no effort to parse out unobserved country-level effects on food prices. As we showed earlier, there is a secular trend towards greater food availability and lesser political instability in the world, which will distort efforts to link food prices and political unrest over time. Moreover, even those studies that did employ fixed effects models used problematic metrics for political instability, either defining it so

narrowly as to only include food riots (which verges on "question-begging") or so broadly to invite irreconcilable problems with endogeneity. No previous study uses actual country-level count data of events of political instability, as we did. Likewise, previous studies relied on food prices as a proxy of food security instead of measuring the available food supply directly. Further, though our findings contradict assumptions embedded in US food security policy, these assumptions themselves did not cohere with prevailing findings on the origins of political violence, which identify individual greed, rather than group grievances, as the primary driver of political unrest.

Thus, our findings, if sound, hold significant implications for US development policy. Policymakers traditionally assume that US developmental, diplomatic, and foreign policy goals are all consonant with increased global food security. However, our results suggest that the US government may have to weigh the obvious humanitarian benefits to fighting hunger abroad against the potential detriments to national security and global political stability for doing so. It may even be the case that the second order effects on political stability of increasing food security within a country may overwhelm the first order benefits, leading current US food security efforts to potentially cause more harm than good globally. If, as Vice President Biden suggested, US development policy seeks to increase both the well-being of its recipients along with US national security interests, it may be more profitable to focus instead on boosting individual wealth directly and working with national governments to control hyperinflation instead of concentrating on increasing food security, as our results suggest the former two interventions are better aligned with both of those goals. However, given the importance of our research question on US food security policy, future research might seek to ensure that our results are indeed sound before any substantial adjustments to development policy are made.

Conclusion

In the end, our findings seem to corroborate the interpretations from the popular press of rising food prices instigating the Arab Spring. After all, the price spike did not push the average Egyptian or Tunisian, impoverished though they were, to the brink of starvation. It did, though, severely reduce their food security from the levels in 2010 and that may have made all the difference. But our results also restrain sweeping generalizations from the same, as the concomitant reduction in absolute levels of food may undermine the preconditions needed for political unrest. Thus, we cannot proclaim that our potentially food insecure future will necessarily also be more unstable. We can only speculate that food will remain important for the survival of states, just as it always has.

Appendix 1: Building the Dataset

In building our dataset, we made a number of alterations to the original source data to further our analysis. Briefly, we adjusted several variables, dropped almost thirty countries, and aggregated the data of others.

Our source for our event count data, the SPEED database, reports discrete events rather than aggregates by country and year. We performed a simple summation of data points by country and year to have this data conform to the panel format of the other sources. However, this does introduce some inherent error, as it may over count discrete events that are nonetheless a part of a broader phenomenon. We did not attempt to correct for this possibility manually. Additionally, to reconcile this nominal event count data with our per capita variables (food supply and country wealth), we multiplied the per capita data by the population data reported by FAO and then included that variable as a regressor in all models. To create the food quality variable, we took the aggregate nominal fat supply and protein supply data reported by FAO per country-year, multiplied each by their respective per gram caloric contents (nine and four, respectively), and then divided it by the nominal food supply (in kilocalories/person). To create the change variables for both food supply and the quality index, we subtracted the previous year's nominal food supply/food quality from the current year's and then divided that amount by the previous year's value to approximate a year-on-year percentage change for each country-year (exempting 1979 observations, of course).

We also dropped a sizable number of countries from our dataset. All data for small, sub-state entities (like Puerto Rico) were dropped, as many of our data sources only looked at sovereign states, including the FAO. In addition, the FAO does not record data for several sovereign states, because they either were too instable to produce reliable data (like Afghanistan) or they were too agriculturally deficient to produce adequate food data (like Qatar). Likewise, Polity IV does not record data for a number of countries, primarily small island states, which were also excluded from

our data. Finally, at the advice of Peter Nardulli from the Cline Center for Democracy, we dropped all observations from the United States, as event reports were heavily skewed towards domestic events. A full accounting of all states removed from our sample can be found in Table A.

Differential reporting of outcomes by country, especially for those countries that have undergone fusion or fission since 1979, also confounded our attempts to create a coherent data set. SPEED generally assigns events to the country in which the event would have taken place at present (2012), while FAO, the World Bank, and Polity IV assign their observations to the country that existed at the time. Further, these latter three do not record start and end points for countries equivalently. In attempting to resolve these differences, we generally opted to assign observations to the country that existed at the time, for instance, aggregating the event data for the Soviet successor states from Armenia to Uzbekistan into one USSR observation for all years between 1979 and 1991 (the last year FAO has data for the USSR). Resolving the different and inconsistent reporting around Germany and Yemen, however, overwhelmed our methodology and we resorted to dropping them from the dataset (see Table A). Finally, the FAO began jointly reporting data for Belgium and Luxembourg beginning in 2000. Given Luxembourg's small size and population, we considered this combined data effectively equivalent to that of Belgium and coded it accordingly. Luxembourg had been removed from the sample for alternate reasons. A full accounting of the problematic countries and the resolutions we used can be found in Table B.

Table A: Countries Dropped

Countries Dropped	Reason
Afghanistan	FAO
Antigua and Barbuda	Polity IV
Bahamas	Polity IV
Bahrain	FAO
Barbados	Polity IV
Belize	Polity IV
Bermuda	Polity IV
Brunei	Polity IV
Dominica	Polity IV
Equatorial Guinea	FAO
French Polynesia	Polity IV
Gaza/West Bank	FAO
Germany (all)	Naming Issues
Iraq	FAO
Luxembourg	Polity IV
Maldives	Polity IV
Malta	Polity IV
Netherlands Antilles	Polity IV
New Caledonia	Polity IV
Papua New Guinea	FAO
Qatar	FAO
Saint Kitts and Nevis	Polity IV
Saint Lucia	Polity IV
Saint Vincent and the Grenadines	Polity IV
Samoa	Polity IV
Sao Tome and Principe	Polity IV
Seychelles	Polity IV
Singapore	FAO
Somalia	FAO
Taiwan	FAO
United States	SPEED
Vanuatu	Polity IV
Vatican	FAO
Western Sahara	FAO
Yemen (all)	Naming Issues
Zaire	FAO

Table B: Countries Altered

Countries Altered	Range	Changed to
Armenia	1979-1991	USSR
Azerbaijan	1979-1991	USSR
Belarus	1979-1991	USSR
Belgium/Luxembourg	2000-2009	Belgium
Bosnia	1979-1991	Yugoslavia
Croatia	1979-1991	Yugoslavia
Czech Republic	1979-1992	Czechoslovakia
East Timor	1979-1993	*Unchanged*
Eritrea	1979-1992	Ethiopia
Estonia	1979-1991	USSR
Georgia	1979-1991	USSR
Germany	1979-2009	*Dropped*
Kazakhstan	1979-1991	USSR
Kyrgyzstan	1979-1991	USSR
Latvia	1979-1991	USSR
Lithuania	1979-1991	USSR
Macedonia	1979-1991	Yugoslavia
Montenegro	1979-1991	Yugoslavia
	1992-2006	Serbia and Montenegro
Moldova	1979-1991	USSR
Russia	1979-1991	USSR
Serbia	1979-1991	Yugoslavia
	1992-2006	Serbia and Montenegro
Slovakia	1979-1992	Czechoslovakia
Slovenia	1979-1991	Yugoslavia
Tajikistan	1979-1991	USSR
Turkmenistan	1979-1991	USSR
Ukraine	1979-1991	USSR
Uzbekistan	1979-1991	USSR
Yemen	1979-2009	*Dropped*

Appendix 2: Other Regression Models

We believe that the models outlined in the body of the paper represent the best approximations of the true effect of food security on political stability. Nevertheless, in the name of thoroughness and to ensure that the results we found were not a result of the model specifications we chose, we have included in this appendix a variety of alternate specifications and the estimates they produce.

Table X portrays the estimates from three different pooled models, with Table X.1 showing a standard OLS pooled model, Table X.2 a pooled Poisson regression model, and Table X.3 a pooled negative binomial regression model. These models do not control for country- or year-specific fixed effects, and thus suffer from inescapable omitted variable bias from a wide variety of variables that do not vary significantly within a specific country across time or affect all countries equally in a given year. Potential problematic omitted variables include the size, climate, culture, and history of individual countries. It also includes problems specific to our database, as with the attenuation of observed events in the years 2004 to 2009. Furthermore, it should be noted that both country and year fixed effects were jointly significant across all models tested previously. Even given these caveats, the similarity between these models and with our primary model is striking.

Table Y conveys the estimates produced by now including these fixed effects in OLS (Tables Y.1 and Y.2) and Poisson (Tables Y.3 and Y.4). As stated earlier, the unusual dispersion of our dependent variable would violate the zero conditional mean assumption, making an OLS regression inappropriate, even when we control for fixed effects. Likewise, the overdispersion of these data violates the central assumption of a Poisson regression, that the conditional mean equals the conditional variance. Despite these issues, though, both of these specifications produce estimates consistent with each other and our initial results.

Finally, in Table Z, we have included a variety of potential alternate specifications of our original negative binomial fixed effects model, to test the robustness of those results. The regressions in Tables Z.1 and Z.2 test the speciousness of our consideration of food quality as an independent variable, as opposed to solely food quantity by removing that variable from the model. Those in Tables Z.3 and Z.4 test instead our proposition that the effect of the overall food supply and GDP on political unrest attenuate over time and thus should be logged before being used as regressors by including only the nominal values of these variables. Tables Z.5 and Z.6 investigate whether the 13 years of our sample that occurred during the Cold War biased our results by looking specifically at those observations that occurred after 1991. Finally, the estimates included in Tables Z.7 and Z.8 come from two-stage models that used only one of our two instruments (tractor density and availability of arable land, respectively) to ensure that our use of both of these variables to predict food supply, rather than one or the other individually, did not overly influence our results. Of note, despite all the differences in these models, the effect of overall levels of food supply remains positive and significant across all specifications, except for those that do not log that variable. In the un-logged specifications, the estimated effect appears negative and of ambiguous significance. None of the other estimated effects for other independent variables appeared consistently significant or pointing in the same direction across all of these models, further emphasizing the robustness of our original result.

Table X: Pooled Models

	(1) OLS	(2) Poisson	(3) Negative Binomial
ln(Food Supply) (kcal/day)	0.812 (1.84)	0.300* (18.62)	0.206* (2.77)
Food Quality (% kcal fat/protein)	-10.61* (-2.38)	-1.848* (-12.82)	-1.918* (-2.98)
Polity IV Score (-10 to 10)	-0.0116 (-0.27)	-0.00744* (-5.63)	-0.0222* (-3.10)
ln(GDP) (1000s of 2005 Int$)	1.109* (2.93)	0.219* (16.46)	0.389* (6.16)
Inflation (annual % change)	0.000472 (0.60)	0.0000823* (4.23)	0.000153 (1.14)
Population (1000s of people)	0.0000155* (7.25)	-0.000000229* (-5.97)	-0.000000342 (-0.99)
N	3300	3300	3300
adj. R^2	0.092		

t statistics in parentheses
* $p < 0.05$

Table Y: OLS and Poisson Fixed Effects Models

	OLS		Poisson	
	(1) Country FE	(2) Country/Year FE	(3) Country FE	(4) Country/Year FE
ln(Food Supply) (kcal/day)	6.320* (3.09)	8.253* (3.64)	0.999* (11.68)	2.147* (19.27)
Food Quality (% kcal fat/protein)	-32.17* (-2.67)	-27.23* (-2.27)	-2.119* (-3.96)	-1.314* (-2.31)
Polity IV Score (-10 to 10)	-0.193* (-2.89)	-0.140* (-2.03)	-0.0321* (-12.89)	-0.0228* (-8.85)
ln(GDP) (1000s of 2005 Int$)	-6.015* (-4.34)	-1.655 (-0.95)	-1.404* (-23.16)	-0.171* (-2.29)
Inflation (annual % change)	0.000299 (0.42)	0.000475 (0.67)	0.0000605* (2.83)	0.0000851* (4.17)
Population (1000s of people)	-0.000106* (-7.71)	-0.000114* (-8.31)	0.000000717* (2.96)	-0.00000131* (-4.98)
Country Effects	*	*	*	*
Year Effects		*		*
N	3300	3300	3177	3177
adj. R^2	0.008	0.026		
chi^2			2674.4	2277.4
p-value			0	0

t statistics in parentheses
* $p < 0.05$

Table Z: Tests for Robustness

	No quality variable		Nominal Variables		Post 1991 data		Instrument: Tractor	Instrument: Arable Land
	(1) C/Y Dummy	(2) C/Y FE	(3) C/Y Dummy	(4) C/Y FE	(5) C/Y Dummy	(6) C/Y FE	(7) C/Y Dummy	(8) C/Y Dummy
ln(Food Supply) (kcal/day)	1.829* (4.99)	0.313* (5.45)			0.128 (0.19)	0.254* (2.27)		
Food Quality (% kcal fat/protein)			-2.745 (-1.43)	1.138* (2.13)	-0.0799 (-0.03)	0.564 (0.60)	3.048 (0.86)	-0.765 (-0.38)
Polity IV Score (-10 to 10)	-0.0263* (-2.98)	0.0113* (2.08)	-0.0241* (-2.72)	0.00984 (1.71)	-0.0512* (-3.46)	-0.0113 (-1.39)	-0.0275* (-2.03)	-0.0271* (-2.38)
ln(GDP) (1000s of 2005 Int$)	-1.308* (-4.71)	0.105* (2.39)			-0.694 (-1.40)	0.107 (1.24)	-2.567* (-2.56)	-1.473* (-3.64)
Inflation (annual % change)	0.000126 (1.47)	0.000143* (3.60)	0.000138 (1.55)	0.000135* (3.52)	0.000225 (1.26)	0.000167 (1.66)	0.000708 (1.37)	0.000106* (2.47)
Population (1000s of people)	0.00000201 (1.12)	-0.000000498* (-2.00)	0.00000374 (0.80)	0.00000417* (3.10)	0.00000107 (0.31)	8.08e-08 (0.23)	-0.00000432 (1.33)	0.00000157 (1.19)
Food Supply (kcal/day)			-1.07e-09 (-0.63)	-1.49e-09* (-2.68)				
GDP (1000s of 2005 Int$)			-1.20e-11 (-0.04)	3.94e-10* (4.34)				
ln(Food Supply) (Predicted – Tractor)							7.750* (2.19)	
ln(Food Supply) (Predicted – Arable)								3.034* (2.48)
Country Effects	*	*	*	*	*	*	*	*
Year Effects	*	*	*	*	*	*	*	*
N	3300	3177	3300	3177	2360	2219	2009	3255
chi²	2699.3	341.8	2667.5	.	2051.8	172.0	1539.7	2628.3
p-value	0	1.70e-52	0	.	0	2.43e-24	1.56e-232	0

t statistics in parentheses
* $p < 0.05$

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