Graduate Thesis Edition

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Editor's Remarks

I am pleased to present the third annual *Georgetown Public Policy Review Graduate Thesis Edition*, highlighting original research and policy analysis conducted by recent McCourt School of Public Policy (MSPP) graduates. As part of the MSPP capstone experience, students have the option of completing a quantitative thesis in partial fulfillment of the Master of Public Policy degree at Georgetown University. The *Graduate Thesis Edition* is reserved for theses, nominated by faculty, that showcase exemplary policy analysis and particularly thoughtful writing.

The *Graduate Thesis Edition* is peer reviewed by Georgetown faculty and publishes condensed versions of the authors' original theses. The result has been a powerful addition to *The Review's* annual journals that draws on empirical analysis from the broader academic and policy communities.

Utilizing a new historical data source on news of international civil unrest, Christopher Adams assesses the link between food security and political instability. In contrast to some prior studies, Adams finds a positive relationship between absolute levels of food security and political protests, while increases in relative changes in food security within a country result in a decrease in instability. His analysis suggests the need for nuance within the study of food security and United States foreign aid polices.

Drawing upon data from the first year of implementation, author David Dickey-Griffith explores the impact of the School Improvement Grant Program (SIG) on outcomes in Texas public schools. Dickey-Griffith's analysis suggests that SIG has a negative or insignificant relationship with student achievement but may have a positive relationship with high school graduation rates. These results provide a foundation for follow-up work to examine the long-term impact of the SIG program.

The Extractive Industries Transparency Initiative (EITI) is designed to help countries rich in natural resources to disclose corporate payments and allow external audits. Fernando Londoño estimates that countries that join EITI experience an increase in foreign direct investment, lending validation to the incentives of the program.

In her work, Katherine Morris examines the validity of the popular United States Department of Agriculture (USDA) food desert metric for predicting local obesity rates. She finds that the scarcity of food resources in a community has a positive and significant relationship with county-level obesity but that the size of correlation is relatively small in magnitude. Her results suggest a need for further evaluation and refinement of the USDA measure.

Continuing the theme of domestic food policy, Catlin Nchako examines the sensitivity of the Supplemental Nutrition Assistance Program (SNAP) to changes in low-skilled unemployment. He finds a positive and significant relationship between unemployment among low-skilled individuals and SNAP take-up rates, indicating that participation in the food program is countercyclical to local economic conditions.

Each author carves out a unique analysis in the quantitative study of public policy, greatly contributing to their respective fields of research. We cannot thank our authors—Adams, Dickey-Griffith, Londoño, Morris, and Nchako—enough for working with us throughout the editorial process. We hope our readers find this volume of the *Graduate Thesis Edition* rewarding and thought provoking.

On behalf of *The Review*, I would also like to extend our gratitude to those members of the MSPP community who greatly enhanced our efforts this year, including Robert Bednarzik, our faculty advisor; Barbara Schone, MPP faculty director and thesis coordinator; and the MSPP thesis advisors: Gurkan Ay, John Christian, Alan de Brauw, William Encinosa, Matthew Fleming, Peter Hinrichs, David Hunger, Andreas Kern, Donna Morrison, Yuriy Pylypchuk, Adam Thomas, Christopher Toppe, Thomas Wei, and Andrew Wise.

I am most grateful to have worked with a remarkable group of peers to carry on *The Review's* high-caliber contributions to public policy discourse. This publication is the result of the dedication and talents of Rachel Spritzer and each member of our exceptional print and copy editing teams.

Finally, I would like to extend special thanks to the Executive Team: Nora Gregory, Jacob Patterson-Stein, Rachel Spritzer, Aaron Gregg, Cristina Lopez G., David Thomsen, and Jose Gonzalez Echevarria. Thank you for your inspiring leadership and unfailing dedication to the myriad aspects of *The Georgetown Public Policy Review*. I look forward to completing another outstanding year.

Kristin Blagg

Editor in Chief

Bread and Riots: Assessing the Effect of Food Security on Political Stability

Christopher S. Adams

Christopher Adams completed his Master of Public Policy from the McCourt School of Public Policy in 2013. Matthew Fleming, PhD served as his advisor. Currently, he works as a Senior Associate Analyst for Analytic Services, Inc. as part of the organization's Analyst Development Program. Adams also holds a BA in Biology and Government from Bowdoin College.

Abstract

Policymakers routinely argue that food security undergirds political stability. While some researchers have demonstrated that the two may be linked, the broader literature suggests that most internal conflicts arise from narrow avarice rather than common grievances. This paper seeks to address this seeming conflict using novel data sources. The results suggest, in contradiction to expectations, that increases in absolute levels of food security significantly increase the frequency of political protests. However, the research also finds that increases in the relative levels of food security significantly reduce political instability. If true, these findings suggest that the US government should consider alleviating both relative and absolute declines in food availability.

I. 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 content in the twilight years of the empire. In Paris in 1789 and Saint Petersburg in 1917, poor harvests led to bread riots that 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. During such periods, the political leadership appears helpless as the public starves, abetting political opposition to the government. 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 its subsequent political effects.[1]

Volatile food prices appear weakly correlated with political instability; however, this correlation 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 Ki-moon warned of mass unrest and instability. The 2008 price increase was indeed followed by numerous food riots across the developing world but did not produce political consequences as significant as those seen during the recent Arab uprisings (Topping 2008). Likewise, observers have connected the so-called "color revolutions" that spread across the post-Soviet world during the first decade of the twenty-first century to a wide variety of causes, but few argue that food insecurity triggered such upheavals. Indeed, the relevant literature on civil wars and unrest suggests that such conflicts are more likely the result of abundant profitable resources like oil and diamonds rather than the dearth of necessary resources such as basic foodstuffs.

Despite the apparent connection between the abundance of profitable resources and political instability, policymakers nonetheless invoke the importance of food security for political stability. In the wake of the 2008 price spike, the leaders of the world's largest economies committed billions of dollars to food aid in a statement that explicitly linked food insecurity with political unrest (G8 2009). Similarly, US officials have defended "Feed the Future," President Obama's $3.8 billion interagency food security initiative along similar lines, arguing that food assistance quells instability abroad and thus improves American national security.[2]

[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 20 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.

[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

Understanding the true relationship between food security and political instability is therefore a relevant policy problem because the Administration links political insecurity abroad with American national security worldwide. This paper tests the proposition that food insecurity causes political instability 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] I find, contrary to US policy but in line with the prevailing literature, that increases in absolute levels of food security, i.e. the total amount of foodstuffs available, are associated with an increase overall political instability. However, I find increases in relative levels of food security, i.e. year-on-year change in the amount of foodstuffs available, are associated with a decrease in political instability.

Before detailing these results I briefly outline the competing literature on the effect of food and other resource abundance on political unrest. From these, I build a complementary conceptual framework for how food security can influence political unrest, which will be the bedrock of the analytical model I will test. After summarizing the results of these tests, I explore their implications and offer a few tentative suggestions to future researchers and policymakers.

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

II. LITERATURE REVIEW

The prevailing view in the policy world holds that food insecurity precipitates political instability. Vice President Joe Biden articulated this view in a 2011 speech on global hunger:

> *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 the vice president's interpretation. Researchers investigating the influence of natural resources on political instability explain the linkage through two competing motivations: greed and grievance.[4] Proponents of the former argue that the abundance of valuable and portable resources, such as minerals, oil, or cash crops, allows self-interested groups to exploit internal conflict by harvesting and selling these goods and using their profits to fuel further conflict (Ross 1999). Grievance-based explanations, on the other hand, cite a dearth of vital resources, such as water, livestock, and staple crops, as instigating conflicts between groups or against the government (Diamond 2005). Comparative studies have traditionally found greed-centric explanations more persuasive for explaining civil war (Collier and Hoeffler 2000; Fearon and Laitin 2003). Wars require significant financial and organizational investments and are especially risky endeavors. These high costs therefore require a reward large enough to incentivize armed revolt. While political grievances can aid opportunistic actors in fomenting conflicts, a more potent motive is often necessary. Greed is one example of such a motive. However, the literature's emphasis on greed runs contrary to current US policy regarding food security, which holds that resource deprivation, rather than greed, leads to political instability.

Civil war represents only one extreme along the spectrum of political instability. In contrast to the above consensus, a number of scholars have proposed that resource scarcities, specifically regarding food or water, can precipitate a range of disorders that do not rise to the level of outright war. Homer-Dixon (1991), for instance, argues that environmental constraints lead to diminished agricultural yields, which in turn disrupt the social order within states. Conversely, both Rotberg (2005) and Bates (2008) attribute food insecurity, and associated ecological problems, to greed and dysfunction at the highest levels, while Cohen and Pinstrup-Anderson (1999) paint a more nuanced picture in which 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 but do not link their findings on hydrology to food shocks specifically. This is especially notable as Lagi et al. (2011b) find no correlation between the instance of extreme weather conditions and international food prices. In a separate but related paper, Lagi et al. (2011a) instead correlate the international food price index calculated by the FAO with instances of food riots globally. However, doing so limits the explanatory power of their findings. It therefore does not seem likely that

[4] 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

one can extrapolate this relationship to political unrest more generally, for which food riots serve as a biased proxy. Additionally, the researchers fail to distinguish between qualitatively different types of events, making no distinction between a minor protest in Bangladesh and the Syrian civil war. The researchers consider both of these as examples of food riots despite the difference in their severity. Finally, the researchers do not control for the possibility that food prices may have been independently increased by global instability. Bellemare (2011) uses instrumental variables to avoid the problems with causality but otherwise suffers similar problems and further fails to capture variation at the national level, looking solely at international time-series data.

Arezki and Bruckner (2011), with the International Monetary Fund, address many of these issues. For instance, 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 excludes a number of factors that dictate prices in a particular country, most notably government interventions such as tariffs, export taxes, and subsidies. Arezki and Bruckner also fail to account for issues of endogeneity in their model and, unlike Bellemare, cannot persuasively demonstrate that food prices precipitate instability rather than the reverse (as Rotberg and Bates theorize). Indeed, this relationship disappeared when Arezki and Bruckner looked specifically at the effect of the previous year's food prices on political instability, suggesting that the linkage was not causal. Finally, Arezki and Bruckner, like the remainder of the above papers, only consider international food prices rather than measures of the food situation within each country. An examination of country-specific food security would be able to better measure the true effect on political instability within that country and thus perhaps reconcile the differing theoretical arguments about the linkage between food shortages and political unrest.

III. CONCEPTUAL FRAMEWORK

This paper seeks to build on these previous works. In accordance with current US development policy, I hypothesize the existence of a positive relationship between food security and political stability, with greater food security enhancing political stability. As with previous empirical studies, I anticipate that deficiencies in food security worsen the well-being of the average citizen, which in turn rob 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. Diminished food security could reduce political stability through less direct channels such as inducing governments to seek other forms of legitimation independent of the public well-being or increasing food prices to increase inflation and further rob the government of its legitimacy.

However, causality need not flow solely from food security to political stability. Indeed, as Cohen and Pinstrup-Anderson (1999) outlined, political unrest frequently precipitates food crises within countries. Growing political unrest disrupts trade internationally and internally, limiting both the availability and access elements of food security, due in part to the capricious actions of repressive regimes or the realities of intrastate conflict. Political unrest also frequently displaces whole groups of people, uprooting them from their homes and traditional food sources. This apparent effect of political unrest on food security requires me to consider the potential for reverse causality in my 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, as more autocratic regimes would be presumed to experience more food insecurity and greater upheaval across equivalent levels of food insecurity. Likewise, 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). Finally, states with insufficient capacity for agriculture will face problems producing and distributing sufficient food in a crisis, which can also amplify the effect of food shortages. My model will need to control for these country-specific factors.

IV. DATA AND METHODS

Compared to previous studies, this paper uses novel and hopefully more accurate data sources to measure both food security and political instability within a particular country.

I use the aggregate food supply measures collected by the FAO as a direct measure of in-country food supply, rather than inferring such data from international food prices. These data are collated from self-reported yield figures and then modified using similar data on agricultural imports and exports to calculate the total foodstuffs available within each country in a particular year. FAOStats (2012) reports this information in kilocalories per capita per day. I 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, which suggest a higher quality and more varied diet.[5] I use 1979 as the starting point for all panel data since the FAO only began tracking such information in that year (FAOStats 2012).

Predicating my 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, doing so allows me to use country-year panel data for all variables rather than time series data using international averages, in contrast to Bellemare or Lagi et al. (2011b). Additionally, unlike Arezki and Bruckner, my data captures more detailed levels of food availability, rather than relying on imputed measures. However, this points to a significant potential disadvantage in using FAO's food supply data rather than international food prices. While the latter are predicated on prices on the international market, the former relies solely on official government sources (FAOStats 2012). This leads to reliability issues as individual governments may have an incentive to misrepresent their food supply. Additionally, some countries do not respond to these surveys at all, either out of dysfunction or pique, which could potentially further bias my results. 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 (such as Somalia and Afghanistan) or with negligible internal food production (such as Qatar and Bahrain) do not report food supply data to the FAO at all and have therefore been removed from the sample (FAOStats 2012).

I 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 within its borders. But this 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 characterizes stable autocratic states as unstable. Instead, this paper will use event data to proxy political stability, but, unlike previous studies, the data are not restricted solely to instances of food riots and are linked specifically to the country of origin.

Specifically, the data come from the Social, Political, and Economic Event

[5] Calculated using the approximation that 1 gram of fat = 10 calories and 1 gram of protein = 4 calories.

Database (SPEED), collated by Dr. Peter Nardulli and others at the Cline Center for Democracy at the University of Illinois.[6] Using automated search protocols, Nardulli and his colleagues combed through the complete archives of the *New York Times* and the *Wall Street Journal* from 1946 to 2009 for instances of instability such as coups, group violence, and anti-regime protests. Finding these sources lacking in international coverage, the researchers also included reports from the Foreign Broadcast Information Service and the Summary of World Broadcasts because the former sources lacked broad international coverage. The latter sources contain English language summaries of tens of thousands of newspapers from most countries globally, collated by the CIA and BBC, respectively. I collapsed the data from SPEED into simple counts of events per country-year to be used as my primary dependent variable. As with the independent variable, this data begins in 1979 and extends to 2009. On the personal advice of Dr. Nardulli, I removed the United States from my sample, as the *New York Times* and *Wall Street Journal* are heavily weighted towards domestic coverage, complicating comparisons between the United States and other countries.

V. ANALYSIS PLAN

I use regression analysis to predict events of political unrest in a country given different levels of food security in order to test my hypothesis that food security is positively associated with political stability. As Gould (2011) argues, the Poisson regression model is most appropriate for attempting to predict count data.[7] This is especially necessary in this instance as most country-years did not have a recorded event, leading to a high concentration of null observations relative to positive ones (see Figure 1). 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, while the variance is assumed to be a simple function of the conditional mean (Wooldridge 2009).

However, this is often an unrealistic assumption for a given set of data: many real-life datasets have greater variability than assumed by a simple Poisson distribution, leading to a problem of overdispersion. Some methodologists, like Wooldridge and Gould, assert that this overdispersion can be easily corrected for, but the majority of researchers have instead moved towards correcting for

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

[7] 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 my own, the latter is more appropriate (Wooldridge 2009).

Figure 1: Distribution of Event Data

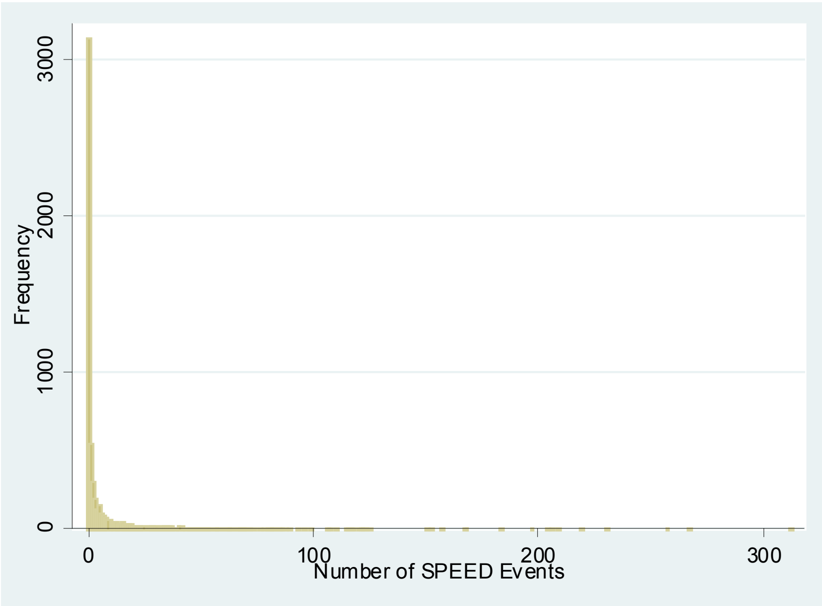

Figure 1: Frequency graph of the number of events recorded for each observation. Over 70 percent of the observations have zero events and, of those that have at least one event, roughly 60 percent had only one event recorded.

overdispersion by using a negative binomial regression (G. Krause 1994; Simmons and Elkins 2004; V. Krause, Suzuki, and Witmer 2006). A negative binomial regression relies on the negative binomial distribution instead of the Poisson distribution and thus allows the estimated conditional variance to vary independent of the conditional mean.[8] The negative binomial regression in effect 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).[9] I primarily use negative binomial regression models to predict the event count variable, since the greater variance enabled by the negative binomial regression better approximates my data.[10]

My initial specification will use event counts per country-year as the dependent variable and food supply measured in both quantity (kilocalories of food per day) and quality (as

[8] 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 are obtained. For instance, one could use the negative binomial distribution to determine the likelihood of surviving various numbers of rounds of Russian roulette with two bullets in the chamber.

[9] As Poisson regressions assume the dependent variable is equal to $e^{(x\beta)}$ (see footnote 7), 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.

[10] It should be noted that additional specifications were run using Poisson and OLS regression models with no significant effect on my results.

stated earlier) as the independent variable. 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. Additionally, all specifications of my models will include the natural logarithm of food supply rather than the linear metric itself, since I expect the positive effects of food security on political stability to diminish as people become less at risk of starvation and deprivation.

Finally, I use fixed effects for both country and year in my models. Doing so allows me 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 the 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 (Allison and Waterman 2002). To adjust for this potential problem, I specify additional models for all regressions that use simple unconditional country and year dummies rather than relying on the conditional estimates generated by a fixed effects model.

Fixed effects models can only control for country-specific elements that remain constant over time and for time-specific elements that are constant across states. Certain secondary elements from my conceptual model vary over time within a country and will need to be controlled. Most notably, I anticipate that the magnitude of a country's population will govern both the overall food supply in a country and the number of events observed. Therefore, I include a measure of population per country-year in all models.[11] Likewise, a country's wealth, measured as gross domestic product (GDP), is controlled for, so that the wealthy, stable, and well-fed countries in Europe and North America do not overly influence my findings.[12] As with food supply, the effect of GDP is hypothesized to diminish as GDP increases, suggesting that a log-transformed variable would be more appropriate to include in all models. Additionally, I control for inflation rate per country-year, since rising food prices could influence more generalized inflation or vice versa, leading to political unrest.[13] Finally, regime type, measured as either more or less democratic, could influence both food security and political stability and thus would need to be accounted for in any model.[14] All four of these factors will need to be included

[11] Population measured in thousands of people per country per year, via FAOStats (2012).
[12] 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.
[13] Inflation rates per country measured yearly based on consumer prices, also via the World Bank, http://databank.worldbank.org/ddp/home.do.
[14] 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.

as separate independent variables in all models specified.

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

$$Events_{it} = \beta_0 + \beta_1 ln(FoodSupply_{it}) + \beta_2 FoodQuality_{it} + \beta_3 Democracy_{it} + \beta_4 ln(GDP_{it}) + \beta_5 Inflation_{it} + \beta_6 Population_{it} + \alpha_t + \alpha_i + \eta_{it}$$

However, I test a number of alternate specifications as well, in order to control for the possibility that the findings are sensitive to certain assumptions implicit in the above model. In order to validate my model, I must first address the issue of reverse causality broached in my conceptual framework. I address this possibility using two different methods. In the first method, I 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 unnecessary assumption.

I also perform an additional series of regressions using instruments for food supply.[15] I 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 in that country. In order to be effective, an instrument must be highly correlated with the independent variable it intends to predict without being correlated with the dependent variable. Both tractor density and the abundance of arable land serve as strong predictors of food supply (t = -11.46 and 14.42, respectively, when regressed with all other independent variables). Likewise, their joint significance (F = 143.81) is well above the traditional cutoff (of F=10) for an effective joint instrument (Wooldridge 2009). Likewise, it is hard to fathom how either variable or their combination could be correlated with political instability except through their effect on food supply once one controls for factors like economic development. Thus, I conclude that these variables together represent worthwhile instruments to control for reverse causation in the model, though I admit, given the impossibility of proving a negative, that I can never know for sure.

In addition, since my dependent variable is a simple sum of discrete events, it gives equal weighting to all events without regard to their individual magnitude. Though this specification accords with that of Bellemare, Lagi et al., and Hendrix and Salehyan, I find it plausible that not only the frequency but also the intensity of events increases with a decline in food security. An aggregate of all individuals killed in a country in a given year from events of political unrest would approximate the seriousness of the observed events

[15] As per Hardin, Schmiediche & Carroll (2003), we performed this process 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.

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, I specify an alternate model that approximates individuals killed rather than events observed for a given country-year as the principal dependent variable.

I likewise could have incorrectly specified my measures of food security. In my primary model, I measured absolute levels of food supply and quality, in keeping with both the previous quantitative literature and US food security policy. However, my conceptual model highlighted that spikes in food prices or declines in food availability were speculated to deteriorate a regime's output legitimacy and thus precipitate political unrest. This framework suggests that year-on-year percentage changes in food security, rather than their absolute levels in a given year, could represent the true cause of my dependent variable. Indeed, previous literature in psychology and sociology suggest a link between relative, rather than absolute, deprivation and crime or other social ills (Walker and Mann 1987; Kawachia, Kennedy, and Wilkinson 1999; Bossert, D'Ambrosio, and Peragine 2007). I shall thus test whether year-on-year percentage changes in my two food security variables better predict events of political unrest than do their absolute levels.

VI. RESULTS

The key results for my primary model do not corroborate the literature that suggests a negative relationship between food security and events of political unrest. Instead, I find 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 1.3).[16] This positive correlation between food security and political unrest remains, albeit slightly attenuated, when only country-level effects are held constant ($p < 0.001$, see Table 1.1). To control for the potential that using two-way fixed effects biased my estimates when using a negative binomial regression, I also use simple dummies for country and year. However, the effect remains equivalently significant ($p < 0.001$, see Table 1.2). By contrast, the effect of food quality appears inconsistently significant.

To help comprehend the magnitude of the relationship between food supply and instability, I produce several projected outcomes in which I varied the key food supply variable while keeping all other regressors at their means. Using the country/year dummy model (Table 1.2), this coefficient suggests that an increase of food supply from the 25th percentile to the median would increase the predicted

[16] I also specified models using a variety of pooled regressions and those using fixed effects with OLS or Poisson regression models. As stated earlier, I do not think these models are appropriate for predicting event count data with the distribution my data have. However, the use of these models instead do not alter my findings significantly.

number of events in a given country fivefold, while going from the median to the 75th percentile would yield an increase of almost tenfold (holding all other variables at their means).[17] Keeping in mind that 70 percent of country-years observed no events and the majority of the rest observed only one, the relationship between food supply and events of insecurity appears substantively positive in both specifications, wholly contrary to expectations.

[17] Predicted events—25th percentile: 0.00390; 50th percentile: 0.0200; 75th percentile: 0.130.

As argued earlier, I have strong reason to believe that food security and political stability are intimately related and I suspect that the potential effect of political unrest on food security could skew my results. However, when I look instead at the effect of lagged food security metrics, the results appear virtually identical to those from comparable models using the given year's food data (see Table 2.1 and 2.2). I also use data on the number of tractors per 100 square kilometers of arable land and the percentage of total land that is arable to predict the overall food supply in a given country-year and then use those predicted

Table 1. Primary Negative Binomial Regression Models

Variables	(1) Country FE	(2) Country/Year Dummy	(3) Country/Year FE
ln(Food Supply)	0.327*	1.813*	0.440*
(kcal/day)	(4.61)	(4.92)	(5.9)
Food Quality	1.106	-0.713	1.828*
(% kcal fat/protein)	(1.72)	(-0.37)	(2.7)
Polity IV Score	-0.0208*	-0.0262*	0.009
(-10 to 10)	(-3.92)	(-2.98)	(1.66)
ln(GDP)	-0.058	-1.281*	-0.014
(1000s of 2005 Int$)	(-0.99)	(-4.48)	(-0.22)
Inflation	0.000206*	0.000125	0.000148*
(annual % change)	(5.13)	(1.46)	(3.72)
Population	-0.000000811*	0.00000203	-0.000000572*
(1000s of people)	(-3.48)	(1.13)	(-2.28)
Country Effects	✓	✓	✓
Year Effects		✓	✓
N	3177	3300	3177
chi2	104.8	2699.5	349.2
p-value	2.44E-20	0	1.89E-53

t-statistics in parentheses
* $p < 0.05$

Table 2. Regressions for Endogeneity			
	Lagged Models		Instrumented Model
	(1)	(2)	(3)
	C/Y Dummy	C/Y FE	C/Y Dummy
ln(Food Supply)	2.169*	0.467*	
(Lagged one year)	(6.04)	(6.29)	
Food Quality	0.241	2.048*	
(Lagged one year)	(0.13)	(3.06)	
Food Quality			0.618
(% kcal fat/protein)			(0.25)
Polity IV Score	-0.0282*	0.009	-0.021
(-10 to 10)	(-3.20)	(1.62)	(-1.86)
ln(GDP)	-1.525*	-0.036	-1.778*
(1000s of 2005 Int$)	(-5.30)	(-0.57)	(-3.48)
Inflation	0.000130	0.000149*	0.0000926*
(annual % change)	(1.52)	(3.72)	(2.13)
Population	0.00000206	-0.000000597*	-0.00000157
(1000s of people)	(1.15)	(-2.38)	(-0.80)
ln(Food Supply)			4.625*
(Predicted)			(3.24)
Country Effects	✓	✓	✓
Year Effects	✓	✓	✓
N	3296	3175	2009
chi2	2715.5	352.9	
p-value	0	3.48E-54	

t-statistics in parentheses
* $p < 0.05$

values in the original regressions (see Table 2.3).[18] Even here, the effect of the instrumented food supply on the predicted number of events of political instability remains positive and significant. The inclusion of the instrumented food supply measure does not seem to impact the effect of food quality on events of political stability, which remains insignificant.

As these results conflict with both my own conceptual model and previous quantitative studies, I want to ensure that my particular specifications do not overly influence the results. Therefore, I specify similar models that predict the total number of individuals killed, across all events of political unrest,

[18]As stated earlier, I actually performed both steps in one motion, via the "qvf" extension for Stata (Hardin, Schmiediche, and Carroll 2003). However, the math and the concept are easier to comprehend as two separate steps and I thus describe it as such.

in a given country-year (see Table 3.1 and 3.2). Despite this, the relationship between the overall food supply and the number of deaths remain positive and statistically significant ($p = 0.009$ and $p > 0.001$, see Table 3.1 and 3.2, respectively). Interestingly, though, I find the relationship between food quality and the predicted number of deaths from political unrest to be the reverse of the previous regressions, though still inconsistent.

I also test a different set of specifications for the two food security variables (see Tables 3.3-3.6). I do this to investigate whether my initial choice of a level variable, instead of a change variable to measure the effect of changes in food security, is appropriate. As individuals might use their past experience as a reference point to judge their well-being, it is not unreasonable to assume that one's relative food security would weigh more heavily than one's overall food security on one's self-assessed level of well-being. Indeed, when I measure the year-on-year percentage change in food supply instead of overall levels of food supply, I find this variable has a statistically significantly negative association with events of political unrest across both the factor dummy and fixed effects specifications ($p = 0.003$ and $p = 0.025$, see Tables 3.3 and 3.4, respectively). In the interest of comparing these disparate effects, I test one more set of models that include both the level and the change variables for both food supply and food quality (see Tables 3.5-3.6).[19] Little changes between these models and those previous. The effect of absolute levels of food supply remains equally positive and significant ($p < 0.001$ for both). Similarly, the effect of change in food supply remains equally negative and significant ($p < 0.001$ and $p = 0.012$, respectively). The effect of both the absolute levels of and changes in food quality still appears ambiguous and infrequently significant.

VII. DISCUSSION

My results suggest that increases in the overall food supply within a country are associated with increases in the predicted number of events of political instability. This contradicts both my initial hypothesis and previous studies.

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. A complication with this explanation stems from the bulk of food products relative to their price which makes them harder to smuggle, especially compared to more commonly exported commodities, like diamonds, metals, or oil. For instance, a previous study found no significant effect of the

[19] 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 variable and their results should therefore be interpreted with caution. I included them simply to note that the differential *ceteris paribus* effects of absolute and relative food security hold up even when controlling for each other.

Table 3. Regressions Using Alternate Specifications

	Models using Individuals Killed instead of Events		Models using Year-on-Year Percentage Change in Food Supply			
	(1)	(2)	(3)	(4)	(5)	(6)
	C/Y Dummy	C/Y FE	C/Y Dummy	C/Y FE	C/Y Dummy	C/Y FE
ln(Food Supply)	3.190*	0.354*			2.02*	0.454*
(kcal/day)	(2.62)	(4.32)			(5.48)	(6.06)
Food Quality	13.58*	-1.227			-0.367	1.86*
(% kcal fat/protein)	(2.33)	(-1.80)			(-0.18)	(2.74)
Polity IV Score	-0.007	0.0368*	-0.0267*	0.003	-0.027*	0.009
(-10 to 10)	(-0.23)	(5.43)	(-2.99)	(0.6)	(-3.07)	(-1.30)
ln(GDP)	-3.903*	0.025	-0.968*	0.291*	-1.47*	-0.028
(1000s of 2005 Int$)	(-4.33)	(0.39)	(-3.65)	(10.98)	(-5.10)	(-0.44)
Inflation	-0.000222	0.0000381	0.000133	0.000142*	0.000122	0.000148*
(annual % change)	(-1.11)	(0.46)	(1.54)	(3.59)	(1.46)	(3.65)
Population	0.00000780	-0.000000697*	0.00000175	0.00000155	0.00000189	-0.00000584*
(1000s of people)	(1.63)	(-2.71)	(0.95)	(0.75)	(1.05)	(-2.33)
Δ Food Supply			-3.08*	-1.894*	-3.73*	-2.12*
(annual % change)			(-3.00)	(-2.24)	(-3.65)	(-2.52)
Δ Food Quality			-1.38	-0.953	-1.39	-1.05
(annual % change)			(-1.34)	(-1.19)	(-1.31)	(-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
* $p < 0.05$

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 may provide another motivation: the resultant greater reward for holding reins of power may induce more frequent power struggles. 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.

My results can also be partially reconciled with the previous quantitative literature if one also considers the ceteris paribus effect of change in the food supply. My 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. It also corresponds with Hendrix and Salehyan's findings that increased volatility in rainfall precipitates riots. In doing so, however, this finding 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 seeks 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, I offer a few caveats for those researchers and policymakers hoping to build off my results. My data are not entirely pristine, as I removed a number of countries from the sample for entirely lacking food supply data, often because of their high levels of unrest, leading to the potential of selection bias in the results. Further, since I aggregate my main dependent variable from a collection of observed events, biases inherent in that dataset towards North America and Europe could also skew the results. In addition, I make no attempt to control for potential autocorrelation in the model as a result of my choice to specify a negative binomial regression instead of standard OLS. I think this tradeoff worthwhile and avoid making conclusions based on results of spurious significance ($0.05 < p < 0.1$) in part to compensate. Finally, though I attempt to control for the most obvious time-variant confounding variables, others inevitably exist and thus could bias any or all of my estimates. The similar potential also exists for my instruments to in truth fail the exclusion condition. Further studies using alternate datasets or additional variables might mitigate these issues and determine whether my results are still valid.

VIII. CONCLUSION

This paper's 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, the 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. It may even be the case that the negative second order effects may overwhelm the first order benefits, leading current US food security efforts to potentially cause more harm than good.

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 on boosting individual wealth directly by working with national governments to control hyperinflation instead of concentrating on increasing food security, as the results suggest the former two interventions are better aligned with both goals. However, given the importance of my research question on US food security policy, future research might seek to ensure that my results are indeed sound before any substantial adjustments to development policy are made.

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Preliminary Effects of the School Improvement Grant Program on Student Achievement in Texas

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Abstract

This paper uses school-level data from the state of Texas to test whether receiving a School Improvement Grant (SIG) has led to higher graduation, completion, or dropout rates and/or increased student achievement, as measured by standardized tests in reading, math, science, social studies, and writing. Part of this analysis tests to see if the effects of the program vary for urban versus rural schools, charter versus non-charter schools, or between demographic subgroups. This paper's results suggest that in its first year of implementation, SIG had a negative effect on student achievement at elementary and middle schools across almost all subjects and subgroups and had little effect on achievement at high schools, although the program does appear to have had a positive effect on graduation rates. These results also suggest that rural schools saw fewer benefits from the program than urban schools, while the effects for charter schools were similar to the effects for traditional public schools.

I. INTRODUCTION

Since the release of *A Nation at Risk* almost thirty years ago, the conspicuous failure of successive waves of school interventions has highlighted the inherent difficulty of raising student achievement at so-called "failing" schools. The School Improvement Grant Program (SIG), promoted by the Obama Administration as the antidote to this chronic underachievement, is the most recent—and arguably the most ambitious—attempt to administer shock therapy to these schools. Since the 2010–2011 school year, SIG has directed over $3.5 billion to more than 1,300 "persistently lowest-performing" schools across the country, in exchange for the adoption of one of four approved models of school improvement: Transformation, Turnaround, Restart, and Closure.[1] Department of Education guidelines allow states to award districts up to $2 million annually to each qualified SIG school. However, in practice, the funding schools have received through SIG has varied, as has the impact of SIG funds on per pupil spending.

Since 2012, the Obama Administration has claimed that SIG is producing "double-digit increases" in the percentage of students deemed proficient in math and English language arts (ELA). However, this claim is based on a correlation and considers only a narrow slice of school performance, i.e. "proficiency," so the true effects of the program remain poorly understood. To date, only one study has rigorously examined the effects of SIG on student achievement. Using school-level data from California, Dee (2012) estimates that SIG had significant positive effects on a school's Academic Performance Index, or API score—a composite measure of school performance calculated annually by the California Department of Education—in its first year of implementation, with the bulk of the gains concentrated in schools that chose to implement the Turnaround model of improvement.

Unfortunately, decades of research on school improvement suggest that the success of even a well-designed intervention is highly dependent on the context in which it is implemented. Thus, since SIG grants have been awarded in all 50 states and the District of Columbia, it is far from certain whether Dee's results are representative of the program as a whole. This is particularly true given that what we do know about SIG suggests that its implementation varies considerably by state. For example, in some states grants were awarded through a competitive process, while in others nearly every eligible school received a grant. Similarly, while in some states schools chose to implement several of the approved improvement models, in

[1] The requirements of these models are defined by federal regulation: Transformation requires that a school incorporate student achievement into teacher evaluations and that the principal be replaced. Turnaround is similar to Transformation, but includes the additional requirement that at least 50 percent of the teaching staff be replaced. Restart requires that a school be closed and reopened as a charter school. Finally, Closure provides minimal funding to assist with the permanent closure of a school.

others virtually all schools chose the Transformation model, which most observers consider to be the least intrusive of the four options (Hurlburt et al. 2011).

This variability in implementation, coupled with the inherent differences that exist between schools and communities, provides ample grounds for interpreting Dee's results narrowly. Accordingly, the primary purpose of this paper is to build upon Dee's work in California by examining the effects of the SIG program in a different context. To that end, I use school-level assessment and graduation data from Texas to test whether receiving a School Improvement Grant leads to higher graduation, completion, or dropout rates and/or increased student achievement, as measured by standardized tests in reading, math, science, social studies, and writing. Further tests are performed to assess the effects of the program for urban versus rural schools and charters versus non-charters. The results of my analysis suggest that in its first year of implementation, SIG has a negative effect on student achievement at elementary and middle schools across almost all subjects and subgroups, and little effect on achievement at high schools, although the program does appear to have a positive effect on graduation rates. Rural schools appear to receive fewer benefits from the program than urban schools, while the effects for charter schools are similar to the effects for traditional public schools—an important result, given the relatively poor performance of these charters.

II. DATA

The data for this paper are drawn from the Common Core of Data and the Texas Department of Education, which provides access to the Texas Assessment of Knowledge and Skills (TAKS) for every campus in the state that reports such data. Prior to the 2011–2012 school year (when Texas switched tests), TAKS data exist for up to five core subjects depending on the grade level, including reading (grades 3–11), math (3–11), science (5, 8, 10, and 11), social studies (8, 10, and 11), and writing (4 and 7).[2] In order to provide a robust evaluation of the impact of SIG, this study uses data for all five subjects, all three proficiency measures, and nine possible subgroups: males, females, Caucasians, African Americans, Hispanic students, economically disadvantaged, at-risk, special education students, and English Language Learners.

The analytical sample for this study consists of all primary and secondary schools in Texas that reported graduation, completion, or dropout rates, and/or TAKS reading, math, science, social studies, or writing assessment data, between the 2007–2008 and 2010–2011

[2] There are no data for the 12th grade because there is no separate test for this grade. Texas students cannot graduate from high schools unless they pass "exit-level" TAKS tests in reading, social studies, math, and science. Consequently, during their junior and senior years of high school, students are given five chances to pass these tests.

school years—approximately 7,800 schools. From these schools, the Texas Department of Education identified those "persistently lowest-achieving" schools that were eligible for SIG funding, as required by federal regulation. More specifically, from a pool of approximately 3,500 Title I-eligible schools in improvement, corrective action, or restructuring, the Department identified approximately 180 schools (roughly five percent) as "persistently lowest-achieving" in Cycle I of SIG. This pool included "Tier I" schools (drawn from the population of schools that received Title I funding), "Tier II" schools (drawn from the population of schools that were eligible for, but did not receive, Title I funds), and a number of "Tier III" schools (other low-performing schools not eligible for Title I funding, but eligible for SIG funding) which received lower priority than Tier I and Tier II schools.

The Tier I and Tier II schools identified as SIG-eligible included schools deemed "persistently lowest achieving" based on schools' average math and reading test scores and their lack of progress in these subjects over the previous two years.[3] Also deemed SIG-eligible were any high schools with graduation rates below 60 percent. Of the Tier I and Tier II schools identified by these criteria, 48 received a SIG award in Cycle I, as did 17 Tier III schools, meaning that graduation and assessment data from these schools in 2010-2011 reflect the impact of the program in its first year of implementation. Of these 65 schools, 53 were high schools and 20 were charter schools. Finally, 63 of the schools receiving grants chose to implement the Transformation model of school improvement, while two schools chose the Turnaround model.

[3] Due to the ambiguous language in the regulations governing SIG, different states developed different definitions of "lack of progress" to identify eligible schools. According to Hurlburt et al. (2011), eleven states used a student-level growth measure to determine whether a school had made progress, while the remaining 39 states (including Texas) focused on school-level improvement over time.

III. METHODOLOGY

The goal of this paper is to estimate the impact of SIG on student achievement during its first year of implementation (2010–2011). However, because there are year-to-year changes in a school's performance that were not attributable to the impact of SIG, simply comparing the test scores and graduation rates of SIG schools before and after implementation will not provide reliable estimates of the program's effect. Econometric techniques can control for the effects of confounding variables such as race, gender, and socioeconomic status, as well as any broader trends in graduation and student achievement that may affect all Texas schools, regardless of their SIG status. In his study of the effects of SIG on student achievement in California, Dee (2012) addresses these issues by using a regression discontinuity model to estimate the effect of SIG eligibility on school performance at various eligibility thresholds. However, in this paper I rely upon the following difference-in-differences model:

$$Achievement_{ct} = \beta_0 + \beta_1 SIG + \beta_2 post + \beta_3 post \times SIG + \beta_4 X + \varepsilon$$

In the above equation, *Achievement* is the academic outcome of interest at campus *c* in year *t*, which may be either the graduation, completion, or dropout rate; the average test score in a given grade and subject; or the percentage of students who met the statewide proficiency or commended standard for a given grade and subject.[4] *SIG* is a dummy variable indicating whether or not a school is part of the first SIG cohort, and *post* is a dummy variable that is equal to "0" for the time period prior to implementation and "1" for the year in which SIG was implemented. The coefficient on the *post×SIG* variable represents the estimated increase or decrease in a given achievement measure that is expected at SIG schools, once the expected differences between SIG and non-SIG schools, captured by *SIG* and the statewide trend in proficiency rates captured by *post*, are taken into account.

The full model includes grade-level controls for race, gender, economically disadvantaged status, special education status, English Language Learner (ELL) status, as well as the proportion of students who meet the Texas definition of at-risk youth. For each grade and subject included in the analysis, these controls were generated by dividing the number of tested students in that subgroup by the total number of tested students to find the proportion of tested students in that subgroup for that grade and subject. Additionally, the full version of the model includes both school and year fixed effects, which effectively control for any unobservable campus- or time-invariant characteristics.

A "Title I" dummy variable was constructed by combining the six categories of Title I eligibility from the Common Core of Data into two categories, which was then used to restrict the sample to test the robustness of the results. Similarly, Common Core charter status was reduced to two categories, and the twelve location codes used in the Common Core of Data were combined into two (urban and rural). The rural and charter dummies were then used to restrict the sample so the effects of SIG on urban versus rural schools and charters versus non-charters could be estimated separately.

For each regression, the data are weighted to reflect the number of students represented by a given school for a given performance measure. Thus, for the specifications used to estimate the effect of SIG on graduation, completion, and dropout rates, the data are weighted by school enrollment. For the specifications used to estimate the effect of SIG on reading, math, science, social studies, and writing achievement, the data are weighted by the number of students taking the test in a given grade and subject. Similarly, for each regression performed on a grade-subject pair the standard errors are clustered by campus to account for the possibility that the errors might be

[4] For the purposes of this paper, a "campus" is distinct from a "school," which may include multiple campuses.

correlated within campuses for that grade and subject.[5]

Finally, because the scoring scale for several tests changed between 2008–2009 and 2009–2010, average test scores in all grades and subjects were normalized by subtracting the statewide mean for individuals for a given grade and subject in a given year and dividing the remaining quantity by the standard deviation for that grade, subject, and year. Consequently, while the units for the estimates of SIG's impact on graduation and proficiency rates are percentage points, all estimates for average test scores presented in this paper are expressed in standard deviations.

IV. RESULTS

Table 1 presents summary statistics for the Texas school system and the subpopulation of schools that received SIG awards in 2010–2011. As can be seen from this table, the SIG cohort differs from the broader population of Texas schools in several important ways. Compared to the broader population of schools, SIG schools have lower test scores,[6] lower graduation and completion rates, higher dropout rates, and higher percentages of African American and Hispanic students, as well as more economically disadvantaged, at-risk, special education students, and English Language Learners. Additionally, SIG schools are more urban, more likely to be eligible for Title I funds, and far more likely to be charter and/or high schools than the broader population of schools, meaning that the results for students in grades 9, 10, and 11 are the most important for evaluating the program's overall impact.

For each grade and subject in which a TAKS test was administered, various specifications of the difference-in-differences model were used to test the robustness of the resulting estimates, as illustrated in Table 2, which shows the estimated coefficients for average 10th grade math scores for the "all students" group. In this table, column 1 shows the results for the basic difference-in-differences model without controls; column 2 shows the results including a range of demographic controls; column 3 shows the results for the full model with school and year fixed effects; and column 4 shows the results for the full model when the sample is restricted to schools that were eligible for school-wide Title I funding in 2010–2011. This population of schools bears a greater resemblance to the SIG cohort than the Texas school system as a whole, making it useful for confirming the results from the full sample.

In the most basic version of the model (column 1) the estimated coefficient on SIG is negative and highly significant, suggesting that (prior to receiving an award) SIG schools performed approximately a

[5] An important limitation of this paper arises from the fact that while this method of clustering allows for non-independence within campuses for each grade-subject pair, it does not allow for non-independence within campuses *across* grades and subjects.

[6] Across all tested grades, SIG students scored between .2 and .5 standard deviations below the average Texas student in math, reading, science, social studies, and writing.

Table 1. Summary Statistics		
Variable	**Texas**	**SIG**
10th Grade Math	0.000	-0.3119
	(0.3788)	(0.3428)
Graduation Rate	87.61	71.75
	(12.01)	(19.76)
Dropout	7.23	18.3
	(8.02)	(13.57)
Completion I	92.77	83.06
	(8.23)	(14.34)
Completion II	93.69	84.78
	(7.35)	(12.78)
Post X SIG	0.0021	0.25
SIG	0.0085	1
Post	0.255	0.25
Percent of tested students who are female	49.51	49.75
	(3.66)	(5.85)
Percent of tested students who are Caucasian	35.89	10.35
	(28.52)	(16.72)
Percent of tested students who are African American	13.91	16.97
	(16.82)	(23.14)
Percent of tested students who are Hispanic	45.68	71.46
	(30.15)	(27.67)
Percent of tested students who are Economically Disadvantaged	53.42	80.68
	(27.13)	(18.32)
Percent of tested students who are "at risk"	41.44	64.61
	(18.28)	(15.97)
Percent of tested students who are Special Ed	6.1	7.71
	(3.28)	(4.86)
Percent of tested students who are Limited English Proficient	9.47	11.05
	(11.84)	(9.78)
High School	0.2427	0.8654
Charter	0.063	0.3077
Rural	0.442	0.2308
Title I eligible	0.7832	0.93
N	7,779	260

Notes: This table shows weighted averages for the Texas school system and the SIG cohort. Standard deviations are in parentheses.

Table 2. Effect of SIG Treatment on 10th Grade Math Scores				
Variable	**(1)**	**(2)**	**(3)**	**(4)**
postXSIG	0.036	0.0274	0.0336	0.0309
	(0.0313)	(0.0321)	(0.0331)	(0.0351)
SIG	-0.3294**	0.0184		
	(0.0606)	(0.0298)		
post	-0.0019	-0.0293**		
	(0.0050)	(0.0056)		
Percent Female		0.0027**	0.0001	0.0001
		(0.0009)	(0.0005)	(0.0006)
Percent Black		-0.0022**	-0.0075**	-0.0067**
		(0.0004)	(0.0009)	(0.0011)
Percent Hispanic		0.0011**	-0.004**	-0.0036**
		(0.0003)	(0.0006)	(0.0007)
Percent Economically Disadvantaged		-0.0037**	-0.0007	-0.0004
		(0.0005)	(0.0004)	(0.0005)
Percent At Risk		-0.0125**	-0.0041**	-0.0039**
		(0.0005)	(0.0004)	(0.0004)
Percent Special Education		-0.0169**	-0.0085**	-0.0077**
		(0.0012)	(0.0008)	(0.0010)
Percent Limited English Proficient		0.004**	-0.0076**	-0.0078**
		(0.0010)	(-0.0013)	(0.0015)
Title I Eligible		-0.0538**	-0.194**	
		(0.0122)	(0.0080)	
School Fixed Effects	No	No	Yes	Yes
Year Fixed Effects	No	No	Yes	Yes
N	6573	6573	6573	4929

Notes: This table shows estimated coefficients for average 10th grade math scores under various specifications. Column 1 shows estimates from the basic model, with no controls included. Column 2 shows estimates including various demographic controls. Column 3 shows estimates including school and year fixed effects. Column 4 limits the sample to schools that are eligible for school-wide Title I programs. All estimates have been normalized and are expressed in standard deviations. Regressions are weighted by the number of students taking the exam at a school. Standard errors that allow for clustering at the school level are in parentheses. A single asterisk denotes significance at the 5% level. A double asterisk denotes significance at the 1% level.

third of a standard deviation worse than non-SIG schools in 10th grade math, as one might expect given that the program is intended to target only the most "persistently lowest-achieving" schools in the state. In more developed versions of the model (columns 2, 3, and 4) the coefficients on the various demographic controls suggest that schools with a greater percentage of male, African American, Hispanic, economically disadvantaged, at-risk, special education, and ELL students performed less well as a result of these differences. Indeed, as the positive coefficient on SIG in column 2 demonstrates, in the case of 10th grade math, the difference between the performance of SIG and non-SIG schools prior to the implementation of the program is entirely explained by these demographic factors—an important result, since it calls into question one of the underlying assumptions of SIG, that the poor academic performance of grant recipients is at least partly explained by the quality of the education they provide. While this result does not hold for every grade and subject, on average there appears to be little difference between the performance of SIG and non-SIG schools, once demographic factors are taken into account.

Most important for the purposes of this analysis, the estimate on *post×SIG*, despite being relatively stable across all specifications of the model, is not significant in any of them, suggesting that SIG did not have a significant effect on 10th grade math scores. Importantly, there are almost no significant differences between the results for the full sample and those for Title I schools for any grade or subject. Consequently, from this point forward all results presented or discussed are generated using the full sample, unless otherwise indicated. Similarly, from this point forward all results presented are generated using the full version of the model, including demographic controls and school and year fixed effects.

Results for the "All Students" Group

Grade-by-grade estimates of the coefficient on *post×SIG* for reading, math, science, social studies, and writing achievement for the "All Students" group are presented in Tables 3 and 4. For both tables, the coefficients in the "average score" columns represent the expected increase or decrease in the average score for a given subject and grade as a consequence of SIG, expressed in standard deviations. The coefficients in the "percent proficient" and "percent commended" columns represent the expected increase or decrease in the percentage of students who are proficient or commended on a 0–100 scale.

The results for the "All Students" group suggest that the overall impact of SIG on average test scores across grades is mixed, and in many cases the estimated coefficient on *post×SIG* for average test scores is negative and statistically significant. For example, the estimates for average reading scores are negative and statistically significant for grades 4 through 7, with effect sizes approaching one fifth of a standard deviation. Similarly, the estimates for 6th and 8th grade math, 5th and 8th grade science, and 7th grade writing suggest that SIG has had a negative impact on these grades and subjects. Indeed, for no subject in the elementary (3–5) or middle (6–8) grades is the estimated effect on average test scores positive and significant, although the estimates for 3rd grade reading and math are

Table 3. Effects of SIG Treatment on Reading and Math Achievement						
Grade	Reading			Math		
	Average Score	Percent Proficient	Percent Commended	Average Score	Percent Proficient	Percent Commended
3	0.017	1.14	-1.05	0.153	8.81*	3.49
	(0.128)	(2.79)	(7.06)	(0.164)	(3.75)	(6.93)
4	-0.091**	-0.86	-5.63**	-0.009	-1.69	0.97
	(0.034)	(3.13)	(1.58)	(0.041)	(4.14)	(0.84)
5	-0.159**	-1.91*	-7.62	-0.11	-3.9*	-5.63
	(0.023)	(0.94)	(3.05)	(0.078)	(1.70)	(3.57)
6	-0.192**	-5.48**	-9.11**	-0.095**	-3.21*	-2.29*
	(0.037)	(0.88)	(0.88)	(0.011)	(1.33)	(0.95)
7	-0.177**	-3.52**	-3.88**	-0.08	-5.98	0.35
	(0.027)	(1.11)	(0.95)	(0.069)	(4.29)	(0.98)
8	-0.086	-2.05	-3.24	-0.186**	-6.42	-6.35**
	(0.051)	(1.61)	(1.86)	(0.049)	(3.49)	(1.29)
9	-0.013	0.14	-1.51	0.07*	4.33**	1.25
	(0.024)	(0.73)	(0.87)	(0.033)	(1.65)	(1.10)
10	0.047*	2.06*	1.7*	0.034	2.42	0.65
	(0.022)	(0.91)	(0.81)	(0.033)	(1.51)	(0.80)
11	0.009	2.67**	1.34	0.033	3.87**	0.67
	(0.027)	(0.83)	(1.07)	(0.031)	(1.39)	(1.00)

Notes: This table shows estimates of the effects of the SIG treatment on Reading and Math achievement for the full sample, including the controls from column 3 of Table 2 and school and year fixed effects. Estimates of effects on average scores have been normalized and are expressed as standard deviations. For all other estimates the units are percentage points. Regressions are weighted by the number of students taking the exam at a school. Standard errors that allow for clustering at the school level are in parentheses. A single asterisk denotes significance at the 5% level. A double asterisk denotes significance at the 1% level.

sufficiently large that we cannot rule out a positive effect.

Fortunately, these troubling results do not carry over to the high-school grades (9–11), which, as previously mentioned, are more consequential, since they represent a greater population of schools and students. In general, the estimates for average test scores for these grades suggest that SIG did not have a big effect on high school achievement, although the positive signs on most of these coefficients suggest that any effect was most likely positive.[7] In two cases (10th

[7] Because these results are based on many separate regressions, interpreting the standard errors is problematic without correcting for the number of hypotheses tested, and since there are a total of 27 grade-subject pairs for the All Students group, it could be argued that there are a total of 27 separate hypotheses. Applying a Bonferroni adjustment to the p-values for these hypotheses yields adjusted p-values of .00185 at the 5% significance level and .00037 at the 1% significance level. At these values, many of the estimated effects on average scores for the All

grade reading and 9th grade math), the estimated coefficient is positive and statistically significant; however, in the case of 10th grade reading, the estimate slips below the threshold for significance when the sample is restricted to Title I schools—the only instance in which this restriction makes a significant difference.

Across all grades and subjects, there is suggestive evidence that the effects of SIG were concentrated at the threshold for proficiency, meaning there was a greater increase in the proficiency rate than might be expected, given the increase in test scores. For example, the estimated effect for 3rd grade math proficiency is 8.81 percentage points, despite the fact that the coefficient on average scores is insignificant. Similarly, for 11th grade reading, math, and social studies, and 10th and 11th grade science, the estimated coefficient on proficiency is positive and significant, despite the fact that the coefficient on average scores is not.

One possible explanation for this pattern is that SIG grantees placed additional emphasis on proficiency by focusing on students who are just below the proficiency threshold (so called "bubble students"), or through other means. However, without access to student-level data it is difficult to know for sure. Similarly, it is difficult to evaluate the estimates for the number of students who were "commended," but these results should be interpreted with caution, since the number of students represented by these estimates is small.

Effects of Urbanicity and Charter Status

Table 5 shows the estimated coefficients on *post*×*SIG* for urban versus rural schools for the "all students" group, which were generated by restricting the sample to each of these subpopulations of schools, using the full model. As can be seen from the table, the results for urban schools appear to be marginally more positive than the results for rural schools. In particular, although only the estimate for 10th grade reading is significant at conventional levels, in virtually every other grade and subject at the high school level, the sign of the coefficients is positive and the estimates are approaching the threshold for significance. Moreover, in several cases the magnitude of the estimated effect is relatively large, approaching a tenth of a standard deviation.

In contrast, nearly all of the estimates for rural schools are negative, and for several grades and subjects (such as 3rd and 9th grade reading, 3rd and 4th grade math, and 10th grade social studies) the results appear to be worse for rural schools than they are for urban schools, although these differences are not necessarily significant. Similarly, the generally negative signs on the estimated coefficients for rural high schools suggest that these schools did

Students group, such as 10th grade reading and 11th grade math, fall below the threshold for significance at the 5% level. However, considering the large number of hypotheses involved, it is likely that this approach to adjusting inference is too conservative. Consequently, standard errors for all regressions are presented without adjustment, with the understanding that the risk of false-positives is significant, if difficult to estimate.

Table 4. Effects of SIG Treatment on Science, Social Studies, and Writing Achievement

Grade	Science			Social Studies			Writing		
	Raw Score	Proficient	Commended	Raw Score	Proficient	Commended	Raw Score	Proficient	Commended
3									
4							-0.011	-1.91	3.39
							(0.047)	(1.40)	(2.97)
5	-0.173*	-5.5	-9.23**						
	(0.077)	(3.64)	(3.02)						
6									
7							-0.066**	-0.88	-4.03
							(0.022)	(0.77)	(3.08)
8	0.184**	6.2*	5.15*	0.039	2.52	0.73			
	(0.040)	(3.03)	(2.49)	(0.046)	(2.22)	(1.54)			
9									
10	0.05	3.23*	-0.54	0.032	1.64	1.42			
	(0.029)	(1.35)	(0.72)	(0.031)	(1.09)	(1.10)			
11	0.023	3.64**	-2.87**	0.01	1.97**	-0.15			
	(0.029)	(1.17)	(1.05)	(0.035)	(0.64)	(1.39)			

Notes: This table shows estimates of the effects of the SIG treatment on science, social studies, and writing achievement for grades 3–11, including all of the controls from column 3 of Table 2. Estimates for average scores have been normalized and are expressed as standard deviations. For all other estimates the units are percentage points. Regressions are weighted by the number of students taking the exam at a school. Standard errors that allow for clustering at the school level are in brackets. A single asterisk denotes significance at the 5% level. A double asterisk denotes significance at the 1% level.

Table 5. Effects of SIG Treatment at Urban vs. Rural Schools

Grade	Rural					Urban				
	Reading	Math	Science	Social Studies	Writing	Reading	Math	Science	Social Studies	Writing
3	-0.58**	-0.269**				0.05	0.177			
	(0.014)	(0.015)				(0.124)	(0.169)			
4	-0.254**	-0.083**			-0.441**	-0.08*	-0.01			0.009
	(0.012)	(0.016)			(0.014)	(0.035)	(0.042)			(0.045)
5	-0.121**	0.246**	0.045*			-0.156**	-0.12	-0.172*		
	(0.016)	(0.019)	(0.022)			(0.026)	(0.082)	(0.085)		
6	-0.18	-0.138**				-0.197**	-0.09**			
	(0.173)	(0.017)				(0.037)	(0.013)			
7	0.091	0.15			-0.032	-0.168**	-0.083			-0.066**
	(0.132)	(0.234)			(0.074)	(0.022)	(0.077)			(0.018)
8	-2.01*	-0.022	-0.116	-0.023		-0.077	-0.19**	-0.186**	-0.038	
	(0.079)	(0.057)	(0.112)	(0.043)		(0.054)	(0.048)	(0.043)	(0.051)	
9	-0.059*	0.031				-0.001	0.073			
	(0.028)	(0.033)				(0.029)	(0.039)			
10	-0.055	-0.022	0.006	-0.101*		0.068**	0.043	0.055	0.055	
	(0.050)	(0.065)	(0.065)	(0.051)		(0.021)	(0.038)	(0.033)	(0.035)	
11	-0.035	0.005	-0.006	-0.044		0.03	0.035	0.019	0.021	
	(0.051)	(0.041)	(0.062)	(0.046)		(0.031)	(0.037)	(0.033)	(0.042)	

Notes: This table shows estimates of the effects of the SIG treatment on achievement at rural vs. urban schools, including all of the controls from column 3 of Table 2 and school and year fixed effects. All estimates have been normalized and are expressed as standard deviations. Regressions are weighted by the number of students taking the exam at a school. Standard errors that allow for clustering at the school level are in brackets. A single asterisk denotes significance at the 5% level. A double asterisk denotes significance at the 1% level.

not benefit from the program as the urban high schools did—a plausible result, given the criticisms that have been leveled at the program by rural policymakers.[8]

Table 6 shows the estimated coefficients on *post*×*SIG* for charters versus non-charters for the "all students" group, which were generated by restricting the sample to each of these subpopulations of schools. As can be seen from this table, the relative performance of charters versus non-charters varies by grade. For example, the results for charters appear to be worse than the results for non-charters in 3rd and 5th grade math, but better in 4th grade writing and 6th and 8th grade math, although many of these differences are not significant at conventional levels.

Graduation, Completion, and Dropout Rates

Table 7 presents estimates of the effect of SIG on graduation rates for the full sample, as well as for urban versus rural schools, charters versus non-charters, and the various demographic subgroups. As can be seen from this table, the results for graduation are more encouraging than any of the results discussed so far. In particular, the estimate for the "all students" group suggests that SIG raised overall graduation rates by approximately five percentage points. This result appears to be driven by even greater improvements for African Americans, at-risk students, and special education students. Perhaps the most striking result for graduation is the estimate for African American charter school students, which suggests an increase of nearly 20 percentage points as a result of SIG. Notwithstanding this result, in general the differences between charters and non-charters appear to be modest, as do the differences between urban and rural schools, although the standard errors on many of these estimates are large enough that there may be differences between these groups that are not reflected in such a small sample.

In addition to the results for graduation, Table 8 presents estimates for the effect of the SIG treatment on dropout rates, as well as both of the completion rates tracked by the Texas Education Agency (TEA). According to the TEA, the numerator for completion I consists of students who have graduated or continued in high school, while the numerator for completion II consists of students who have graduated, continued in high school, or received General Education Development (GED) certificates. Interestingly, the estimated effect of SIG on graduation is larger than the estimated effect on completion I, suggesting that some of the increase in graduation rates attributable to SIG may have resulted from the conversion of continuing students (rather than dropouts) into graduates. Similarly, the estimated effect of SIG on completion I is larger than the estimated effect

[8] In a recent survey of state and school-level officials, for example, Scott et al. (2012) found that several SIG requirements, such as the criteria for identifying and funding schools, and the staff replacement requirements of the improvement models, were considered inappropriate for rural schools.

Table 6. Effects of SIG Treatment at Charter vs. Non-Charter Schools

Grade	Charters					Non-Charters				
	Reading	Math	Science	Social Studies	Writing	Reading	Math	Science	Social Studies	Writing
3	-0.186	-0.509*				0.041	0.212			
	(0.131)	(0.229)				(0.144)	(0.162)			
4	-0.173*	0.163			0.466**	-0.105**	-0.013			-0.044
	(0.088)	(0.112)			(0.107)	(0.033)	(0.044)			(0.032)
5	-0.057	-0.217*	0.021			-0.159**	-0.091	-0.174*		
	(0.073)	(0.097)	(0.096)			(0.025)	(0.079)	(0.081)		
6	-0.456**	-0.135				-0.162**	-0.091**			
	(0.110)	(0.114)				(0.016)	(0.010)			
7	-0.204*	-0.137			-0.114	-0.169**	-0.072			-0.062**
	(0.111)	(0.176)			(0.083)	(0.026)	(0.075)			(0.022)
8	0.019	0.131	-0.022	0.079		-0.102	-0.231**	-0.21**	-0.061*	
	(0.068)	(0.069)	(0.103)	(0.187)		(0.059)	(0.012)	(0.027)	(0.025)	
9	-0.09	0.053				-0.01	0.068			
	(0.075)	(0.044)				(0.025)	(0.035)			
10	0.022	0.033	-0.029	0.002		0.046	0.032	0.052	0.031	
	(0.063)	(0.091)	(0.076)	(0.061)		(0.028)	(0.035)	(0.030)	(0.032)	
11	0.069	0.053	0.064	0.113		0.004	0.031	0.021	0.004	
	(0.059)	(0.054)	(0.053)	(0.070)		(0.028)	(0.032)	(0.030)	(0.036)	

Notes: This table shows estimates of the effects of the SIG treatment on average scores at charters versus non-charters, including all the controls from column 3 of Table 2 and school and year fixed effects. All estimates have been normalized and are expressed as standard deviations. Regressions are weighted by the number of students taking the exam at a school. Standard errors that allow for clustering at the school level are in brackets. A single asterisk denotes significance at the 5% level. A double asterisk denotes significance at the 1% level.

Table 7. Effect of SIG Treatment on Graduation Rates					
Subgroup	All Schools	Urban	Rural	Charters	Non-Charters
All Students	5.17**	4.65*	5.49	6.82	4.79**
	(1.63)	(1.86)	(3.41)	(6.85)	(1.59)
Male	4.95**	4.72*	3.57	5.28	4.78**
	(1.87)	(2.14)	(4.17)	(6.74)	(1.92)
Female	5.18**	4.32*	7.23*	7.68	4.63**
	(1.75)	(1.99)	(3.51)	(8.25)	(1.62)
Caucasian	3.61	5.99*	-0.07	-1.35	3.69
	(2.71)	(2.47)	(2.29)	(11.68)	(2.73)
African American	8.53**	7.93*	6.09	19.87	7.92*
	(3.76)	(3.75)	(9.40)	(8.34)*	(3.72)
Hispanic	2.6	1.71	5.24	6.32	2.15
	(1.70)	(1.85)	(3.52)	(7.57)	(1.63)
Economically Disadvantaged	4.12*	3.43	6.13*	6.02	3.76*
	(1.62)	(1.82)	(2.73)	(8.07)	(1.54)
At Risk	7.39**	8.05**	2.17	3.45	7.35**
	(2.35)	(2.47)	(6.02)	(8.45)	(2.43)
Special Ed	7.98**	6.49	9.67	4.88	7.95*
	(3.07)	(3.32)	(8.65)	(11.70)	(3.22)
ELL	0.06	-0.27	-4.35	-13.05	0.8
	(4.93)	(5.30)	(8.98)	(22.08)	(5.01)

Notes: This table shows estimates of the effect of the SIG treatment on graduation rates, including demographic controls and school and year fixed effects. Regressions are weighted by the number of students at a school. Standard errors that allow for clustering at the school level are in brackets. A single asterisk denotes significance at the 5% level. A double asterisk denotes significance at the 1% level.

on completion II, suggesting that as a result of SIG, fewer students opted for a GED. Additionally, there is some evidence that the effects of SIG on completion are more modest for rural schools than for urban schools, although the magnitude of the standard errors means we cannot rule out the possibility that there is no difference between the two groups.[9]

V. CONCLUSION

The results of this analysis suggest that in its first year of implementation, SIG had a negative effect on student achievement at elementary and middle schools and little effect on high school achievement, although it does appear to have had a positive effect on graduation rates. These results also suggest that rural schools saw fewer

[9] With no data on completion or dropout rates for the demographic subgroups, it was not possible to estimate the effect of SIG on dropout or completion rates for these groups.

Table 8. Effect of SIG Treatment on Dropout and Completion Rates					
Variable	All Schools	Urban	Rural	Charters	Non-Charters
Dropout	-4.65**	-3.88**	-6.67	-4.93	-4.49**
	(1.35)	(1.09)	(5.20)	(2.54)	(1.43)
Completion 1	2.87*	3.45*	-0.49	5.77	2.55
	(1.43)	(1.60)	(2.75)	(4.09)	(1.46)
Completion 2	2.58	3.01	-0.12	3.63	2.41
	(1.37)	(1.54)	(2.82)	(2.58)	(1.44)

Notes: This table shows estimates of the effect of the SIG treatment on dropout and completion rates, including demographic controls and school and year fixed effects. Regressions are weighted by the number of students at a school. Standard errors that allow for clustering at the school level are in brackets. A single asterisk denotes significance at the 5% level. A double asterisk denotes significance at the 1% level.

benefits from the program than urban schools, while the effects for charter schools were similar to the effects for traditional public schools.

We should be cautious in interpreting these results, for a number of reasons. First, because this analysis was limited to the first year of implementation, these findings must be considered preliminary and subject to revision. Second, because this analysis was limited to school-level data, it was not possible to control for the effects of attrition, which may be significant given the number of youth attending SIG schools who are marginally attached to the education system. Third, because of limitations in the Common Core finance data, it was not possible to fully control for the effects of school spending, which could bias the results. Finally, because schools eligible for SIG are likely to have performed badly in the year prior to receiving the grant, it is possible that any increase in test scores that occurred after the program was implemented reflected a regression to the mean and not the impact of the program itself.[10]

Despite these limitations, the results of this analysis as presented are plausible, if somewhat discouraging, given the scale of federal investment. It should not be surprising to see mixed results, given the generally pessimistic tone of the literature on school improvement. Given what we know about the effects of principal tenure on student achievement, it seems likely that the leadership transitions that occurred at SIG schools in the first year of implementation negatively impacted academic outcomes, meaning that data from subsequent years may paint a more accurate, and potentially favorable, picture of the program's direct impact.[11] In the case of rural

[10] This is one possible explanation for the relatively positive results for third grade, which would probably be the first to reflect the enrollment of a more academically capable group of students.

[11] Béteille et al. (2012) found that Miami schools with first-year principals had lower achievement gains than other schools. Similarly, Miller (2009) found that schools in North Carolina experienced a decline in student achievement

schools, these leadership transitions were probably particularly rough, given the difficulty of attracting qualified principals to rural areas. However, without additional information on district hiring practices, it is difficult to say how important this factor was. Similarly, while it is likely that certain demographic subgroups were targeted for improvement as part of the turnaround process, without additional information on how this demographic targeting occurred, any attempt to account for it would be speculative. In particular, it is difficult to know whether the absence of a positive effect for a particular group reflects a lack of effort or a lack of success, especially since different schools likely took different approaches to raising achievement.

Texas is unique in that it decided to award a large percentage of its SIG grants to low-performing charter schools—a confusing policy, since one of the primary motivations for encouraging the growth of charters is to introduce a measure of market discipline into the education sector.[12] Since charter grantees are failing to make significant progress, despite receiving additional funding from SIG, it seems reasonable to ask why nearly all of these schools are implementing Transformation as opposed to Closure. The answer, of course, is that in many states, low-performing charters (like low-performing district schools) are rarely closed (Stuit 2010). Nationwide, only two percent of the first SIG cohort chose Closure over the other three models (Hurlburt et al.). Thus, the real debate going forward may have less to do with the merits of charters versus non-charters than with the merits of school closure versus school turnaround generally.

Arguably, the results of this analysis bolster the case for closure, since SIG schools for the most part failed to make progress, despite receiving additional funding and support. However, since Texas used absolute performance (rather than some measure of school value added) to identify which schools were eligible for a SIG grant, and since essentially all of the difference in performance between SIG and non-SIG schools can be explained by demographics, it could also be argued that these schools are not really "failing" in the first place. By assuming that poor absolute performance reflects poor teaching and/or school management, it is possible that the education officials responsible for implementing SIG are repeating the mistakes of No Child Left Behind by identifying the wrong schools for improvement. If this is the case, we should not be surprised that replacing the leadership at these schools is not leading to better academic outcomes.

in the years immediately following a change in leadership.

[12] Interestingly, including the dummy variables *charter* and *SIG×charter* in various alternative specifications of the model yields negative and statistically-significant estimates for both coefficients across most grades and subjects, implying that not only are charters in Texas performing poorly relative to the rest of Texas schools, but that grant-receiving charters are an unusually low-performing bunch, even after their charter status, SIG status, and demographic characteristics are taken into account.

Before we pass judgment on the merits of SIG, we should remember that implementation of the program has varied considerably by state, and consequently, the impacts of the program may also have varied. While the overwhelming majority of Texas schools chose to implement the Transformation model, the two schools that implemented the Turnaround model saw generally positive results in their first year.[13] Thus, since Dee (2012) found that the positive effects of SIG in California were concentrated in Turnaround schools, it is possible that the results of the two studies may prove consistent with one another, insofar as they reflect the differential impacts of the two models.

[13] Specifically, Azleway Charter School Pine Mountain saw a 17-point increase in 9th grade math proficiency, while Floresville Choice program saw a 24-point increase in 10th grade ELA proficiency and a 6-point decrease in 10th grade math proficiency.

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Does Joining the Extractive Industries Transparency Initiative Have an Impact on Extractive and Non-Extractive FDI Inflows?

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Abstract

The Extractive Industries Transparency Initiative (EITI) invites resource-rich countries to voluntarily publish the payments they receive from corporations and open their books to the scrutiny of certified auditors. In return, the EITI offers potential members a seal of approval inherent to EITI candidacy or compliance that will signal lower political risk to investors, thereby attracting foreign direct investment (FDI) inflows. This thesis uses the Arellano-Bond General Method of Moments estimation to find that changes in EITI status are associated with net FDI inflow increases of over 50 percent on the year of the status change, holding the usual determinants of FDI inflows constant. This paper attempts to determine whether these effects are different across primary, secondary, and tertiary sectors of the economy but does not find significance at conventional levels for this portion of the analysis. These results suggest that countries can attract FDI inflows by joining the EITI and that the incentive structure of the EITI is valid.

I. INTRODUCTION

The Extractive Industries Transparency Initiative (EITI) is a partnership between governments, corporations, and financial investors designed to promote transparency and to address the resource curse: a tendency of resource-rich countries to have poor economic performance, a higher incidence of conflict, and suffer from poor governance.[1] The EITI is voluntary; countries that choose to join the initiative must meet certain requirements, which include the publication of rents derived from resource extraction and independent audits of payments from private and state-owned corporations. In return, the EITI validates the country as a *candidate* and later *compliant* country as it reaches milestones in publishing data accurately. A key assumption of the EITI incentive structure is that validation will signal a better investment climate to investors and attract more foreign direct investment (FDI).

If membership attracts FDI, resource-rich countries have an incentive to join the EITI. Policymakers and academics often agree that FDI fosters economic growth because it brings foreign technology and management practices, which can be adapted by the host country in other contexts (Walsh and Yu 2010; Moran, Graham, and Blomström 2005). Papers that study the determinants of FDI suggest that a country's investment climate is important for investors making FDI decisions. Besides economic and market analyses, investors can be swayed by qualitative governance indicators or deterred by perceptions of high political risk. Participation in international organizations and partnerships like the EITI can increase a government's credibility (Dreher and Voigt 2011) and attract FDI inflows (Dreher, Mikosch, and Voigt 2010). Joining international organizations can signal lower political risk, because it restricts a country from pursuing policies that are harmful to investors such as expropriation, currency manipulation, and discriminatory treatment against foreign investors. EITI membership could have a similar signaling effect.

However, FDI in the extractive industry, which involves physically extracting metals, minerals, and aggregates from the earth, may be less sensitive to the qualitative measures of governance that the EITI tries to address because companies must operate where resources are naturally found, regardless of the quality of institutions. When the determinants of FDI are analyzed disaggregating extractive from non-extractive FDI (manufacturing, services, and construction), qualitative institutional factors are found to have little impact on extractive FDI inflows (Walsh and Yu 2010). This finding raises an interesting concern for resource-rich countries that may consider joining the EITI in order to attract FDI inflows. Would signaling transparency in the extractive sector improve

[1] "Benefits," EITI website, accessed April 3, 2013, http://eiti.org/eiti/benefits

countries' investment climates, as the EITI advertises, or are extractive corporations obliged to invest in resource-rich countries regardless of the quality of their institutions? Alternatively, can resource-rich countries attract non-extractive FDI inflows by joining the EITI and thereby diversify their economies?

This paper uses panel data on 166 countries from 2002 to 2011 to assess whether changing a country's EITI status (by announcing interest, achieving candidacy, or achieving compliance) has an impact on extractive or non-extractive FDI inflows. It thereby assesses whether governments should join the EITI and evaluates the EITI's potential to provide incentives for resource-rich governments to become more transparent and accountable.

II. LITERATURE REVIEW

Papers that use multivariate analysis to assess the impact of institutional quality variables on FDI generally find a positive relationship between the two (Busse and Hefeker 2007; Wei 2000; Dreher, Mikosch, and Voigt 2010). Strong institutions matter to the multinational enterprises (MNEs) that make investment decisions, and transparency is likely to make governments more accountable and breed better quality institutions that can attract FDI. However, investment decisions differ depending on the sector of a particular MNE (See Blonigen 2005; Kolstad and Villanger 2004). Although joining international organizations and reducing corruption are both believed to attract FDI, the existing literature does not study differences between extractive and non-extractive FDI.

The quality of government institutions matters to firms that are making investment decisions. Busse and Hefeker (2007) use data on 83 developing countries for the 1984-to-2003 period and conclude that, "government stability, the absence of internal conflict and ethnic tensions, basic democratic rights, and ensuring law and order are highly significant determinants of foreign investment inflows."

Existing literature has found evidence that corruption in particular can influence FDI inflows. Wei (2000) finds that an increase in the corruption level of a country has a negative effect on inward FDI. Smarzynska and Wei (2000) use empirical evidence to investigate whether perceptions of corruption in a country make MNEs more likely to enter a joint venture or use wholly-owned subsidiaries. They find that corruption perceptions changed the behavior of MNEs regarding FDI, reducing inflows and shifting the ownership structure toward joint ventures.

In order for the EITI to have an impact on FDI, investors would have to be persuaded that joining international initiatives will have an effect on the behavior of governments. Dreher, Mikosch, and Voigt (2010) found that membership in international organizations is an important determinant of FDI inflows because it may restrain a country from pursuing policies that are harmful to investors. In a similar vein, Dreher and Voigt (2011) argue that joining international organizations improves government credibility. The EITI may have a similar effect, even if it is not as formal an international organization as those studied by Dreher, Mikosch and Voigt (the World Trade Organization, the International Center for the Settlement of Investment Disputes, the International Finance Corporation, and certain United Nations conventions).

The literature mentioned so far suggests that EITI status may have a positive impact on FDI because institutions, corruption perceptions, and international organization membership matter to foreign investors. Schmaljohann (2013) finds consistent evidence by evaluating the impact of the EITI on FDI. She finds that joining the EITI increases FDI inflows as a share of Gross Domestic Product (GDP) by up to two percentage points. However, Schmaljohann and the other authors cited above study total FDI inflows. It is very likely that extractive (primary) FDI has different determinants than manufacturing (secondary) or services and construction (tertiary) FDI.

Some researchers have found evidence of different factors influencing FDI across sectors. Kreinin, Abe, and Plummer (1999) use a survey of motivation for outward Japanese FDI compared across sectors. They find that natural resources are the most important motivation for agriculture FDI. A wide range of industries in manufacturing FDI are surveyed and the results are different; securing local markets, establishing production and distribution networks, and cheap labor are their key motivations. For financial services, however, government regulations and restrictions are the most important.

Papers concentrating on particular production sectors also find differing factors that influence FDI inflows. Bajo-Rubio and Lopez-Puejo (2002) find that exchange rates are more important for manufacturing FDI, while economic growth and inflation are less significant. The key determinants for FDI in the food industry are gross national product (GNP) per capita, wages, and exchange rates, while subsidies, stock prices, corporate income taxes, and environmental regulations are insignificant, according to Gopinath (2000). For the chemical industry, McCorriston and Sheldon (1998) and Xing and Kolstad (2002) find that relative stock prices and environmental regulations are important determinants, while corporate income taxes, exchange rates, and GDP per capita are insignificant. In the case of tire manufacturing, Ito and Rose (2002) find significance in a country's GDP and distance from investor country, while the tax rate and political risk are insignificant. Xing and Kolstad (2002) find that neither GDP, exchange rates, stock prices, nor environmental regulations have a significant impact on the machinery and transportation equipment industry FDI, while that of electronics and electrical equipment is determined primarily by exchange rates and corporate income taxes. The key determinants of services FDI are GNP per capita, wealth, GNP growth, trade, exchange rates, and FDI stock, while wages and interest rates are insignificant, according to Yamori (1998), Moshirian (1997), and Miller and Parkhe (1998).

If FDI in different industries within the manufacturing and services sectors have different determinants, the difference between extractive and non-extractive FDI is likely to be even greater. In fact, Walsh and Yu (2010) argue that qualitative institutional variables have an insignificant impact on extractive FDI, while they affect non-extractive FDI flows in different ways for advanced and emerging economies. This finding is consistent with Kreinin's survey, which suggests that resource abundance is the key motivation for agriculture FDI.

These findings pose an interesting question about the growth potential of the EITI, which focuses on transparency in extractive industries. The EITI relies on resource-rich countries joining voluntarily in order to attract FDI. If governments did not believe that they could attract FDI by joining, the EITI would not succeed. On the other hand, it is possible that the EITI could help resource-rich countries diversify their economies. Ofori-Brobbey, Ojode, and Desai (2008) found that political and economic stability attracts non-extractive FDI in sub-Saharan Africa.

III. DATA SOURCES

Table 1 presents a description of the main variables used in this paper. The dependent variable is a measure of net FDI inflows, which is the yearly country data of investment minus disinvestment in the country by foreign investors, as reported by countries to the United Nations Conference on Trade and Development (UNCTAD). These data are combined with World Bank GDP data to generate FDI inflows as a percentage of GDP. Primary, secondary, and tertiary FDI figures are also as reported by countries to UNCTAD and published in the International Trade Center Investment Map. They cover extractive, manufacturing, and construction and services investments respectively. The key independent variable is a dummy variable reflecting EITI status constructed using EITI data.

EITI status is measured by constructed dummy variables based on data from the EITI website. The variable EITI Interest indicates the year in which countries that achieved candidate status signaled their intention of joining the EITI and began collecting data to report for EITI validation. Countries that have declared interest but have not been approved as candidates by the EITI are not included, because it is not possible to measure the degree of commitment of this announcement. EITI Candidate and EITI Compliant are dummy variables that indicate the year in which each member country was awarded that status by the EITI. To avoid penalizing EITI candidates

Table 1. Description of Variables		
Variables	**Description**	**Source**
EITI Interest	Dummy=1 on year when countries that achieved candidate status signaled their intention of joining the EITI	EITI
EITI Candidacy	Dummy=1 on year when EITI awarded candidacy status	EITI
EITI Compliance	Dummy=1 on year when EITI awarded compliance status	EITI
Total FDI	Total net FDI inflows, millions of current US$	UNCTAD
Primary FDI	Primary net FDI inflows, millions of current US$	ITC
Secondary FDI	Secondary net FDI inflows, millions of current US$	ITC
Tertiary FDI	Tertiary net FDI inflows, millions of current US$	ITC
FDI Stock	FDI stock, billions of current US$	WDI
GDP	Gross Domestic Product, billions of US$	WDI
GDP growth	GDP growth, annual %	WDI
GDP p.c. growth	GDP per capita growth, annual %	WDI
Population	Total population, millions of inhabitants	WDI
Tax rate	Total tax rate as a proportion of commercial profits	WDI
Inflation	Inflation, GDP deflator annual %	WDI
Interest rate	Real interest rate, %	WDI
Real effective exchange rate	Real effective exchange rate index	WDI
Official exchange rate	Official exchange rate, 1,000 local currency/US$	WDI
Total trade	Net trade in goods and services, BoP current US$	WDI
Trade Openness	Total trade as a proportion of GDP	WDI
School life expectancy	Expected years of education at birth	UNESCO
Natural resource rents	Total government income from natural resources as a proportion of GDP	WDI
Battle-related deaths	Battle related deaths, thousands	WDI
Internally displaced persons	Internally displaced persons, thousands (high estimate)	WDI
Corruption	Control of corruption percentile rank (0-100)	WGI

Note: When EITI compliance=1, EITI candidacy and interest also=1 in order to avoid penalizing countries that achieve compliance when the model runs the Candidacy or Interest variables.

UNCTAD: United Nations Conference on Trade and Development Statistics

ITC: International Trade Center Investment Map

WDI: World Development Indicators, World Bank

UNESCO: United Nations Educational, Scientific and Cultural Organization Data Centre

WGI: Worldwide Governance Indicators, World Bank that become compliant in models that use the EITI Candidate dummy, the value of the EITI Candidate dummy continues to be one when the value of the EITI Compliant dummy is one. Similarly, EITI Interest continues to be one when a country achieves Candidate and Compliant status.

Other variables of interest are from UNESCO and the World Bank's World Development Indicators and Worldwide Governance Indicators, based on the determinants of FDI frequently used in the literature. Table 2 presents summary statistics for key variables.

IV. METHODS

An ordinary least squares (OLS) regression could show a correlation between EITI membership and FDI inflows, holding observable country characteristics constant. However, it would certainly suffer from bias caused by omitted variables that are not

Table 2. Summary Statistics

Variables	Obs.	Mean	Standard Deviation	Min	Max
Total FDI	1,655	6,459.17	20,788.31	-32,080.20	306,366
Primary FDI	688	1,382.61	6,083.96	-11,267	105,060
Secondary FDI	722	3,112.77	9,633.98	-8,819.50	102,756
Tertiary FDI	733	6,559.87	17,919.95	-28,160	221,214
FDI Stock	1,646	83.97	280.39	0.00	3,600
GDP	1,649	304.51	1,193.87	0.07	15,000
GDP growth	1,641	4.38	5.07	-41.3	46.5
GDP per capita growth	1,641	2.77	4.93	-42.77	42.83
Population	1,660	37.81	139.4	0.07	1,344.13
Tax rate	1,660	0.51	0.43	0.08	3.4
Inflation	1,648	7.31	9.34	-33.79	120.5
Interest rate	1,286	7.26	19.15	-32	508.74
Real effective exchange rate	869	104.78	37.53	57.76	1025.26
Official exchange rate	1,647	0.81	2.89	0.00	25
Total trade	1,426	769.9	58,187.18	-753,286	348,833
Trade as proportion of GDP	1,525	0.9	0.46	0.00	4.4
School life expectancy	1,630	10.7	2.14	2.84	16.53
Natural resource rents	1,483	0.1	0.16	0.00	1.07
Battle-related deaths	1,659	0.1	0.58	0.00	8.4
Internally displaced persons	1,659	127.01	546.03	0.00	6100
Corruption	1,660	48.15	27.85	0.5	100

Sources: International Trade Center Investment Map, World Development Indicators, World Governance Indicators, UNESCO.

observable and correlated with both EITI membership and FDI inflows. A fixed effects model could remove some of the bias caused by time-invariant omitted variables, but bias caused by the time-variant country characteristics would remain. To address the omitted variable bias problem, this paper uses the Arellano-Bond General Method of Moments estimation.

If a single OLS regression could answer this paper's research question, it would be:

$$FDI = \beta_0 + \beta_1 EITI + \beta_2 X_2 + \dots + \beta_n X_n + u$$

Where *FDI* is the log of net FDI inflows, *EITI* is a dummy variable that indicates a country's EITI status, and X_2 through X_n represent the observable control variables that have relationships with both EITI status and FDI inflows.

If the error term "u" were uncorrelated with EITI status after including control variables, then β_1 would accurately measure the true impact of a country's EITI status on FDI inflows. However, it is unlikely that all variables that are possibly correlated with both FDI and EITI status can be included in the model. For example, though a country's cultural tolerance of foreign investment or transparency could affect both FDI inflows and the decision to join the EITI, it cannot be included as a control variable because it is difficult to measure. OLS is rarely used to estimate effects on FDI inflows, because many of their determinants are unobservable.

Fixed effects models are more generally used to find the determinants of FDI. They hold time-invariant country characteristics fixed, because they measure differences in FDI inflows and in all independent variables over periods of time. Consider the linear unobserved effects model for n observations and t time periods:

$$FDI_{i,t} = \beta_0 + \beta_1 EITI_{i,t} + \beta_2 X_{2i,t} + \dots + \beta_n X_{ni,t} + \alpha_i + u_{i,t}$$

Where t = 2002, 2003, … 2011, and i = Afghanistan, Albania … Zimbabwe. Here α_i represents all unobserved time-invariant country effects that could influence both FDI inflows and EITI membership, such as cultural affinity to large source countries of FDI inflows that could be interested in transparency, e.g. the United States. Since parts of α_i are not observable, they cannot be included in the model as control variables. The fixed effects model eliminates α_i by demeaning the variables using the transformation:

$$\begin{aligned} FDI_{i,t} - \overline{FDI}_i &= \beta_0 + \beta_1\left(EITI_{i,t} - \overline{EITI}_i\right) \\ &+ \beta_2\left(X_{2i,t} - \bar{X}_{2i}\right) + \cdots \\ &+ \beta_n\left(X_{ni,t} - \bar{X}_{ni}\right) \\ &+ (\alpha - \bar{\alpha}_i) + (u_{i,t} - \bar{u}_i) \end{aligned}$$

Because α_i is time-invariant, $(\alpha - \alpha_i) = 0$. This means that all time-invariant country effects are automatically controlled for in the model and that time-invariant endogeneity—fixed country characteristics that are correlated with both FDI inflows and EITI status that would bias the results—is removed. This is likely to lead to a more accurate estimate of the effects of EITI status on FDI inflows than the OLS model. However, endogeneity remains in the form of unobserved variables that change over time and are correlated with both FDI inflows and EITI status. For example, a new regime could engage in broad investment promotion that includes EITI membership. The fixed effects model cannot separate the effect of EITI membership on the log of FDI inflows from other investment promotion policies.

The Arellano-Bond General Method of Moments (GMM) is used in FDI literature, because it attempts to solve this and other problems associated with estimating the determinants of FDI. First, causality for many of the X_n variables may run in both directions. For example, GDP growth signals a growing market and may attract FDI inflows. However, FDI inflows may spur GDP growth as well. A new mining project may require the construction of roads, or a new manufacturing plant could raise demand for secondary products. Both would stimulate employment and increase GDP growth. OLS and fixed effects models cannot account for reverse causality.

Second, the Arellano-Bond General Method of Moments is a dynamic panel model that introduces a lagged version of the dependent variable as a control variable. This removes bias in the model by controlling for trends in FDI inflows that were occurring before the EITI status change. It also controls for any clustering effect, which occurs when FDI inflows attract further FDI inflows. For example, if Intel installs a large microprocessor plant in Costa Rica, other companies may invest in Costa Rica in the following years to supply this new plant. The resulting new dynamic panel model would look like:

$$FDI_{i,t} = \beta_0 + \beta_1 EITI_{i,t} + \beta_2 FDI_{i,t-1} + \beta_3 X_{3i,t} + \ldots + \beta_n X_{ni,t} + u_{i,t}$$

Even though the value of β_2 may not be of direct interest, "allowing for dynamics in the underlying process may be crucial for recovering consistent estimates of other parameters," (Bond 2002).

Third, in order to control for time-invariant country characteristics, Arellano-Bond estimators use first differences to further transform the equation:

$$FDI_{i,t} - FDI_{i,t-1} = \beta_0 + \beta_1(EITI_{i,t} - EITI_{i,t-1}) + \beta_2(FDI_{i,t-1} - FDI_{i,t-2}) + \beta_3(X_{3i,t} - X_{3i,t-1}) + \ldots + \beta_n(X_{ni,t} - X_{ni,t-1}) + (u_{i,t} - u_{i,t-1})$$

The effects of this transformation are similar to those of the demeaning process of the fixed effects model.

However, the presence of the lagged version of FDI gives rise to autocorrelation, the correlation between values of a process at different times. Notice that the term $FDI_{i,t-1}$ is on both sides of the equation (Keele and Kelly 2006). Also, time-variant endogeneity persists. To address these issues, Arellano-Bond estimation uses lagged values of independent and dependent variables as instruments (Arellano and Bond 1991; Arellano and Bover 1995; Blundell and Bond 1998). When the idiosyncratic errors $u_{i,t}$ are independent from each other and identically distributed, the first differenced errors $(u_{i,t}-u_{i,t-1})$ are first-order serially correlated. However, assuming that $u_{i,t}$ is serially uncorrelated, the lagged level $FDI_{i,t-2}$ will be uncorrelated with $(u_{i,t} - u_{i,t-1})$ and available as an instrument for the first differenced equation (Bond 2002). Because only lags of two time periods are used as instruments, only serial correlation at order two or higher will result in a misspecified model.

The model is also designed for situations with heteroskedasticity and autocorrelation within countries. In all estimations the standard errors are clustered at the country level to account for possible correlation of a country's error terms over time and heterogeneity between the clusters (Roodman 2006).

V. FINDINGS

The following tables illustrate how countries differ according to their EITI status. EITI member countries are generally less open to trade, have less educated populations, and have lower governance indicators.

Table 3 shows how FDI values differ on average based on EITI status (See Appendix for list of EITI countries by year of status change). The raw FDI data confirms the impression that there may be a reverse causality problem. That is, EITI countries could have lower FDI inflows because these low FDI inflows pushed them to join an initiative that is meant to attract FDI. However, it seems that the EITI may be associated with higher FDI inflows as a percentage of GDP.

Regarding control variables, it appears that EITI countries generally have lower GDPs and populations, and higher tax rates, inflation rates, and interest rates than non-EITI countries. Likewise, EITI countries have higher total trade figures but lower trade openness and lower education rates than other countries. Due to large variations, there are no variables with significant differences in means between EITI status groups. Predictably, EITI countries have higher resource rents.

Evidence of the resource curse is shown by the consistently lower corruption percentile rankings for EITI countries (lower percentile rankings indicate higher perceptions of corruption). This finding is generalized across all Worldwide Governance Indicators. War, measured in battle-related deaths and internally displaced persons, is included because it is a time-variant factor correlated with FDI inflows and also possibly correlated with EITI status. For example, Yemen was suspended from the EITI between June 2011 and June 2012 after a period of prolonged violence. Such violence probably had a negative effect on FDI inflows, and if these variables were not included in the model this decline could mistakenly be attributed solely to EITI status changes.

VI. RESULTS

Arellano Bond estimation shows that EITI candidacy is associated with a 55

Table 3. Differences in Means of FDI Inflows According to EITI Status

	EITI Status			
Variables	None	Interested	Candidate	Compliant
Net Foreign Direct Investment (FDI)	6,948.79	4,102.01	2,305.21	2,277.63
	(21,301.77)	(26,170.82)	(3,933.67)	(20,788.31)
Primary FDI	1,440.98	581.79	966.67	2,146.30
	(6,358.97)	(1,612.96)	(1,317.66)	
Secondary FDI	3,286.68	-97.25	634.74	
	(9,887.10)	(1,460.74)	(1,334.08)	
Tertiary FDI	6,867.64	919.98	2,193.87	
	(18,396.25)	(1,737.14)	(4,257.96)	
FDI as a percentage of GDP	4.89	5.17	8.49	13.95
	(7.74)	(6.44)	(9.33)	(17.23)
Primary FDI as a percentage of GDP	0.8	2.72	6.24	4.84
	(2.76)	(6.83)	(8.76)	
Secondary FDI as a percentage of GDP	1.05	0.58	2.07	
	(2.45)	(1.10)	(4.96)	
Tertiary FDI as a percentage of GDP	3.14	1.86	3.26	
	(6.74)	(2.14)	(3.87)	

Standard deviations in parenthesis

Sources: International Trade Center Investment Map, dummy variables for EITI status constructed based on EITI data.

percent increase in FDI inflows holding the variables included in the model constant. The method removes some of the upward bias found in OLS and fixed effects estimates, moderating the results.

Table 5 shows the estimated influence of EITI candidacy on the natural log of FDI inflows using ordinary least squares (OLS), fixed effects, and Arellano-Bond General Method of Moments estimation. In column 1, OLS suggests that holding the stated control variables constant, EITI candidacy is associated with an approximate 110 percent increase in net FDI inflows. These results seem exaggerated. Schmaljohann (2013) uses a different model with Arellano-Bond estimation to find that EITI candidacy is associated with an increase of FDI as a percentage of GDP of 2 percent, with the mean of FDI as a percentage of GDP for this sample being close to 5 percent. It is very likely that the OLS model in Table 5 suffers from omitted variable bias because unobserved variables may have an effect on both EITI candidacy and FDI inflows. It is probable that the results are biased upward because many of the unobserved variables that make a country more likely to join the EITI are also likely to attract higher FDI inflows.

Table 4. Differences in Means of Key Variables According to EITI Status

	EITI Status			
Variables	None	Interested	Candidate	Compliant
FDI Stock	91.34	58.29	14.37	17.2
	(283.93)	(403.59)	(31.47)	(40.31)
GDP	330.03	241.51	47.76	54.31
	(1,209.36)	(1,729.70)	(118.33)	(117.48)
GDP growth	4.22	5.15	5.66	6.74
	(5.06)	(4.09)	(5.29)	(6.19)
GDP p.c. growth	2.7	2.88	3.38	4.15
	(4.93)	(3.96)	(5.37)	(5.95)
Population	39.51	27.79	25.26	17.39
	(147.65)	(52.62)	(42.32)	(36.04)
Tax rate	0.49	0.61	0.66	0.38
	(0.40)	(0.50)	(0.68)	(0.13)
Inflation	7.01	8.27	10.52	8.41
	(9.13)	(9.43)	(8.41)	(9.14)
Interest rate	6.85	11.64	10.55	9.83
	(19.81)	(11.47)	(10.60)	(12.91)
Real effective exchange rate	102.99	103.74	142.14	104.51
	(12.45)	(12.32)	(165.25)	(10.49)
Official exchange rate	0.76	1.04	1.42	0.25
	(2.90)	(2.53)	(3.10)	(0.41)
Total trade	845.68	-5,151.89	3,728.81	4,540.21
	(58,987.73)	(76,094.17)	(11,156.27)	(9,195.23)
Trade openness	0.91	0.8	0.81	1.02
	(0.48)	(0.32)	(0.27)	(0.32)
School life expectancy	10.9	8.94	9.28	9.81
	(2.07)	(2.39)	(1.80)	(1.94)
Natural resource rents	0.08	0.2	0.23	0.22
	(0.15)	(0.18)	(0.21)	(0.17)
Battle-related deaths	0.1	0.1	0.11	0.00
	(0.58)	(0.61)	(0.64)	0.00
Internally displaced persons	128.184	90.58	148.11	62.05
	(565.23)	(342.91)	(426.47)	(185.87)
Corruption	51.2	27.2	24.57	29.37
	(27.51)	(20.27)	(16.92)	(23.19)

Standard deviations in parenthesis

Sources: International Trade Center Investment Map, World Development Indicators, World Governance Indicators, UNESCO, dummy variables for EITI status constructed based on EITI data.

Table 5. Estimated Influence of EITI Candidacy on Log of FDI Inflows			
	(1)	(2)	(3)
Variables	**Ordinary Least Squares**	**Fixed Effects**	**Arellano-Bond GMM**
EITI Candidate	1.10***	0.63***	0.55*
	(0.22)	(0.19)	(0.32)
Log. GDP p.c. growth (t-1)	0.38***	0.06	0.04
	(0.07)	(0.05)	(0.07)
GDP p.c. (t-1)	0.09***	0.00	0.00
	(0.01)	(0.02)	(0.01)
Trade openness (t-1)	-0.44**	0.07	-0.35*
	(0.19)	(0.38)	(0.20)
Log. Resource rents (%GDP) (t-1)	0.10**	-0.06	0.03
	(0.04)	(0.08)	(0.03)
Total tax rate	-0.24	-0.12	0.16
	(0.27)	(0.21)	(0.21)
Official exchange rate	0.04*	-0.08	0.00
	(0.02)	(0.19)	(0.02)
Inflation (GDP deflator, annual)	-0.01	0.02**	0.00
	(0.01)	(0.01)	(0.01)
Real interest rate	-0.03*	0.01	-0.03**
	(0.01)	(0.01)	(0.01)
School life expectancy	0.16**	0.16*	0.00
	(0.06)	(0.08)	(0.06)
Battle-related deaths	0.01	-0.03	-0.07
	(0.09)	(0.04)	(0.04)
Displaced persons	0.00***	-0.00	0.00
	(0.00)	(0.00)	(0.00)
Corruption	0.00	0.01	0.01*
	(0.00)	(0.01)	(0.00)
Log. FDI (t-1)			0.83***
			(0.04)
Constant	4.96***	3.66***	1.28**
	(0.68)	(1.16)	(0.60)
Observations	812	812	786
R-squared	0.32	0.32	
Included fixed country effects		yes	
Included fixed year effects		yes	
Sargan p-value			0.537
Number of countries		127	126

Panel data for 2003-2011 used

Robust standard errors in parentheses

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

Joining the EITI may be part of a broader investment promotion strategy, or countries that are more open to international participation could attract more FDI and be more likely to join the EITI.

Table 6 addresses the possibility of multicollinearity based on the OLS regression in column 1. Since the variance inflation factor of the following variables when regressed on the log of FDI inflows is low, multicollinearity is discarded as a serious concern that could inflate the standard errors of the coefficients of a model using these regressors.

Column 2 of Table 5 shows that controlling for fixed country and year effects moderates the estimated influence of EITI candidacy on FDI inflows to an approximate 63 percent increase. This confirms the prediction that time-invariant endogeneity would bias the OLS results upward. This could happen, for example, if countries are more culturally open to participating in the global economy and more willing to allow foreign investment within their borders, making them more likely to join the EITI. OLS would incorrectly interpret this unobservable cultural affinity for international participation as part of the estimated influence of EITI candidacy on FDI inflows. Because the fixed effects model estimates only variation within countries, these time-invariant characteristics that were biasing the results upward are removed from the model.

Table 6. Variance Inflation Factor

Variables	VIF
Corruption	2.75
GDP p.c.(t-1)	2.01
School life expectancy	1.98
Inflation (GDP deflator, annual)	1.84
Real interest rate	1.67
Log. Resource rents (%GDP)(t-1)	1.32
Total tax rate	1.22
EITI candidacy	1.15
Displaced persons	1.14
Trade openness(t-1)	1.13
Log. GDP p.c. growth(t-1)	1.11
Official exchange rate	1.09
Battle-related deaths	1.09
Mean VIF	1.5

It is possible that fixed year characteristics could bias the results as well. For example, the years 2008 to 2009 had significant activity in countries declaring interest and achieving compliance or candidacy in the EITI, and the 2008 financial crisis could have been associated with lower FDI inflows. Lower FDI inflows associated with the financial crisis could mistakenly be attributed to EITI status changes for these countries if time fixed effects are not included. However, time fixed effects can be included or removed from the fixed effects model without significant changes in the results of column 2.

With the fixed effects model, the possibility of time-variant endogeneity remains and is likely to continue to bias results upward. The calculations in column 3 try to remove both forms of endogeneity by using Arellano-Bond estimation. It uses dynamic panel estimation to control for existing trends in FDI inflows not associated with EITI candidacy, first differences to control for time-invariant fixed country effects, and lagged versions of all control variables as instruments to remove autocorrelation and endogeneity caused by time-variant country characteristics (see Methods section). The result is a further moderation of the estimated influence of EITI candidacy on FDI inflow to an approximated 55 percent. This makes sense, because countries that try to attract FDI inflows are likely to do so by various means that span multiple years. They could decide to join the EITI, strengthen investment promotion agencies, and offer foreign investors incentives that are not quantified by the control variables in the model. The fixed effects model would mistakenly interpret other changes in investment promotion strategies as part of the influence of EITI candidacy on FDI inflows. These results are significant at the 10 percent level, while OLS and fixed effects are significant at the one percent level.

This specification, in which the lagged determinants of FDI inflows are used as instruments, passes the Sargan tests for overidentifying restrictions, providing evidence of the validity of the choice of instruments (Roodman 2006).[2]

Table 7 continues to use the Arellano-Bond GMM method to estimate the influence of EITI "interest" and "compliance" on the log of FDI inflows**.** Column 2 is the base case, equal to column 3 in Table 5, and columns 1 and 3 replicate the model using EITI interest and compliance as dependent variables.

These results show that there may be an influence on FDI inflows in each step of EITI membership. Countries that were eventually awarded candidacy may have increased their FDI inflows by an approximate 52 percent by declaring interest in the EITI. The EITI candidate status award may have increased FDI inflows an approximate 55 percent, and compliant status an approximate 71 percent. Even though the latter estimate is not significant at conventional levels, it has a p-value of 0.15. Because there are only 19 countries that were awarded compliant status on or before 2011, it is likely that more data would show this estimate to be significant in future years.

Table 8 continues to use Arellano-Bond GMM estimation to show sectoral results of the influence of EITI candidacy on the natural log of FDI inflows. Column 1 is the base case, and columns 2, 3 and 4 use the same model with primary, secondary, and tertiary FDI inflows as dependent variables. Differing estimated results and differing significance for control variables seem to imply that

[2] Arellano-Bond estimation is intended for large-N, small-T panels because the use of lags could lead to over-identification in long (large-T) panels. This leads to potential danger of correlation between over-identifying instruments and the residuals. The central assumption of the Arellano-Bond estimation that the instruments, as a group, are exogenous can be tested with the Sargan test. The null hypothesis of this test is that the instrumental variables are uncorrelated with the residuals and therefore useful as instruments; the higher the p-value of the Sargan statistic, the greater the probability that the instruments are valid (Mileva 2007).

Table 7. Estimated Influence of EITI Status on Log of FDI Inflows Using Arellano-Bond GMM			
Variables	**(1)**	**(2)**	**(3)**
EITI Interest	0.52*		
	(0.27)		
EITI Candidate		0.55*	
		(0.32)	
EITI Compliant			0.71
			(0.60)
Log. GDP p.c. growth (t-1)	0.02	0.04	0.00
	(0.08)	(0.07)	(0.07)
GDP p.c. (t-1)	0.00	0.00	0.00
	(0.01)	(0.01)	(0.01)
Trade openness (t-1)	-0.32	-0.35*	-0.28
	(0.22)	(0.20)	(0.23)
Log. Resource rents (%GDP) (t-1)	0.02	0.03	0.03
	(0.04)	(0.03)	(0.03)
Total tax rate	0.15	0.16	0.17
	(0.21)	(0.21)	(0.20)
Official exchange rate	0.01	0.00	-0.00
	(0.02)	(0.02)	(0.02)
Inflation (GDP deflator, annual)	0.01	0.00	0.01
	(0.01)	(0.01)	(0.01)
Real interest rate	-0.02*	-0.03**	-0.02*
	(0.01)	(0.01)	(0.01)
School life expectancy	0.03	0.00	0.01
	(0.06)	(0.06)	(0.06)
Battle-related deaths	-0.08**	-0.07	-0.05*
	(0.03)	(0.04)	(0.03)
Displaced persons	0.00	0.00	-0.00
	(0.00)	(0.00)	(0.00)
Corruption	0.01*	0.01*	0.01
	(0.00)	(0.00)	(0.00)
Log. FDI(t-1)	0.83***	0.83***	0.84***
	(0.05)	(0.04)	(0.04)
Constant	0.83	1.28**	1.18**
	(0.62)	(0.60)	(0.58)
Observations	786	786	786
Sargan p-value	0.539	0.537	0.533
Number of countries	126	126	126

Panel data for 2003-2011 used. Two-step robust standard errors in parentheses.

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

Note: EITI compliance is not significant at conventional levels, possibly due to a small number of observations. The p-value for this estimate is .15.

there may be a different influence between sectors. This finding would be consistent with the literature that suggests that the determinants of FDI are different across sectors. It makes intuitive sense that an extractive enterprise that will drill for oil or minerals in a country to sell them on the global market would be motivated differently than a company that would manufacture and sell or deliver services to the local market. However, none of the estimates are statistically significant at conventional levels. Sensitivity analyses that added sector-specific determinants did not reveal significant results either. It is very likely that this is due to missing sector-specific FDI data.

VII. DISCUSSION

The results of this research suggest that the key assumption used to promote the EITI (that a country can increase FDI inflows by joining the EITI) is supported by empirical evidence. This finding has policy implications for both countries that want to attract FDI inflows and for the EITI itself.

From the perspective of countries that want to attract FDI inflows, the EITI is a useful policy option. The results of this paper suggest that improving corruption perceptions, included in the model as a control variable, may not be enough to attract FDI. The EITI seal of approval seems to be an effective signaling mechanism of lower corruption for investors. When leads are introduced to the model to see whether the gains from EITI status changes carry over to future years, the results lose significance. Even though the influence suggested by this paper of EITI status on FDI inflows is limited in time, the gains from increased FDI inflows do carry over to future years, as investment is associated with an increase in future production. Furthermore, because the effects of declaring interest and achieving compliance are similar, joining the EITI may deliver gains through multiple stages of the process. It is likely that future research with more observations regarding EITI compliance would show that this step in the EITI process also delivers FDI gains.

This paper uses models to isolate the influence of EITI status on FDI inflows. It does not consider the effects of more comprehensive investment promotion strategies. Future research that examines the relationships between joining the EITI and other strategies, such as spending on investment promotion agencies or other initiatives that are not reflected by the control variables in this paper, could assist countries in designing more effective investment promotion strategies.

From the perspective of the EITI as an institution, the results of this paper suggest that its promotion strategy of appealing to countries to join in order to attract FDI inflows is supported by empirical evidence. The incentive structure of the EITI does not need reform in order to be effective. Furthermore, other attempts by the international community to promote good governance may find a useful example in the EITI. Because participation is voluntary and

Table 8. Estimated Influence of EITI Candidacy on Log of FDI Inflows, Total and by Sector				
	(1)	(2)	(3)	(4)
Variables	Log. Total FDI	Log. Primary FDI	Log. Secondary FDI	Log. Tertiary FDI
EITI Candidate	0.55*	0.04	0.53	-0.37
	(0.32)	(0.45)	(0.67)	(0.31)
Log. GDP p.c. growth (t-1)	0.04	0.21*	0.11	0.12
	(0.07)	(0.11)	(0.14)	(0.08)
GDP p.c. (t-1)	0.00	0.01	0.02	0.01
	(0.01)	(0.01)	(0.02)	(0.01)
Trade openness (t-1)	-0.35*	-0.79	-0.81*	-0.59*
	(0.20)	(0.59)	(0.42)	(0.32)
Log. Resource rents (%GDP) (t-1)	0.03	0.03	0.03	-0.05
	(0.03)	(0.12)	(0.05)	(0.06)
Total tax rate	0.16	0.34	1.1	-0.07
	(0.21)	(0.60)	(0.68)	(0.63)
Official exchange rate	0.00	-0.04	0.06	0.06
	(0.02)	(0.05)	(0.05)	(0.03)
Inflation (GDP deflator, annual)	0.00	-0.00	-0.04	-0.03
	(0.01)	(0.03)	(0.03)	(0.02)
Real interest rate	-0.03**	0.01	-0.07**	-0.02
	(0.01)	(0.02)	(0.03)	(0.02)
School life expectancy	0.00	0.13	-0.06	0.02
	(0.06)	(0.10)	(0.16)	(0.10)
Battle-related deaths	-0.07	-0.22*	-0.06	-0.04
	(0.04)	(0.12)	(0.17)	(0.06)
Displaced persons	0.00	0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Corruption	0.01*	0.00	0.01	0.00
	(0.00)	(0.01)	(0.01)	(0.01)
Log. FDI (t-1)	0.83***			
	(0.04)			
Log. Primary FDI (t-1)		0.77***		
		(0.08)		
Log. Secondary FDI (t-1)			0.75***	
			(0.08)	
Log. Tertiary FDI (t-1)				0.79***
				(0.05)
Constant	1.28**	-0.02	2.57	1.6
	(0.60)	(1.34)	(2.33)	(1.17)
Observations	786	330	377	391
Sargan p-value	0.537	0.387	0.765	0.942
Number of countries	126	75	78	78

Panel data for 2003-2011 used. Two-step robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

beneficial to members, it is possible that this incentive structure can work as a less confrontational alternative for other international initiatives that use more coercive diplomatic efforts by governments or name-and-shame strategies by NGOs. Voluntary multi-stakeholder partnerships to promote good governance similar to the EITI could be applied to issues such as human trafficking, trade, labor standards, and environmental policies.

Although a more than 50 percent increase in FDI inflows may seem high, FDI inflows are highly volatile (see Table 2) and this estimate of the influence of EITI membership is not unrealistic. This paper measures FDI differently, but finds similar results to those of Schmaljohann (2013), who finds that EITI candidacy is associated with an increase of FDI as a percentage of GDP of up to two percent. The mean of FDI as a percentage of GDP for the dataset used in this paper is close to five percent. Therefore, a two percentage point increase in FDI inflows as a percentage of GDP is comparable to a 55 percent increase in FDI inflows.

This paper's results are robust to certain stress tests. For example, some papers use real effective exchange rates as opposed to official exchange rates because the latter may introduce government distortions to market exchange rates. Though a valid concern, this paper's results are robust to the replacement of official exchange rates for real effective exchange rates, although missing data from the latter reduces the number of observations used to estimate results to 371. Some papers include FDI stock to control for clustering effects. This paper does so by including the lagged version of FDI inflows as a control variable, but results are also robust to the inclusion of FDI stock as a control variable. The results are also robust the inclusion of all other World Governance Indicators, including the removal of corruption, battle-related deaths, and displaced persons, and changing the dependent variable to FDI as a percentage of GDP does not change results significantly.

The results of this paper lose significance when lags from the log of GDP per capita growth, GDP per capita, log of resource rents, and trade openness are removed. This highlights one of the limitations of the data used; while FDI decisions are made in real time, data are reported in yearly intervals. Similarly, changes in EITI status are recorded by year. Though it would make sense to use differently timed control variables when a country changes its status in January or December, the data does not allow for that. It is possible that future research using monthly data would produce more accurate results. However, some variables may need to be lagged even with the use of monthly data. Investors make some decisions based on the observed values of some economic indicators like GDP, GDP growth, and trade openness. On the other hand, they could base their decisions on the expectation in future values of other variables like taxes, exchange rate volatility or interest rates that are not necessarily based on past performance but rather information about policy changes. Because projections of some economic variables are usually based on recent past economic performance, the use of lags for these variables makes sense.

The greatest limitation to the results of this paper is the quality of data. Most variables used are as reported by governments to the World Bank, UNESCO, or UNCTAD and data manipulation or inconsistent data measurement across countries could affect the results. Missing sector-specific FDI data prevented this paper from reaching a conclusion on the influence of joining the EITI on FDI inflows by sector. Future research with better and more abundant data could show that the EITI influences FDI inflows in each sector differently. Meanwhile, the question of whether the EITI can help a country diversify its economy remains unresolved. The estimates that did show a significant influence of the EITI on FDI inflows also have data problems. Of a total of 1660 observations in the dataset, the model includes slightly less than half. Even though more developed countries are more likely to have better data, this is unlikely to bias the results of this paper because the Arellano-Bond model estimates this relationship based on changes in the variables of interest. The effect that this missing data problem is likely to have on these results is that high- and middle-income countries will have a more prominent role in the estimates than low-income countries with missing data.

Despite of data limitations, this paper reaches a conclusion similar to that of Schmaljohann (2013) through a different model specification. Future research can improve on sectoral analyses with better data, but in the meantime, the governments of resource-rich countries will make policy decisions based on existing information. They should consider the EITI as an effective tool to attract FDI.

VIII. APPENDIX

Table 9. List of EITI Countries by Year of Status Change

EITI Country	Interested	Candidate	Compliant
Afghanistan	2009	2010-2011	
Albania		2009-2011	
Azerbaijan		2002-2008	2009-2011
Burkina Faso	2008	2009-2011	
Cameroon	2002-2006	2006-2011	
Central African Republic	2006-2008	2009-2010	2011
Chad	2007-2009	2010-2011	
Congo, Dem. Rep.		2007-2011	
Congo, Rep.	2006	2007-2011	
Cote d' Ivoire	2006-2007	2008-2011	
Gabon	2004-2006	2007-2011	
Ghana	2003	2004-2009	2010-2011
Guatemala	2010	2011	
Guinea	2005-2006	2007-2011	
Indonesia	2009	2010-2011	
Iraq	2009	2010-2011	
Kazakhstan	2005-2006	2007-2011	
Kyrgyz Republic	2004-2009	2010	2011
Liberia	2008		2009-2011
Madagascar	2007, 2011	2008-2010	
Mali	2006	2007-2010	2011
Mauritania	2005-2008	2009-2011	
Mongolia	2006	2007-2009	2010-2011
Mozambique	2008-2010	2011	
Niger	2005-2009	2010	2011
Nigeria	2002-2006	2007-2010	2011
Norway	2008-2009	2010	2011
Peru	2004-2009	2010-2011	
Sao Tome and Principe	2011	2008-2010	
Sierra Leone	2006-2007	2008-2011	
Solomon Islands	2011		
Tanzania		2009-2011	
Timor-Leste	2007	2008-2009	2010-2011
Togo		2010-2011	
Trinidad and Tobago	2010	2011	
United States	2011		
Yemen, Rep.	2005-2006	2007-2010	2011
Zambia	2008-2010	2011	

Note: Candidate and Compliant status are awarded by the EITI. Interest is determined based on the previous years reported by countries that achieved candidacy, except for the United States, whose "interested" status was recognized by the EITI. Suspended countries that are not delisted are demoted to Interested (Sao Tome and Principe 2011, Madagascar 2011).

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An Analysis of the Relationship Between Food Deserts and Obesity Rates in the United States

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Abstract

Defined as large geographic areas where residents have limited access to grocery stores, food deserts are thought to contribute to poor diets, especially among people with low incomes. In 2009, the Economic Research Service (ERS) at the United States Department of Agriculture (USDA) delivered a report to Congress that included the Food Desert Locator database, which provides a nationwide quantitative standard for categorizing a census tract as a food desert. In this study, I aggregate these data to the county level in order to conduct a cross-sectional analysis of the relationship between food desert intensity and obesity rates. I find that while there is a positive and statistically significant relationship between these two variables, the magnitude of this relationship is too small to have a compelling impact. These results suggest that the USDA's Food Desert Locator may have some promise as a nationwide measure, but they also suggest the need for additional testing and improvement in order to enhance the measure's utility as a guide for policymaking.

I. INTRODUCTION

Michelle Obama's "Let's Move" campaign to eliminate childhood obesity by 2030 has captured the attention of the public, the media, and lawmakers across the country. Pundits have given the first lady credit for securing the passage of the "Food Conservation and Energy Act of 2008" as well as the "Healthy Hunger-Free Kids Act of 2010" (Huber 2010; USDA ERS 2012b). The first lady's campaign has been so successful in part due to the widespread recognition of the problem of rising obesity rates. In 2008, 20 percent of children aged six through eleven were obese, compared to just 7 percent in 1980 (Centers for Disease Control 2012a). Obesity rates among older age groups are even higher, peaking at 31 percent among 45 through 64 year olds (Mendez 2010). Obesity also has a powerful effect on personal and public health-care costs. Compared to people of normal body mass index (BMI), obese people are estimated to have lifetime medical costs that are between 36 to 100 percent higher, and nearly 20 percent of current health-care costs in the United States are estimated to be obesity related (Hammond 2012).[1]

[1] BMI is calculated by dividing weight in kilograms by height in meters and squaring the result. This measure serves as a simple and inexpensive approximation of body fatness that correlates with direct measures of body fat. The Centers for Disease Control (CDC) define weight categories based on BMI scores as follows: any score over 30 is considered obese, scores between 25 and 19.9 are overweight, scores between 18.5 and 24.9 are normal, and any score under 18.5 is underweight (2011).

In addition, public health experts have noted that the segments of the population with the highest obesity rates have the lowest incomes and the least education (Drewnowski 2004). In investigating the relationship between poverty and obesity, scholars have begun analyzing the geographical distribution of healthy food (Clarke, Eyre, and Guy 2002; Whelan et. al. 2002; Wrigley 2002; Ver Plog 2010; Leete, Bania, and Sparks-Ibanga 2012). When residents of a local area have limited incomes and mobility, stores that sell healthy food are often scarce or difficult to access. Experts in the field have coined the term "food deserts" to describe areas with low access to healthy food that are often economically disadvantaged. The food desert phenomenon has gained so much visibility that "ensuring access to healthy food" has been incorporated as one of the five pillars of the Let's Move campaign (Let's Move 2011).

In light of these developments, this thesis seeks to determine whether food deserts can increase understanding about the causes of obesity. In this paper, I investigate whether food desert intensity is related to county obesity rates. My findings indicate that there is a positive and statistically significant relationship between these two variables. However, the magnitude of this relationship is quite small.

II. BACKGROUND

In 2009, in response to a federal directive to perform a year-long study of areas with limited access to affordable and nutritious food, the United States Department of Agriculture's Economic Research Council (USDA ERS) delivered a report to Congress that included the *Food Desert Locator* database generated using a quantitative standard for categorizing census tracts as food deserts (USDA ERS 2012a).[2]

The USDA report, which was produced by the Healthy Food Financing Initiative (HFFI), defined a food desert as a census tract in which: a) at least 33 percent of residents live farther than one mile from a grocery store in urban areas or ten miles in rural areas and b) the poverty rate is 20 percent or higher and/or the median family income is 80 percent lower than the median family income for the surrounding area (USDA ERS 2012a).[3] Based on these standards, the HFFI identified 6,530 census tracts in the United States that fit its definition of a food desert. Almost 60 percent of US counties contain at least one food desert.[4]

The functional accuracy of the HFFI's categorization matters because it will affect how the debate over food access is framed and how the federal government allocates funds. For fiscal year 2013, the USDA requested $86.5 million in funding to combat food deserts through five different financial and technical assistance programs (USDA 2012). In concert with these USDA programs, in both 2011 and 2012 the Treasury Department's Community Development Financial Institutions Fund (CDFI) distributed 24 grants totaling $43.5 million to community enterprises and local lending institutions (The Reinvestment Fund 2012). The *Food Desert Locator* plays a large role in determining the geographical distribution of these grants and other assistance. Data from the *Food Desert Locator* database are made available to the public, in part to assist grant writers in making a case for their proposals (USDA ERS 2012).

Though finding that the *Food Desert Locator* is not predictive of obesity would not necessarily negate the underlying theory that access to grocery stores impacts health, it would highlight the constraints of the HFFI designation. Evaluating the validity of this metric is an especially important task since it directly impacts policy decisions. Along these lines, while the primary objective of this research is to evaluate the link between food deserts and obesity rates, a secondary goal of the study is to assess the utility of the *Food Desert Locator* as a guide for government funding decisions.

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[2] A census tract is a statistical area defined by the US Census Bureau. The areas are semi-permanent designations designed for use over time in order to be able to compare statistical data. Census tracts range between 1,200 and 8,000 people in population size.

[3] The HFFI is an inter-agency working group composed of members from the Treasury Department, the Agriculture Department, and the Department of Health and Human Services (Office of Community Services, 2011). In some smaller census tracts, if the percent of residents who live farther than the specified distance from a grocery store is above 33 percent, but the actual number of people is below 500, the tract is not classified as a food desert.

[4] This figure is calculated from the 3,138 US counties used in this study, which does not include counties in Puerto Rico or the five counties for which food desert and/or obesity data was unavailable.

III. LITERATURE REVIEW

History of Food Desert Research

Despite the high level of public attention on food deserts, scholarly work on the subject is still developing. The first use of the term has been credited to British researchers analyzing food access in the United Kingdom during the early 2000s (Clarke et al. 2002; Whelan et al. 2002; Wrigley 2002). Later, as the concept began to gain traction in the United States, scholars used it as a framework to analyze food access at a local level. For example, the Mari Gallagher Research and Consulting Group (2006) found that Chicago census tracts with the least access to healthy food had the highest levels of health problems. Raja, Ma, and Yadav (2008) studied differences in food access between white and minority neighborhoods in Erie County, New York. Widener, Metcalf, and Bar-Yam (2011) studied how seasonal farmers markets relate to the distribution of food deserts in Buffalo, New York. Leete et al. (2012) compared several methods of defining food deserts in Portland, Oregon and found that more dispersed suburban areas also suffer from this problem despite the perception that food deserts are an inner city issue.

Food desert definition methodology has evolved over time, each study building on innovations from previous work. However, researchers have nonetheless used diverse criteria to define food deserts. As Leete et al. (2008) point out, each research team must decide on four basic criteria when creating a definition: the geographic unit of analysis, a definition of nutritious food, a geographical threshold for access to food, and a threshold for defining populations that are vulnerable to food-access limitations.

Comparison of Food Desert Definitions

The analysis by Raja et al. (2008) of Erie County and Widener et al.'s (2011) study of Buffalo both use census block data to identify food deserts. This approach allows for a relatively geographically precise definition of food deserts, as a census block is roughly analogous to a city block and is the smallest geographical area used by the Census Bureau (US Census Bureau 2011). Most other food desert studies use the census tract, which are larger statistical areas that have average populations of about 4,000 people, as their geographic unit of analysis (Ver Plog 2010; Leete et al. 2008).

The easiest method for defining access to nutritious food is to use large chain grocery stores as a proxy, since larger stores typically carry fresher and more diverse products. Both Ver Plog (2010) and Leete et al. (2008) use this approach. However, some researchers adopt a more fine-grained approach. For example, Widener et al.'s (2011) research team supplemented supermarket data with listings of seasonal farmers markets. A USDA research team (Mantovani et al. 1997) used a composite score based on the relative availability of the various foods in the government's "Thrifty Food Plan" sample basket. Raja et al. (2008) distinguished six categorizations of food retail store types. The Mari Gallagher Research and Consulting Group (2006) combined supermarket data with fast food restaurant data to create a food balance score, which attempts to capture access to healthy food relative to unhealthy food.

Researchers typically define thresholds for access to food either in terms of distance or travel time to healthy food retailers. Across all studies, the threshold for low access depends on urbanicity (USDA ERS 2012b). Standard distance for urban areas tends to be an approximately 15-minute walking time, though actual distances vary slightly based on the walking speed estimate that a given researcher assumes. Researchers' rural estimates vary more. Some studies use simple radial distance to a grocery store (Leete et al. 2008), whereas other studies calculate actual travel times based on available routes (Raja et al. 2008). Leete et al. (2008) also take into account that individual preferences may cause a family to forego the closest store for one that better meets their needs, and they use an average of the distance to the three nearest grocery stores as one of the three different measures of grocery store proximity in the study. Bader, Purciel, Yousefzadeh, and Neckerman (2010) point out that food access goes beyond distance to a grocery store; it can also be influenced by vehicle ownership, access to public transit, and neighborhood safety.

Food desert definitions differ greatly in their measurement of how vulnerable a community is to the problem of food access. Measures of economic resources such as income level (Ver Plog 2010; Leete et al. 2008) are the most commonly used measure of vulnerability, but race (Raja et al., 2008) and access to a car (The Reinvestment Fund 2012) have also been used as measures. However, some studies leave this dimension out of the analysis altogether (Widener et al. 2011; Mari Gallagher Research and Consulting Group 2006).

With such disparate ways of defining food deserts, results are difficult to compare across studies. However, the first numerically based, nationwide studies of food deserts in the United States may be starting to resolve this problem.[5] The HFFI created a standardized national database of food deserts at the census tract level based on income, population, and food retailer data (USDA ERS 2009). The HFFI defines households as having sufficient access to nutritious food if they are within a one-mile radius of a supermarket in urban areas and a

[5] The Food Conservation and Energy Act of 2008 mandated two studies of food deserts. This investigation relies on data from the first of these studies. A second major federal government report was produced by the Treasury Department's Reinvestment Fund (2012). This report defined "Limited Supermarket Access Areas" using census block groups. These areas were defined by combining census block data on income, car ownership data, and distance to a full service food retail store into a composite scale. The specificity of this measure allows for deeper analysis of patterns of food access within the report, but the opacity of the definition makes it less amenable to future application by other researchers.

10-mile radius in rural areas. They define economically vulnerable areas as census tracts with a poverty rate of 20 percent or higher and/or a median family income is 80 percent lower than the median family income for the surrounding area (USDA ERS 2009). Census tracts are classified as food deserts by the HFFI if they fall in the criteria for being economically vulnerable and a majority of the households within the census tract do not meet the criteria for having sufficient access to a supermarket.

Since the study of food deserts is still in its infancy, the majority of analysis of this topic focuses mainly on whether food deserts actually exist. Though some papers touch on why food deserts arise and their effects, this arena remains largely unexplored, especially at the national level. To help fill in this gap, the present investigation uses *Food Desert Locator* data to analyze the relationship between the concentration of food deserts and obesity using county-level data. In so doing, this study also evaluates the utility of the food desert categorization scheme created by the HFFI.

Additional Obesity Factors

Food deserts are one aspect of what public health researchers describe as the "built" food environment (Wright and Aronne 2012). The built food environment encompasses the structural aspects of the food retail market that encourage overeating such as increased portion size, increased calorie and fat content in foods, increased marketing of processed food products, and the food offerings at retail stores (Wright and Aronne 2012).

In addition to food deserts, there are several other factors that have been found to be associated with obesity. Notably, general health has several potential impacts on physical activity, which is closely related to obesity. Exercise is more difficult for people who are afflicted with incapacitating diseases. Heart disease, stroke, diabetes, and some forms of cancer are comorbid with obesity (Grundy 2000). Many of these conditions are likely mutually reinforcing (Ells et al. 2006). Furthermore, disability status and obesity are correlated. Thirty-six percent of adults with disabilities are obese, while 23 percent of adults without disabilities are obese (Centers for Disease Control 2012b). A number of common medications are also associated with increased weight gain (Wright and Aronne 2012). I control for general health factors in my model using three variables: mortality rate, years of potential life lost, and percentage of residents who report fair or poor health. Further description of these variables can be found in the data and methods section.

Demographic factors are also related to obesity rates. Economic circumstances can limit resources that could be devoted to purchasing and preparing healthy food, as healthy diets tend to be more expensive (Drewnowski and Specter 2004). Education levels have also been found to be correlated with obesity (Drewnowski and Darmon 2005). However, it is unclear whether this relationship is attributable to the income and social status benefits of additional education or if educational attainment directly improves dietary and exercise choices (Tai-Seale and Chandler 2010).

IV. CONCEPTUAL FRAMEWORK

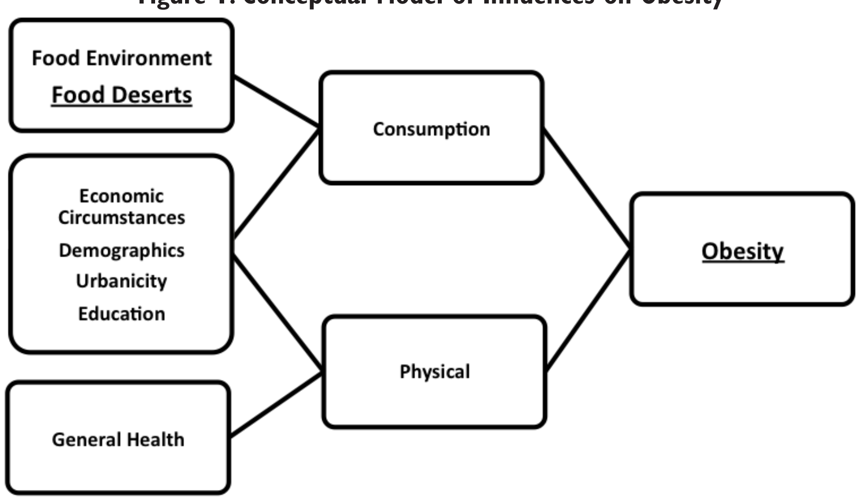
To understand the drivers of obesity rates at a macro level, factors related to individual weight gain must first be considered. The basic mechanism underlying weight gain is well understood: individuals gain weight when their calorie intake exceeds their calorie expenditure (Finkelstein, Ruhm, and Kosa 2005). However, reasons for calorie imbalance can be varied and diffuse. Any explanation of the causes of obesity must account for variation in calorie intake (called "consumption" in Figure 1 for simplicity) and calorie expenditure (called "physical activity" in Figure 1). The factors that influence obesity outlined in the literature review are diagramed in Figure 1. Controlling for these factors helps to reduce omitted variable bias in my regression analyses.

V. DATA AND METHODS

In this study, I estimate the association between county-level factors and obesity rates, focusing primarily on food deserts. In order to conduct this investigation, I used a cross-sectional county-level analysis. My data set contains information on 3,138 US counties and accounts for every county in the United States, with the exception of six counties for which data were not available.

A primary source of data for this study is the HFFI *Food Desert Locator*, maintained by the USDA's Economic Research Service. This dataset provides the numerator for my independent variable of interest, the number of people in a given county who live in a food desert. This variable was constructed by the HFFI using 2000 Census data on population

Figure 1. Conceptual Model of Influences on Obesity



characteristics and Supplemental Nutrition Assistance Program (SNAP) data on grocery store locations for the year 2006. Though these data are provided by the USDA at the census tract level, in order to match the observation level of my dependent variable, I sum the population of people living in food desert census tracts for each county and divide that sum by the population of the county.[6] Note that population and grocery store location measures within the *Food Desert Locator* are taken from different years. Since the availability of grocery stores is the most important dimension of the *Food Desert Locator*, whenever possible I use data from 2006 to measure the other variables in my model. The dependent variable for my regressions is the county-level obesity rate, taken from the Centers for Disease Control (CDC)'s *Diabetes Data and Trends* database for 2006.

Since rural residents tend to suffer from obesity more than urban residents (Tai-Seale and Chandler 2010), and the *Food Desert Locator* uses different standards of classification for rural and urban areas, I include a dummy variable for metropolitan counties in my regressions as a control.[7] This dummy variable is based on the USDA's most recent Rural-Urban Continuum codes from 2003. For simplicity, I took nine categories the USDA uses and collapsed them into a binary variable.[8]

To capture calorie expenditure, I include two control variables in the model: rate of physical inactivity and number of recreational facilities per person. The physical inactivity rate comes from the CDC's *Diabetes Data and Trends* for 2006. This variable reflects the proportion of negative responses to the question: "In the past month, outside of your regular job, have you participated in any physical activities or exercises such as running, calisthenics, golf, gardening, or walking for exercise?" The number of recreational facilities per person is taken from the USDA's *Food Environment Atlas* and reflects the number of establishments in the county that are primarily devoted to physical activity as defined by North American Industry Classification System (NAICS) code. While this variable does not capture how many people the facilities serve or how frequently, it gives an approximation of availability.

To capture other aspects of the food environment that are not included in the *Food Desert Locator*, I add the number of fast food restaurants per person and fast food expenditures per person as measured by the *Food Environment Atlas* (USDA 2012).[9] The *Food Environment Atlas* only provides the fast food expenditure data at the state level, so this aspect of the analysis is more vulnerable to measurement

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[6] The population figures used were census data averaged between the year 2000 and the year 2010.

[7] I also use this variable to divide the sample for a stratified analysis as shown in Table 2.

[8] Consistent with the Office of Management and Budget's delineation between metro and non-metro areas (USDA ERS, 2012), I collapsed codes one through three into the metro category and codes four through nine into the non-metro category.

[9] As these variables are highly correlated with the *Food Desert Locator*, I conducted a sensitivity test by estimating my model both with and without these variables.

error. Due to the high correlation of poverty and obesity (Chang and Lauderale 2005), I add the median income and the unemployment rate, averaged between the 2000 and 2010 Censuses, as control variables.

County-level controls for general health include the mortality rate, years of potential life lost (YPLL), and the percentage of residents who are in fair or poor health from the County Health Rankings and Roadmaps program.[10] The mortality rate is the number of deaths in the year 2011 for each county divided by its population. The YPLL variable is a measure of mortality where deaths occurring at younger ages are given greater weight to better capture premature deaths. The YPLL is generated by subtracting the age at which each death occurs from 75 (County Health Rankings and Roadmaps 2012). The fair or poor health measure reflects the percentage of residents who responded to a telephone survey, conducted by the CDC, by saying that, in general, their health is either "fair" or "poor" on a four-point scale.

I also use Census data to control for educational attainment. Education level is aggregated into four categories: "less than high school diploma," "high school diploma or equivalent," "some college," and "bachelor's degree or higher." These variables are expressed as percentages of the population age 25 and above, averaged between the 2000 and 2010 Censuses.

Since obesity rates vary systematically by age, I control for the median age of each county in the model (Mendez 2010). This variable is based on data averaged between the 2000 and 2010 Censuses.

I also include demographic controls for race in the model. Obesity rates in the US are consistently higher among blacks and Hispanics (Paeratakul et al. 2002; Cossrow and Falkner 2004), even when controlling for age, marital status, gender, employment, income, education, and region (Mendez, Newport, and McGeeney 2012). I construct race controls by averaging data from the 2000 and 2010 Censuses on the percentage of residents in each county who report being white, black or African American, another race, and Hispanic (regardless of race). Table 1 displays all the variables described above.

[10] The County Health Rankings and Roadmaps program is a non-profit collaboration between the Robert Wood Johnson Foundation and the University of Wisconsin Population Health Institute.

Table 1: Summary of Variables			
Variable	**Short Name**	**Source and Year**	**Unit of Measure**
Dependent Variable			
Obesity Rate	*Obesity*	CDC, 2006	The percentage of people with a BMI of 30 or greater living within a county during the year 2006.
Key **Independent Variable**			
Percent of People Living in a Food Desert	*Food Desert*	USDA Food Desert Locator, Mixed years	The number of people in each county living in census tracts that meet the USDA's definition of a food desert, divided by the county's population.
Urbanicity			
Metro Dummy (1= metro 0= nonmetro)	*Metro*	USDA, 2003	A dummy variable signifying whether the county is metropolitan (contains a census metropolitan statistical area) or non-metropolitan.
Physical Activity			
Rate of Physical Inactivity	*Physical Inactivity*	CDC, 2006	The percentage of people who report having no physical activity in the county.
Number of Recreational Facilities per 10,000 People	*Recreation*	Food Environment Atlas, averaged 2007 & 2009	Number of recreational facilities in the county per ten thousand county residents.
Food Environment			
Number of Fast Food Restaurants per Person[11]	*Fast Food Restaurants*	Food Environment Atlas, averaged 2007 & 2009	Number of fast food establishments in each county per ten thousand county residents.
Fast Food Expenditures per Capita	*Fast Food Expenditures*	Food Environment Atlas, averaged 2007 & 2009	State-level fast food expenditures per person.
Economic Factors			
Median Income	*Median Income*	Census data averaged for 2000 & 2010	County-level median income.
Unemployment Rate	*Unemployment*	Census data averaged for 2000 & 2010	Percent of people in the county labor force without a job.
General Health			
Mortality Rate[12]	*Mortality*	County Health Rankings, 2011	Number of deaths in the county for the year 2011 per thousand county residents.
Years of Potential Life Lost	*YPLL*	County Health Rankings, 2011	Number of deaths weighted to emphasize premature deaths. The measure is created by subtracting the age at which death occurs from 75.
Percent with "Fair" or "Poor" Health	*Fair/Poor Health*	County Health Rankings, 2011	Percent of county residents that self-report fair/poor health.

Table 1 Continued			
Variable	**Short Name**	**Source and Year**	**Unit of Measure**
Demographics			
Percent with less than high school diploma	*High School*	Census data averaged 2000 & 2010	The percentage of people in the county over 25 who have not completed high school or an equivalent (ex: General Education Development diploma).
Percent with High School Diploma or Equivalent	*Less than High School*	Census data averaged 2000 & 2010	The percentage of people in the county over 25 with a high school diploma or the equivalent and nothing more.
Percent with Some College	*Some College*	Census data averaged 2000 & 2010	The percentage of people in the county over 25 who have completed some college (including Associates degree holders) but do not have a four-year degree.
Percent with a BA or Higher	*BA Plus*	Census data averaged 2000 & 2010	The percentage of people in the county over 25 who hold a Bachelor's degree or higher.
Median Age	*Age*	Census data averaged 2000 & 2010	Median age in the county.
Percent White	*White*	Census data averaged 2000 & 2010	The percentage of people living in the county who self-report as White.
Percent Black	*Black*	Census data averaged 2000 & 2010	The percentage of people living in the county who self-report as Black.
Percent Other Race	*Other Race*	Census data averaged 2000 & 2010	The percentage of people living in the county who self-report to be a race other than Black or White (category includes Native Alaskan or American Indian, Asian, American Indian, Pacific Islander, another race, or two or more races).
Percent Hispanic (Any Race)	*Hispanic*	Census data averaged 2000 & 2010	The percentage of people living in the county who self-report as Hispanic regardless of race.

[11] This variable is at the state level.
[12] Mortality rate adjusted to give more weight to deaths occurring at younger ages.

Table 2. Summary Statistics Disaggregated by Food Desert Presence				
Variable		All Counties	Counties with Food Desert(s)	Counties without Food Desert(s)
Number of Observations		3,138	1,847	1,291
Variable of Interest				
People Living in a Food Desert per 10,000 County Residents	Mean	0.678	1.153	0
	s^2	1.805	2.234	0
	Min	0	0.0067	0
	Max	31.153	31.153	0
Dependent Variable				
Obesity Rate	Mean	27.46	27.42	27.51
	s^2	3.57	3.76	3.28
	Min	12.6	12.7	12.6
	Max	41.9	41.8	41.9
Urbanicity				
Non-Metro Dummy	Mean	0.653	0.617	0.704
(1= non-metro, 0=metro)	s^2	0.476	0.486	0.457
	Min	0	0	0
	Max	1	1	1
Physical Activity				
Physical Inactivity Rate	Mean	25.51	25.27	25.86
	s^2	5.28	5.21	5.37
	Min	9.4	9.4	11.4
	Max	43.8	43.4	43.8
Number of Recreational Facilities per 10,000 people	Mean	0.87	0.86	0.88
	s^2	0.87	0.78	1
	Min	0	0	0
	Max	13.78	9.98	13.78
Food Environment				
Number of Fast Food Restaurants per 10,000 People	Mean	5.92	5.96	5.86
	s^2	3.12	3	3.29
	Min	0	0	0
	Max	63.64	63.64	37.9
Fast Food Expenditures per Capita	Mean	641.78	644.05	638.55
	s^2	96.68	96.89	96.32
	Min	402.1	402.1	402.1
	Max	1043.86	1043.86	1036.48
General Health				
Deaths per 1,000 people	Mean	12.9	12.96	12.81
	s^2	3.42	3.38	3.47
	Min	0	4.36	0
	Max	30.09	30.09	26.43

Table 2 Continued				
Variable		**All Counties**	**Counties with Food Desert(s)**	**Counties without Food Desert(s)**
Years of Potential Life Lost Per Person	Mean	8382	8517.9	8187.6
	s^2	2491.5	2485	2488.8
	Min	0	2794.9	0
	Max	24829.4	24829.4	23605
Percent with "Fair/Poor" Health[14]	Mean	17.09	17.34	16.71
	s^2	5.7	5.43	6.04
	Min	2.1	3.5	2.1
	Max	44.8	40.7	44.8
Economic Factors				
Median Income	Mean	39,820	38,929	41,096
	s^2	10,076	9,814	10,309
	Min	17,578	18,223	17,578
	Max	98,111	93,233	98,111
Unemployment Rate	Mean	5.48	5.8	5.03
	s^2	2.18	2.19	2.09
	Min	0	0	0
	Max	20.6	20.6	20.25
Demographics				
Less than High School Diploma	Mean	18.12	18.32	17.83
	s^2	7.64	7.52	7.81
	Min	2.02	4.43	2.02
	Max	55.73	55.73	45.73
High School Diploma	Mean	34.83	34.02	35.98
	s^2	6.73	6.76	6.52
	Min	9.75	11.98	9.75
	Max	55.81	52.69	55.81
Some College	Mean	28.61	28.81	28.33
	s^2	5.54	5.45	5.65
	Min	12.23	12.76	12.23
	Max	48.43	48.43	45.04
Bachelor's Degree or Higher	Mean	18.44	18.85	17.86
	s^2	8.37	8.46	8.22
	Min	5.14	5.62	5.14
	Max	69.32	57.31	69.32
Median Age	Mean	38.84	38.42	39.43
	s^2	4.47	4.75	3.97
	Min	20.95	21.65	20.95
	Max	56.95	55.1	56.95

Table 2 Continued				
Variable		**All Counties**	**Counties with Food Desert(s)**	**Counties without Food Desert(s)**
Percent White	Mean	83.66	81.06	87.38
	s^2	16.65	17.34	14.82
	Min	3.7	3.7	3.7
	Max	99.3	99.3	99.2
Percent Black	Mean	8.83	10.49	6.46
	s^2	14.49	15.74	12.09
	Min	0	0	0
	Max	86.1	86.1	81.05
Percent Other Race	Mean	7.5	8.44	6.15
	s^2	10.2	10.52	9.58
	Min	0.45	0.45	0.7
	Max	96.2	96.2	96.15
Percent Hispanic (regardless of race)	Mean	7.24	8.45	5.52
	s^2	12.55	14.21	9.45
	Min	0.1	0.1	0.2
	Max	96.6	96.6	91.3

Using these variables, I estimate the following model:[13]

$$Obesity\ Rate = \beta_0 + \beta_1 Food\ Desert + \beta_2 Metro + \beta_3 Physical\ Inactivity + \beta_4 Recreation + \beta_5 Mortality + \beta_6 YPLL + \beta_7 Fair/Poor\ Health + \beta_8 Fast\ Food\ Restaurants + \beta_9 Fast\ Food\ Expenditures + \beta_{10} Unemployment + \beta_{11} Median\ Income + \beta_{12} Less\ than\ HS + \beta_{13} HS + \beta_{14} BA\ Plus + \beta_{15} Age + \beta_{16} White + \beta_{17} Black + \beta_{18} Hispanic + u$$

Because the dependent variable is a continuous measure and my data are cross-sectional, I use ordinary least squares to estimate my regression model. Inclusion of the control variables specified above reduces the

[13] The variables "Some College" and "Other Race" are omitted from the model in order to avoid multicollinearity and serve as the reference groups for educational attainment and race, respectively.

[14] Data on the years of potential life lost and percentage of residents reporting fair or poor health were missing for 401 counties, and data on the number of deaths per 1000 people were missing for 95 counties. I imputed values for these missing variables in order to include these counties in my regression. I performed imputations by regressing each variable with missing values on the obesity rate, physical inactivity rate, poverty rate, median income, unemployment rate, education variables, percent age 60 or over, median age, percent white, percent black, and percent married, and I used the results of the regressions to produce predicted values for the missing data. A t-test showed that the mean of these imputed values was significantly different from the non-imputed values. Furthermore, a series of t-tests showed that there were significant differences in population size, median age, obesity rates, poverty rates, and racial composition between counties for which years of potential life lost were imputed and counties for which years of potential life lost were not imputed. See Appendix E for a representative selection of these analyses. I also conducted a sensitivity test by performing the regression analysis with and without the imputed values, and the results for the key independent variable were similar in both samples. See Appendix C for the results of the regression without imputed data.

amount of omitted variable bias in my estimate of the relationship between food deserts and obesity rates.

VI. DESCRIPTIVE ANALYSIS

Table 2 presents summary statistics for the variables included in the regression model, disaggregated according to whether counties contain food deserts. Counties with one or more food deserts account for 1,847 of the 3,138 counties, or 59 percent, used in this analysis. Contrary to expectation, obesity rates are similar between the two groups. Using a simple correlation analysis, the results of which are reported in Appendix A, I find that there is a small but statistically significant correlation between obesity rates and food desert intensity ($r = 0.0728$; $p=0.0000$).

Additionally, average values for the control variables for physical activity are very similar between the two groups. The rate of self-reported physical inactivity differs by only 0.59 percentage points between counties with food deserts and those without. The number of recreational facilities per 10,000 people is similar, but counties without food deserts have a higher variance.

The variables measuring general health differ a bit more between counties with food deserts and those without. The most striking difference is between the Years of Potential Life Lost: residents of counties containing food deserts tend to die at younger ages. Additionally, the number of residents who self-report being in fair or poor health is also higher in counties containing food deserts.

Counties without food deserts have higher median incomes and lower rates of poverty, unemployment, and food insecurity. Differences in education rates between the two groups of counties are almost non-existent. Racial composition differs notably between counties with food deserts and those without. Non-white racial groups disproportionately live in counties containing food deserts and whites disproportionately live in counties without any food deserts. Furthermore, the results reported in Appendix A show statistically significant correlations between food desert intensity and each of the racial composition variables. This finding is consistent with the results of other studies on the racial dynamics of food deserts (Raja et al. 2008).

VII. REGRESSION RESULTS

I estimated five ordinary least squares (OLS) regressions of the county-level obesity rate on the percentage of county residents living in a food desert and the aforementioned county-level controls. Table 3 displays the results of these regressions. The first model is the simplest, controlling for urbanicity, general health, education rate, and demographic factors. Model one is estimated without controls for median income and the unemployment rate because they are, to some extent, mechanically correlated with the food desert measure. However, since the correlation between poverty and

Table 3. OLS Regression Results					
	(1)	(2)	(3)	(4)	(5)
Variables	All Counties	All Counties	All Counties	Metro Counties	Non-metro Counties
Variable of Interest					
Food Desert Rate	0.0214***	0.0224***	0.0222***	0.0358**	0
	(0.006)	(0.006)	(0.006)	(0.015)	(0.003)
Urbanicity					
Non-metro	-0.2728*	-0.2678*	-0.0671		
	(0.148)	(0.156)	(0.156)		
Physical Activity					
Physical Inactivity			0.2251***	0.2272***	0.2077***
			(0.032)	(0.039)	(0.021)
Recreational Facilities			-0.3561**	-0.6758**	-0.1635*
per 10,000 Residents			(0.175)	(0.322)	(0.085)
Food Environment					
Fast Food Restaurants			0.0176	0.0649	0.0117
per 10,000 Residents			(0.046)	(0.071)	(0.026)
Fast Food Expenditures			-0.0001	0.0001	-0.0057***
per Capita			(0.001)	(0.001)	(0.001)
Economic Factors					
Unemployment Rate		-0.0803	0.0654	0.0603	0.0753**
		(0.075)	(0.074)	(0.121)	(0.037)
Mean Income in Thousands		0.0053	0.003	0.0139	-0.0374**
		(0.012)	(0.013)	(0.015)	(0.015)
General Health					
Deaths per 1,000 Residents	0.0425	0.0592	-0.0499	-0.058	-0.0102
	(0.120)	(0.121)	(0.118)	(0.207)	(0.069)
Years of Potential Life Lost	0	0	0.0001	0	-0.0001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Fair/Poor Health Rate	0.0726**	0.0777***	-0.0145	-0.0326	0.0498***
	(0.029)	(0.028)	(0.029)	(0.046)	(0.018)
Demographics					
Less than High School	-0.0054	-0.007	-0.0448	-0.0055	-0.0557***
	(0.030)	(0.030)	(0.030)	(0.045)	(0.018)
High School	0.032	0.0297	-0.0491*	-0.0461	-0.0375**
	(0.023)	(0.023)	(0.025)	(0.039)	(0.017)
BA Plus	-0.1972***	-0.2066***	-0.2203***	-0.2175***	-0.2025***
	(0.023)	(0.026)	(0.025)	(0.032)	(0.024)
Median Age	-0.3351***	-0.3440***	-0.2705***	-0.3078***	-0.1194***
	(0.042)	(0.045)	(0.047)	(0.071)	(0.024)

Table 3 Continued					
	(1)	(2)	(3)	(4)	(5)
Variables	All Counties	All Counties	All Counties	Metro Counties	Non-metro Counties
Percent White	0.0740***	0.0728***	0.0687***	0.0878***	-0.0204**
	(0.012)	(0.012)	(0.014)	(0.017)	(0.009)
Percent Black	0.1247***	0.1267***	0.1003***	0.1092***	0.0632***
	(0.014)	(0.014)	(0.014)	(0.020)	(0.009)
Percent Hispanic	-0.0975***	-0.0970***	-0.0845***	-0.0921***	-0.0591***
	(0.016)	(0.016)	(0.016)	(0.021)	(0.008)
Constant	34.0692***	34.6485***	32.5298***	31.4318***	39.5540***
	(2.405)	(2.551)	(3.181)	(4.074)	(1.939)
Observations	3,138	3,138	3,138	1,090	2,048
R-squared	0.739	0.74	0.759	0.762	0.676

Robust standard errors are in parentheses. Significance: * p<0.10, ** p<0.05, *** p<0.01

obesity has been well established, leaving these variables out of the model may cause omitted variable bias. The first two models also omit the control variables for physical activity and food environment. These categories are omitted in part as a basic robustness check and in part due to the fact that the number of recreational facilities per 10,000 people and the number of fast food restaurants per 10,000 people vary strongly by metropolitan status, which is a key component of the food desert measure.[15] The third model adds physical activity and food environment control variables. The fourth and fifth models include the same variables as model three but divide the sample into metro and non-metro counties. I estimate each of these models with analytic weights for county population size, and I report robust standard errors for all coefficients.

In the first three models, the coefficient on the percentage of county residents living in a food desert per county is positive, of consistent magnitude, and statistically significant at the 99 percent level. This finding supports my hypothesis that the prevalence of food deserts has a positive relationship with obesity rates by county. It is important to note that the inclusion of control variables for economic factors, physical activity, and food environment have no meaningful impact on the coefficient of interest, which provides evidence for the robustness of the results. However, the magnitude of the coefficient is quite small. In models one through three, an increase of one percentage point in the proportion of county residents living in a food desert is associated with an increase of about 0.02 percentage points in the obesity rate. Since the average obesity rate across all counties

[15] Appendix D shows t-tests for the means of the variables measuring the number of recreational facilities per 10,000 people and the number of fast food restaurants per 10,000 people. Both variables differ significantly between metro and non-metro areas (p<0.0001), with metro areas having higher concentrations of both types of establishments per person.

is about 27 percent, this estimated relationship has little meaningful impact on the prevalence of obesity.

The results reported in columns four and five—in which the sample is divided into metro and non-metro counties—add greater insight. In the sample of metro counties, the coefficient on the percentage of county residents living in a food desert per county increases to 0.0358 and remains statistically significant. Meanwhile, the main coefficient of interest in the model of non-metro counties falls to less than 0.0001 and is not statistically significant.

VIII. SENSITIVITY TESTS

When the model is estimated without weights as reported in Appendix B, the estimated coefficients on the independent variable of interest are reduced in significance and magnitude. This is likely another reflection of the differences between metro and non-metro areas. In the weighted model, less populous counties, which are more likely to be non-metro, would be counted more.[16] As shown in models four and five in Table 3, the magnitude of the relationship between food deserts and obesity is weaker in non-metro counties. A sensitivity test of the influence of my missing data imputations on the results can be found in Appendix C. When observations with imputed data are removed from the sample, the main coefficient of interest remains significant and retains the same sign, but increases slightly in magnitude.

There are three key takeaways from these regression results. First, the main coefficient of interest is positive, robust, and significant. Second, the magnitude of this coefficient is small, which has implications for the central hypothesis of this thesis. Third, the food desert measure is more predictive of obesity rates in metro areas than in non-metro areas. These three findings each have relevant implications for policymaking, which are elaborated upon in the next section.

IX. DISCUSSION

The results of this study suggest that food desert intensity is unpredictive of obesity rates in non-metro counties and only very slightly predictive of obesity rates in metro counties. In metro counties, the regression models show that an increase of one percentage point in the proportion of county residents living in a food desert is associated with less than a tenth of a percentage point decrease in the obesity rate. Since the average obesity rate in the United States is about 30 percent, this finding suggests that food desert intensity does not impact obesity rates in a meaningful way.

These findings also have implications for the secondary purpose of this study, which is to evaluate the usefulness of the United States Department of Agriculture (USDA) *Food Desert Locator* as a metric. As one of the first nationwide food desert classification

[16] Appendix D displays a t-test of the statistical difference between populations in metro and non-metro counties.

schemes, the *Food Desert Locator* will influence the way policymakers, scholars, and the public think about food deserts. This measure will also play a role in the distribution of millions of dollars of grants. The insubstantial county-level correlation between the concentration of food deserts and obesity rates casts some doubt on the usefulness of the measure. While predicting obesity rates is not the only purpose of identifying food deserts, it is one of the central outcomes relevant for policy change.

The findings of this study should, however, be appreciated in context of its limitations. Although the *Food Desert Locator* is classified at the census tract level, I am compelled to aggregate the food desert data to the county level because that is the observation level of the obesity data. Therefore, I can evaluate the relationship between food desert intensity and obesity rates, but I cannot directly compare obesity rates between food deserts and non-food deserts. A study conducted at the census tract level rather than at the county level would produce more precise results.

Although the model of obesity presented in this paper outlines some of the main contributors to obesity rates, many of the concepts included are difficult to measure precisely. The proxies included in the model are the best data available to measure these factors, but they are not exact reflections of the dynamics that they are intended to measure. For example, the number of recreational facilities per 10,000 people is used as a proxy for physical fitness levels in each county. However, this variable is influenced by income levels, the real estate market, and urban planning strategies. The same could be said for the number of fast food restaurants per 10,000 people.

In addition, some food desert experts might argue that the research question at hand is overly ambitious. Obesity is influenced by a number of factors, including physical activity, general health, and genetics, all of which are difficult to measure and control for in a regression model. Obesity would be a more distal effect of the presence of food deserts than other concerns such as the healthfulness of food consumed and the amount of time spent procuring healthy food. These proximate effects of living in a food desert also have policy relevance, and their relationships with food desert presence might be easier to observe—especially in the short run—than second-order health outcomes such as obesity. This investigation seeks to understand the relationship between these two factors due to steadily climbing obesity rates and the health and policy problems this phenomenon creates, but a study of the more proximate predicted effects of food deserts might yield more robust correlations.

Despite these limitations, there are a number of ways in which the findings of this study can inform policymaking with regard to food access problems. Even if food deserts were definitively found to have no influence on obesity rates, systematic low access to healthy food would still pose a policy problem.

There are a number of other health outcomes that are affected by a poor diet, including diabetes, heart disease, and stroke. The fact that low-income individuals disproportionately suffer from the problem of limited food access, even when poverty measures are not included in food desert measures, is an indication that food deserts deserve further attention.

The most fundamental recommendation for further research involves the testing and improvement of food desert classification systems. The differing results for metro and non-metro areas in this study provide a good starting point for further investigation of food desert measure validity. Though the *Food Desert Locator* has different threshold distances from grocery stores for rural and urban areas, the USDA should consider amending these thresholds or adding another dynamic to the measure that varies between rural and urban areas. A more thorough food retail store classification system, like the one that Raja et al. (2008) use, could provide a model for more precisely capturing differences in food retail environments between rural and urban areas.

A valid, standardized, and nationwide food desert classification system would help to make regional academic studies of food deserts more comparable and would serve as a critical tool for policymakers and grant writers seeking to address the problem of limited food access. The creation of the *Food Desert Locator* was a valuable step forward in creating a useful and uniform metric of food deserts nationwide. Further investigation and improvement of this database would increase understanding of the phenomenon of food deserts and would better guide efforts to solve the problems that they create.

Appendix A. Bivariate Analysis	
Variable	**Correlation with Percent of People Living in a Food Desert**
Obesity Rate	0.0728
	(0.0000)
Physical Inactivity Rate	0.0214
	(0.2312)
Mortality Rate	0.1536
	(0.0000)
Years of Potential Life Lost	0.1609
	(0.0000)
Median Income	-0.3385
	(0.0000)
Unemployment Rate	-0.0617
	(0.0005)
Percent White	-0.1123
	(0.0000)
Percent Black	0.0711
	(0.0001)
Percent Hispanic	0.075
	(0.0000)
Percent Other Race	0.0824
	(0.0000)

Appendix B. Unweighted Regression Results					
Variables	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
	All Counties	**All Counties**	**All Counties**	**Metro Counties**	**Non-metro Counties**
Variable of Interest					
Food Desert Rate	0.0022	0	0.0016	-0.0022	0.0012
	(0.002)	(0.002)	(0.002)	(0.005)	(0.002)
Urbanicity					
Non-metro	-0.0757	-0.2445**	-0.1191		
	(0.100)	(0.108)	(0.097)		
Physical Activity					
Physical Inactivity			0.2625***	0.2225***	0.2642***
			(0.014)	(0.025)	(0.016)
Recreational Facilities			-0.1469***	-0.2854	-0.1386**
per 10,000 Residents			(0.055)	(0.175)	(0.056)

Appendix B Continued					
Variables	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Food Environment					
Fast Food Restaurants			-0.0152	0.1042**	-0.0347**
per 10,000 Residents			(0.018)	(0.050)	(0.015)
Fast Food Expenditures			-0.0057***	-0.0032***	-0.0074***
per Capita			(0.001)	(0.001)	(0.001)
Economic Factors					
Unemployment Rate		-0.0586**	0.0362	-0.0798	0.0451*
		(0.027)	(0.025)	(0.062)	(0.027)
Mean Income in Thousands		-0.0273***	-0.0081	0.0004	-0.0051
		(0.007)	(0.007)	(0.010)	(0.011)
General Health					
Deaths per 1,000 Residents	0.0044	-0.0012	0.0308	0.2565**	0.0159
	(0.035)	(0.036)	(0.035)	(0.100)	(0.036)
Years of Potential Life Lost	0.0001**	0.0001*	0	-0.0004***	0
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Fair/Poor Health Rate	0.0779***	0.0789***	0.0095	0.0003	0.0303**
	(0.012)	(0.013)	(0.012)	(0.023)	(0.014)
Demographics					
Less than High School	0.001	0.0032	-0.0498***	-0.0454**	-0.0482***
	(0.012)	(0.012)	(0.011)	(0.021)	(0.014)
High School	0.0439***	0.0476***	-0.0314***	-0.0186	-0.0325**
	(0.012)	(0.012)	(0.011)	(0.022)	(0.013)
BA Plus	-0.1632***	-0.1420***	-0.1726***	-0.1872***	-0.1657***
	(0.014)	(0.015)	(0.013)	(0.021)	(0.019)
Median Age	-0.1698***	-0.1667***	-0.1260***	-0.2257***	-0.0964***
	(0.015)	(0.016)	(0.015)	(0.039)	(0.016)
Percent White	-0.0022	-0.0109	-0.0135**	0.0579***	-0.0263***
	(0.007)	(0.008)	(0.007)	(0.015)	(0.007)
Percent Black	0.0719***	0.0666***	0.0568***	0.1312***	0.0514***
	(0.007)	(0.008)	(0.007)	(0.015)	(0.007)
Percent Hispanic	-0.0841***	-0.0851***	-0.0504***	-0.0343***	-0.0433***
	(0.006)	(0.006)	(0.005)	(0.011)	(0.006)
Constant	33.4480***	35.5540***	35.7437***	31.4209***	36.1932***
	(1.198)	(1.326)	(1.289)	(2.577)	(1.456)
Observations	3,138	3,138	3,138	1,090	2,048
R-squared	0.604	0.607	0.676	0.694	0.676

Robust standard errors are in parentheses. Significance: * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Appendix C. Regression Results Without Imputed Data					
Variables	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
	All Counties	**All Counties**	**All Counties**	**Metro Counties**	**Non-metro Counties**
Variable of Interest					
Food Desert Rate	0.0249***	0.0262***	0.0252***	0.0363**	0
	(0.007)	(0.007)	(0.007)	(0.016)	(0.003)
Urbanicity					
Non-metro	-0.3120**	-0.2978*	-0.0977		
	(0.151)	(0.159)	(0.160)		
Physical Activity					
Physical Inactivity			0.2218***	0.2255***	0.2077***
			(0.032)	(0.040)	(0.021)
Recreational Facilities			-0.3816**	-0.6846**	-0.1635*
per 10,000 Residents			(0.187)	(0.329)	(0.085)
Food Environment					
Fast Food Restaurants			0.0064	0.0536	0.0117
per 10,000 Residents			(0.049)	(0.074)	(0.026)
Fast Food Expenditures			0	0.0001	-0.0057***
per Capita			(0.001)	(0.001)	(0.001)
Economic Factors					
Unemployment Rate		-0.0761	0.0708	0.0581	0.0753**
		(0.078)	(0.077)	(0.121)	(0.037)
Mean Income in Thousands		0.0068	0.0046	0.0142	-0.0374**
		(0.012)	(0.013)	(0.015)	(0.015)
General Health					
Deaths per 1,000 Residents	0.0518	0.0695	-0.0399	-0.0472	-0.0102
	(0.126)	(0.128)	(0.125)	(0.213)	(0.069)
Years of Potential Life Lost	0	0	0.0001	0	-0.0001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Fair/Poor Health Rate	0.0721**	0.0767***	-0.0127	-0.0313	0.0498***
	(0.029)	(0.029)	(0.029)	(0.046)	(0.018)
Demographics					
Less than High School	0.0003	-0.0012	-0.0407	-0.0029	-0.0557***
	(0.031)	(0.031)	(0.031)	(0.045)	(0.018)
High School	0.0325	0.0303	-0.0477*	-0.0446	-0.0375**
	(0.024)	(0.024)	(0.026)	(0.040)	(0.017)
BA Plus	-0.1970***	-0.2076***	-0.2189***	-0.2157***	-0.2025***
	(0.024)	(0.026)	(0.026)	(0.032)	(0.024)
Median Age	-0.3372***	-0.3475***	-0.2743***	-0.3116***	-0.1194***
	(0.044)	(0.047)	(0.049)	(0.072)	(0.024)
Percent White	0.0728***	0.0722***	0.0689***	0.0880***	-0.0204**
	(0.012)	(0.012)	(0.014)	(0.017)	(0.009)

Appendix C Continued					
Variables	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Percent Black	0.1227***	0.1249***	0.0990***	0.1089***	0.0632***
	(0.014)	(0.014)	(0.014)	(0.020)	(0.009)
Percent Hispanic	-0.1010***	-0.1004***	-0.0876***	-0.0931***	-0.0591***
	(0.016)	(0.016)	(0.017)	(0.021)	(0.008)
Constant	34.2293***	34.7067***	32.4938***	31.4620***	39.5540***
	(2.448)	(2.592)	(3.275)	(4.120)	(1.939)
Observations	2,737	2,737	2,737	1,035	2,048
R-squared	0.741	0.742	0.761	0.762	0.676

Robust standard errors are in parentheses. Significance: * p<0.10, ** p<0.05, *** p<0.01

Appendix D. Statistical Tests By Metro Status

Recreational Facilities per 10,000 Residents

Group	N	Mean	Standard Error	Standard Deviation
Metro	1090	0.985	0.0181	0.5975
Non-metro	2048	0.81	0.0217	0.9837
Summary				
t=5.3877 Degrees of Freedom=3136 p=0.0000				

Fast Food Restaurants per 10,000 Residents

Group	N	Mean	Standard Error	Standard Deviation
Metro	1090	6.495	0.0804	2.656
Non-metro	2048	5.608	0.073	3.303
Summary				
t=7.6475 Degrees of Freedom=3136 p=0.0000				

Population Size

Group	N	Mean	Standard Error	Standard Deviation
Metro	1090	224862	14721.97	486048
Non-metro	2048	24378	529.3505	23955.67
Summary				
t=18.6268 Degrees of Freedom=3136 p=0.0000				

Appendix E. Imputed Data T-tests

Metro versus Non-metro

Group	N	Mean	Standard Error	Standard Deviation
Non-Missing	2737	0.6218	0.0093	0.4850
Missing	401	0.8628	0.0172	0.3444
Summary				
t=9.6010 Degrees of Freedom=3136 p=0.0000				

Population Size

Group	N	Mean	Standard Error	Standard Deviation
Non-Missing	2737	106083	6157.013	322112.4
Missing	401	11664	504.1912	10096.42
Summary				
t=5.8686 Degrees of Freedom=3136 p=0.0000				

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The Relationship Between Low-Skilled Unemployment Rates and SNAP Participation

By Catlin N. Nchako

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Abstract

The Supplemental Nutrition Assistance Program (SNAP) is designed to operate counter-cyclically, with participation rising as the economy contracts. The growth in program costs and participation has raised political concerns over whether the program serves truly needy individuals. This study tests the sensitivity of SNAP participation to the unemployment rates of low-skilled individuals, and shows a statistically significant, positive, yet reasonably small correlation. This analysis also finds that the increase in participation becomes larger as unemployment rises and lags behind unemployment. These results suggest the need for caution among policymakers in reaching hasty conclusions about the utility of the program.

I. INTRODUCTION

The Supplemental Nutrition Assistance Program (SNAP), formerly called the Food Stamp Program (FSP), provides benefits to low-income individuals to enable them to purchase food.[1] SNAP is administered by the United States Department of Agriculture (USDA). In an average month in 2012, it served 46.6 million people (USDA 2013). SNAP plays a critical role in reducing poverty; if SNAP benefits were counted in the official poverty measure in 2010, they would have lifted 3.9 million people out of poverty (USDA 2012).

Despite its positive attributes, there is nonetheless a political debate over whether the program contributes to dependence on government welfare. As federal spending on SNAP totaled $80 billion in 2012 (US Dept. of the Treasury 2012), the cost effectiveness of the program has been called into question, in light of the tight fiscal climate. Before the 2008 Farm Bill expired in September 2012, Congress debated the amount of funding to cut from SNAP in a new Farm Bill. The proposed Senate and House bills reduced funding by $4.5 billion and $16.5 billion, respectively (Nixon 2012). The legislation stalled and, one year later, no compromise has been found. New Senate and House bills proposed in the second half of 2013 feature funding cuts of $4.5 billion and $40 billion, respectively (Steinhauer 2012; Nixon 2013). As of November 2013, a joint conference committee is negotiating on a final bill.

These debates reflect a basic question: To what extent should the US government provide assistance to low-income citizens? Advocates for more assistance often argue that it relieves low-income individuals of the all-consuming effort to meet their basic needs, enabling them to focus on improving their economic condition. They contend that SNAP has served as a much-needed cushion during the economic recovery, and that the rise in the program's costs reflects the growth in economic need during the 2007–2009 recession (Center on Budget and Policy Priorities 2012). In contrast, proponents for less government assistance believe that the social safety net is bloated and robs the recipients of the incentive to work. They argue that funding cuts will root out long-term dependence on government welfare by serving only those who are legitimately in need (Rector 2012).

This paper contributes to this debate by analyzing how changes in the unemployment rate of individuals without a high school degree affect the number of individuals who participate in SNAP. The goal of this study is to test the sensitivity of SNAP participation to changes in the unemployment rates of a group that is likely to be economically vulnerable.[2] If SNAP participation

[1] The 2008 Farm Bill changed the name of the Food Stamp Program (FSP) to the Supplemental Nutrition Assistance Program (SNAP), effective as of October 2008.

[2] Workers with no more than a high school education held nearly four out of every five jobs lost during the 2007 – 2009 recession, and employment among this group has declined since 1989 (Carnevale et al. 2012). The median weekly wage in 2012 for full-time workers in this group was $471 (US Bureau of Labor Statistics

were unaffected by unemployment, this would suggest that recipients take advantage of the program's benefits regardless of their employment circumstances, and that SNAP is not providing benefits primarily to individuals in need. In contrast, if SNAP participation were sensitive to unemployment, this would suggest that recipients' use of the program varies with their level of economic need and that the program is performing as designed.

To answer this research question, I combine annual data on aggregated state totals of the number of SNAP participants with annual data on state-level unemployment rates of the segment of the US population that did not graduate from high school. While previous research has examined the relationship between SNAP participation and state unemployment rates, there appears to be no other study that has directly tested how SNAP participation is affected by changes in the unemployment rates of a disadvantaged group. This analysis fills that void, using individuals without a high school degree as the disadvantaged group of interest. The term "low-skilled population" is used hereafter to refer to this group.

II. BACKGROUND

SNAP and its predecessor, FSP, are means-tested programs that have provided benefits to low-income individuals for over 40 years. To be eligible for benefits, households must have monthly gross and net incomes below 130 percent and 100 percent, respectively, of the poverty line.[3] They must also have less than $2,000 in assets, or less than $3,250 if they include an elderly or disabled member. Benefits decrease by 30 cents for each additional dollar in net income (Tiehen et al. 2012).

SNAP reaches many segments of the US population that are vulnerable to economic downturns. In fiscal year 2011, 76 percent of SNAP households contained children, elderly or disabled individuals. Forty-seven percent of all SNAP households included children; of this group, 56 percent were headed by single parents. Eighty-three percent of SNAP households had incomes below the federal poverty level. SNAP recipients also take advantage of other public assistance programs: during the same year, eight percent of SNAP households received cash assistance from the Temporary Assistance for Needy Families (TANF) program (Strayer et al. 2012).[4]

2012). On an annualized basis, these wages are slightly more than double the 2012 federal poverty level for a household of one. These data provide evidence that this group is likely to be economically disadvantaged.

[3] Net income is defined as gross income minus several deductions allowed under SNAP program rules: a standard deduction; deductions for earned income, for child care expenses, for medical care for elderly or disabled dependents, for legally owed child support payments, and for shelter costs in excess of half of the household's income after the other deductions are applied (US Dept. of Agriculture 2012).

[4] In contrast, nearly 98 percent of all households that participated in the Temporary Assistance for Needy Families program obtained SNAP benefits in fiscal year 2010 (Eslami et al. 2012).

The growth in SNAP expenditures and participation has fueled the ongoing Congressional disagreement over the program's funding. Federal spending on SNAP has increased from $34 billion in 2007 to $80 billion in 2012 (Congressional Budget Office 2012). The average monthly household benefit in 2011 was $284, up from $215 in 2007 (USDA FNS Program Data 2012). Between 2007 and 2011, the national unemployment rate rose from 4.6 percent to 8.9 percent, and the number of SNAP participants increased from some 26 million to nearly 45 million individuals (US Bureau of Labor Statistics 2012; Congressional Budget Office 2012).

SNAP is intended to work as a counter-cyclical program. In theory, participation in the program should rise when employment declines and decrease as employment increases. This study examines whether SNAP is, in fact, operating as designed by analyzing how well the program responds to changes in unemployment for an economically vulnerable group, namely, the segment of the US population without a high school degree.

III. LITERATURE REVIEW

Previous studies of FSP and SNAP have consistently found that changes in the economy have an impact on participation. Quantifying this effect has been a central challenge for researchers due to the concurrent influence of changing FSP and SNAP eligibility rules on program participation. Using fixed effects analyses, the studies cited in the following literature review highlight previous findings on the effects of the economy and of SNAP policies on SNAP participation.

The Impact of the Economy on FSP and SNAP Participation Rates

Most of the literature confirms that the program responds counter-cyclically to economic changes. Previous research has found a positive correlation between state unemployment rates and food stamp caseloads between 1980 and 1999 (Ziliak et al. 2003), and between 1989 and 2004 (Danielson and Klerman 2006). It has also found a positive relationship between state unemployment rates and the number of FSP-eligible individuals between 2000 and 2006 (Mabli et al. 2009). Additionally, previous research has demonstrated both positive contemporaneous and lagged relationships between state unemployment rates and FSP and SNAP caseloads between 1989 and 2009 (Klerman and Danielson 2011), and has found that the positive impact of state unemployment on food stamp caseloads per capita increased after welfare reform in 1996 (Bitler and Hoynes 2010).

The Impact of FSP and SNAP Policies on Participation

The difficulty in assessing the impact of the economy on participation lies in separating such effects from those of FSP and SNAP policy changes on participation. Various FSP- and

SNAP-related policies have been established over the past two decades. In 1996, as the economy expanded after the 1990–1991 recession, welfare reform legislation reduced FSP benefit levels, set time limits for benefit receipt for adults without disabilities in childless households, and denied FSP eligibility to many legal immigrants (Congressional Budget Office 2012). In 2001, many states eased the requirements in income reporting and in counting assets to determine benefit eligibility (USDA 2003). The 2002 Farm Bill reinstated FSP eligibility for certain types of immigrants and funded state efforts to encourage SNAP participation (Mabli et al. 2009). It also provided transitional benefits to families who moved off of welfare (USDA 2003). Subsequently, the 2008 Farm Bill increased the program's deductions in order to facilitate participation (Andrews 2012), and the 2009 American Recovery and Reinvestment Act temporarily raised the maximum monthly benefit (Congressional Budget Office 2012).

According to existing research, the adoption of these FSP and SNAP policies may have had a separate effect from the economy on food stamp participation. Ratcliffe et al. (2008) find a positive relationship between FSP participation between 1996 and 2003 and the exemption of vehicles from asset limits, a conclusion that contradicts a previous study by Hanratty (2006) that found no such significant relationship. Mabli et al. (2009) find that simplified reporting and expanded categorical eligibility were positively associated with the FSP caseload growth between 2000 and 2006. They also find no significant association between participation and the availability of outreach expenditures. Conversely, Mabli and Ferrerosa (2010) find that the availability of outreach spending is positively correlated with SNAP caseloads for the elderly-only, adult-only, and poorest households, between 2000 and 2008.

Demography and SNAP Participation

Previous studies also accounted for demographic factors in their analyses of SNAP participation. However, the specific demographic measure used varies from study to study. Researchers have included controls for the share of the population within specific age categories (Danielson and Klerman 2006; Klerman and Danielson 2011), family characteristics (Hanratty 2006), household composition (Ratcliffe et al. 2008), the share of non-citizens in the population (Mabli and Ferrerosa 2010), and the presence of single-female headed households (Bitler and Hoynes 2010).

Implications for this Study

Thus, the existing literature confirms that there is a relationship between SNAP participation and unemployment that may be confounded by other factors impacting participation. While previous studies have used overall state unemployment rates as a measure of economic changes, this measure does not offer a precise picture of the responsiveness of SNAP to unemployment among an economically vulnerable group. General state unemployment rates measure the economic conditions of individuals regardless of their eligibility for SNAP benefits or their likelihood to ever use the program. This approach makes these general unemployment rates a less perfect measure of the economic conditions of those vulnerable individuals who are the intended targets of the SNAP program. To address this gap in the current research and provide a more direct test of the relationship between SNAP and unemployment among such a targeted group, this study uses an alternate measure as its key independent variable, namely, the unemployment rates of low-skilled individuals.

IV. CONCEPTUAL FRAMEWORK

I hypothesize that state-level unemployment rates for the low-skilled population are positively correlated with the number of SNAP participants. As noted in the Literature Review above, economic conditions and the adoption of SNAP policies affect SNAP participation. Differences between states in the adoption of SNAP policies may contribute to changes in SNAP participation. This impact may be separate from the effect of changes in the economy on participation. My model also accounts for the influence of changes in the demographic composition of state populations on participation. These factors are diagrammed in Figure 1 below.

Demographic Composition

SNAP households participate in the program at different rates depending on their composition. The poorest households are most likely to participate in the program (Congressional Budget Office 2012). Similarly, households with children or that receive TANF benefits also participate at high rates. Households that include the elderly, immigrants, childless non-disabled adults, or that receive earnings participate at lower rates (Leftin et al. 2011).

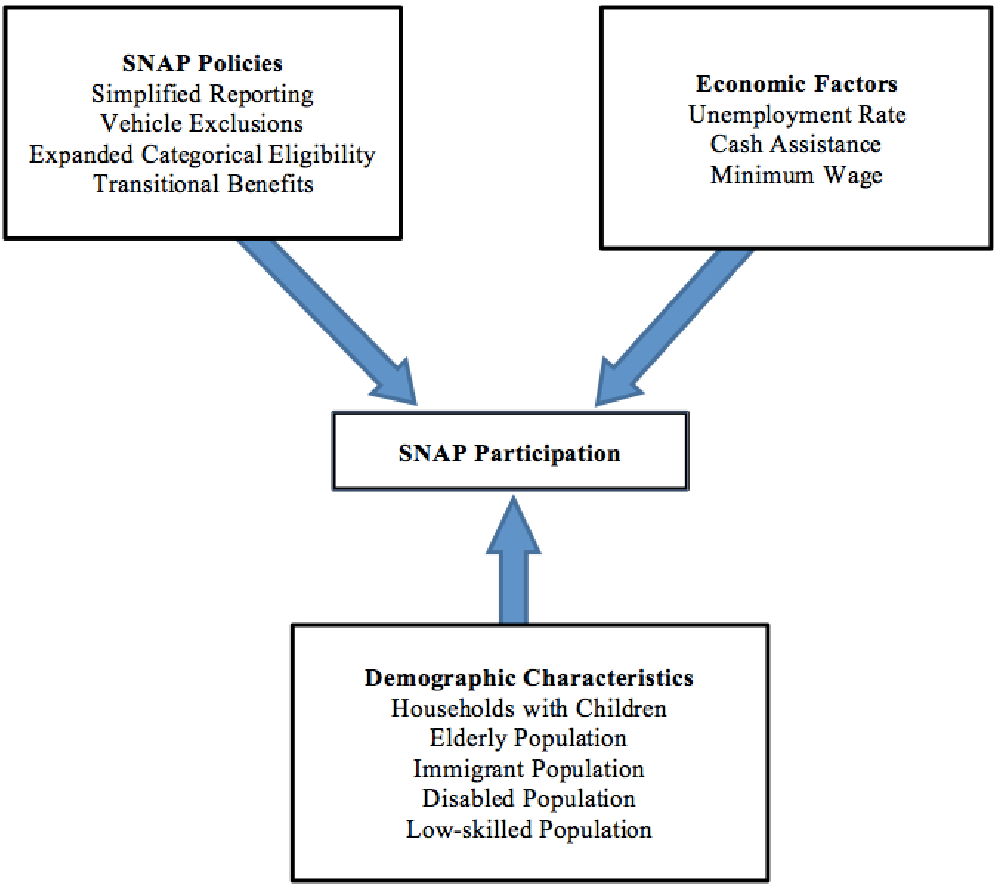
Adoption of SNAP Policies

Federal regulations enable states to adopt optional policies intended to expand eligibility, encourage participation, and streamline FSP operations at the state level (Andrews 2012). The following policy options directly impact the accessibility of benefits (USDA 2012) and have been recognized in previous research:

Simplified Reporting
This option enables states to simplify the reporting of household income and to lengthen certification periods, making it easier for households to

[5] In addition to the policies listed in Figure 1, changes in SNAP benefit levels also plausibly affect SNAP participation. The 2008 Farm Bill raised benefits by increasing the standard and child care deductions, raising the minimum benefit levels, and indexing the benefits to inflation (Andrews 2012). The 2009 American Recovery and Reinvestment Act raised the maximum benefit levels and suspended time limits for benefit receipt among childless non-disabled adults (Leftin et al. 2011). SNAP benefit levels are not listed in the Figure 1 because they are set at the federal level and apply uniformly to all states, so they are captured under state and year fixed effects.

Figure 1. Conceptual Framework of the Factors that Affect SNAP Participation[5]



receive benefits (Mabli and Ferrerosa 2010).[6] By 2011, 50 states had implemented this policy, up from 33 in 2003 (USDA 2012; Andrews 2012).

Vehicle Exclusions
This option enables states to exclude vehicles in the counting of assets when determining household eligibility for SNAP benefits. By 2005, 25 states excluded all vehicles from these asset limit tests (USDA 2005). By 2011, 35 states had implemented this exclusion (USDA 2012).

Expanded Categorical Eligibility
Under this option, households that participate in certain public assistance programs automatically qualify for SNAP benefits. In addition, categorically eligible households are not subject to asset tests (Congressional Budget Office 2012). In 2011, 42 states had implemented this policy, up from nine in 2002 (USDA 2012; Andrews 2012).

Transitional Benefits
Under this option, households that are leaving the TANF program can obtain

[6] SNAP households with shorter certification periods are required to report changes in their financial circumstances to state agencies more frequently in order to continue receiving SNAP benefits (Mabli and Ferrerosa 2010).

temporary SNAP benefits. In 2011, 20 states provided this benefit, up from 15 in 2005 (USDA 2012; USDA 2005).

V. DATA & METHODS

Data on state unemployment rates, economic factors, and demographic characteristics of the US population were collected from the US Census Bureau's American Community Survey (ACS) 1-year series. My analysis is restricted to the years between 2005, when the ACS was fully implemented, and 2011, the most recent year for which data are available (US Census Bureau 2009). State SNAP participation data were obtained from the USDA Food and Nutrition Service (FNS) Annual State Level Program Data for the fiscal years 2007–2011 and from the USDA 2006 and 2005 State Activity Reports.[7] Information on SNAP policies was obtained from the USDA FNS SNAP State Options Reports, corresponding to the years 2005–2007 and 2009–2011.[8]

[7] The Annual State Level Program Data are provided for the last five completed fiscal years and are subject to revision. The data used in this study were obtained from USDA Program Data that were revised as of November 9, 2012. USDA FNS Program Data and USDA State Activity Reports are available online at http://www.fns.usda.gov/pd/SNAPmain.htm.

[8] USDA FNS SNAP State Options Reports can be found at http://www.fns.usda.gov/snap/government/Policy.htm. No State Options Report was published for the year 2008, when the 2008 Farm Bill introduced SNAP policy changes. In footnote 11, I explain how I handle this issue so that data for these years can be included. Policy data for these years are imputed in my study under the assumption that the states carried out the same SNAP policies implemented in the previous year for which data are available.

I estimate a fixed effects regression model to analyze the relationship between the unemployment rates of the low-skilled population and SNAP participation. My specification controls for differences between states that are correlated with SNAP participation and unemployment rates as well as differences that do not change over time. Year fixed effects also control for characteristics that vary over time, that are common to all states, and that are correlated with SNAP participation and unemployment rates. The unit of analysis is the state-year.

The model specification is as follows:

$$\begin{aligned}&Foodstamp_{it}=\beta_0 + \\ &\beta_1 unemployedlowskill_{it} + \beta_2 lowskilled_{it} \\ &+ \beta_3 householdchild_{it} + \beta_4 elderly_{it} \\ &+ \beta_5 immigrant_{it} + \beta_6 disabled_{it} + \\ &\beta_7 cashassistance_{it} + \beta_8 minimumwage_{it} \\ &+ \beta_9 simplifiedreporting_{it} \\ &+ \beta_{10} transitionalbenefit_{it} + \\ &\beta_{11} categoricaleligibility_{it} + \beta_{12} vehicle_{it} + \\ &\alpha_i + \gamma_t + \mu_{it}\end{aligned}$$

where *i* represents the state index, *t* is the year index, α_i represents state time-invariant characteristics, γ_t represents dummy variables for each year, and μ_{it} is the error term. The initial sample size for the combined data set is 357 observations (51* 7).[9] Due to missing values in the dataset for some variables in the regression model, my final sample size is 342 observations. The model includes control variables for the demographic characteristics of state populations, economic factors that may affect participation other than low-skilled unemployment, and the

[9] Fifty states and the District of Columbia.

SNAP policy options adopted by states over the period under study. Table 1 provides definitions for all of the variables.

VI. DESCRIPTIVE STATISTICS

Tables 2 and 3 provide descriptive statistics for the dependent and key independent variables, state demographic characteristics, economic factors and policy controls. Table 2 shows that there is substantial variation in the sample in the number of SNAP participants per capita in an average month, ranging from a minimum of 4,111 participants per 100,000 people in a state to a maximum of 21,820 participants per 100,000 people in a state. The average monthly number of SNAP participants per 100,000 people in a state was 10,649. The average unemployment rate of individuals between 25 and 64 years of age without a high school degree was about 13 percent across states, and unemployment within this group ranged from 3.5 percent to 29.9 percent.[10]

Table 3 shows that the share of states—including the District of Columbia—that have adopted the policies defined in the Conceptual Framework section have increased between 2005 and 2011.[11]

[10] In comparison, the US annual average unemployment rate in 2011 for the nationwide population 16 years and older was 8.9 percent (US Bureau of Labor Statistics 2012).

[11] Data on the state adoption of the four SNAP policies of interest (simplified reporting, transitional benefits, vehicle exclusion, and expanded categorical eligibility) were not available for fiscal year 2008 as of this writing. Missing 2008 data for these variables were imputed when there were data available for 2007 and 2009 under the assumption that, if the policy was adopted in both of these years, it was also adopted in 2008. Similarly, if the policy was not adopted in both of those years it was assumed not to have been adopted in 2008. Missing 2008 data for these variables were not imputed if the policy was adopted in one year but not in the other year. Other than the policy options, there were no other missing values in the dataset. The original dataset contained 18 variables, with 357 state-year observations (51*7), for a total of 6,426 data points. Some 51 observations did not have data for the four policy variables, for a total of 204 missing values, or about three percent of the data points. Using the above-mentioned assumptions, values were imputed for 187 of the 204 missing data points. Values could not be imputed for some 15 observations. Consequently, these observations were dropped, resulting in a final sample size of 342 observations. A preliminary fixed effects analysis using all data and only non-imputed data indicates that the estimated effect of unemployment among low-skilled individuals on SNAP participation per capita is not sensitive to the inclusion of imputed data in the SNAP policy variables. The key coefficient of interest remains positive and statistically significant whether the regression uses imputed or non-imputed data. Moreover, a series of t-tests show that there are no statistically significant differences in the demographic and economic characteristics between observations for which simplified reporting, transitional benefits, vehicle exclusion, or categorical eligibility data were imputed and observations for which these data were not imputed, except for one control variable: the percentage of the population that is disabled. Although this may bias the coefficient for this control variable, this is not a concern because its effect on SNAP participation is not the main focus on this study.

Table 1. Variable Definitions

Variables	Definitions
Dependent Variable	
Foodstamp	This continuous variable measures the average monthly number of SNAP participants per 100,000 people in a state. The USDA defines SNAP participation for a given year as the number of SNAP participants in an average month of that year. Monthly totals of SNAP participants are obtained from states, summed and divided by twelve (USDA 2012). These estimates were obtained from the USDA FNS Program Data and converted into per capita measures using population data from the ACS.
Independent Variable of Interest	
Unemployedlowskill	This continuous variable measures the unemployment rate of the segment of the state population between 25 and 64 years old without a high school degree. These data are gathered from the ACS.
Demographic Characteristics	
Lowskilled	This continuous variable measures the percentage of the state population aged 25 and older without a high school degree. These data are gathered from the ACS.
Householdchild	This continuous variable measures the percentage of households in a state that contain families with children under the age of 18. These data are gathered from the ACS.
Elderly	This continuous variable measures the percentage of the state population that is 65 years and older. These data are gathered from the ACS.
Immigrant	This continuous variable measures the percentage of the state population that is foreign-born and does not have U.S. citizenship. These data are gathered from the ACS.
Disabled	For the years 2005, 2006 and 2007, this continuous variable measures the percentage of the state civilian population five years and older that is disabled. For 2008, 2009, 2010, and 2011, this variable measures the percentage of the total civilian non-institutionalized population that is disabled. These data are gathered from the ACS.
Economic Characteristics	
Cashassistance	This continuous variable measures the percentage of households in a state that received cash assistance from the TANF and General Assistance programs during the 12 months prior to the day of the survey interview (US Census Bureau 2012). These data are gathered from the ACS.
Minimumwage	This continuous variable measures the state minimum wage. The federal minimum wage prevails in states with no state minimum wage. The minimum wages are adjusted for inflation and expressed in 2011 dollars using the annual Consumer Price Index for All Urban Consumers. These data are gathered from the US Census Bureau.
Policy Options	
Simplifiedreporting	This dichotomous variable indicates whether or not a state has implemented simplified reporting in a given year. These data are gathered from the USDA.
Transitionalbenefit	This dichotomous variable indicates whether or not a state has implemented transitional benefits in a given year. These data are gathered from the USDA.
Categoricaleligibility	This dichotomous variable indicates whether or not a state has implemented expanded categorical eligibility in a given year. These data are gathered from the USDA.
Vehicle	This dichotomous variable indicates whether or not a state opted to exclude all vehicles from the counting of assets when determining a household's eligibility for SNAP benefits in a given year. These data are gathered from the USDA.

Table 2. Descriptive Statistics for Dependent, Key Independent, and Control Variables				
Variables	**Mean**	**Minimum**	**Maximum**	**Standard Deviation**
Average Number of SNAP Participants per Month per 100,000 people in a State	10,649	4,111	21,820	3,723
Unemployment Rate of Low-Skilled Population	12.96	3.5	29.9	4.27
Demographic Characteristics				
Percentage of Population without High School Degree	15.11	7.7	22.1	3.49
Percentage of Households with Children	30.71	16.7	40.3	2.7
Percentage of Population that is Elderly	12.7	6.6	17.6	1.83
Percentage of Population that is Immigrant	0.07	0	0.16	0.04
Percentage of Population that is Disabled	13.36	8.5	23.7	2.55
Economic Factors				
Percentage of Households on Cash Assistance	2.56	1.1	6.7	0.83
Minimum Wage (2011 Dollars)	7.02	2.77	8.96	0.99
N = 342				

Table 3. Descriptive Statistics for SNAP Policy Adoption Indicators*							
	Percent of States and the District of Columbia that Adopted SNAP Policies						
SNAP Policy	**2005**	**2006**	**2007**	**2008**	**2009**	**2010**	**2011**
Simplified Reporting	86%	90%	92%	94%	94%	96%	98%
Vehicle Exclusion from Asset Test	49%	51%	57%	67%	73%	67%	69%
Expanded Categorical Eligibility	76%	73%	69%	77%	78%	88%	82%
Transitional Benefits	29%	33%	35%	36%	37%	41%	39%
N = 342							

For each policy, the percentages indicate the share of all 50 states and the District of Columbia that adopted a given policy for each year in the sample, except for 2008. My analysis for that year excludes the 15 observations in the sample that have incomplete data for the four policy indicators in 2008, even after imputation.

Table 4. Regression Results					
Dependent Variable	**Number of SNAP participants per 100,000 people in a state**				
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
State and Year Fixed Effects	No	Yes	Yes	Yes	Yes
Key Independent Variable					
Low-skilled Population	366.58***	217.65***	171.32***	157.49***	138.56***
Unemployment Rate	(49.76)	(47.59)	(49.09)	(49.5)	(34.11)
Demographic Variables					
Percent Population without	800.79***		305.67	252.3	270.7
High School Degree	(68.68)		(268.98)	(241.62)	(197.02)
Percent Households	-500.70***		118.63	93.1	45.36
with Children	(90.38)		(180.1)	(165.87)	(176.53)
Percent Population	-361.85***		1,465.85**	1,294.00**	1,243.04**
that is Elderly	(135.3)		(608.15)	(628.93)	(560.62)
Percent Population	-35,033.50***		-15,794.80	-7,264.63	-5,292.90
that is Immigrant	(6,417.37)		(45,623.84)	(45,838.54)	(40,843.43)
Percent Population	-195.40**		-69.79	-104.6	-69.9
that is Disabled	(84.61)		(220.2)	(225.27)	(226.91)
Economic Variables					
Percent Households on	627.35***			566.81	601.92
Cash Assistance	(232.19)			(399.68)	(394.82)
Minimum Wage	2,643.42**			-261.94	-270.55
(logarithm)	(1,144.7)			(727.24)	(670.39)
Policy Variables					
Simplified Reporting	4,023.07***				-1,133.49**
	(602.51)				(494.16)
Transitional Benefits	4.61				318.64
	(296.3)				(249.94)
Vehicle Exclusion	-571.62*				364.76
	(313.92)				(295.34)
Categorical Eligibility	989.79***				-186.12
	(258.45)				(356.73)

VII. RESULTS

The results of my fixed effects analyses are summarized in Tables 4 and 5. Column 1 of Table 4 shows the results of the OLS regression that does not contain fixed effects, while the columns numbered 2 through 5 show the results of the fixed effects analyses in which groups of control variables are cumulatively added to the regression model. Table 5 shows alternative functional form specifications of the main regression model.[12] The

[12] For all of these regressions, I estimate robust standard errors clustered at the state level to correct for heteroskedasticity and autocorrelation. Furthermore, in these

Table 4 Continued					
	(1)	(2)	(3)	(4)	(5)
Constant	8,300.36*	6,518.63***	-17,025.76	-14,213.00	-12,135.06
	(4,714.21)	(516.83)	(12,053.99)	(11,697.87)	(11,233.25)
Observations	342	342	342	342	342
R-squared	0.749	0.911	0.919	0.921	0.925
F-statistics and p-values of Joint Hypotheses					
Demographic variables			2.06*	1.81	1.96
			(0.086)	(0.129)	(0.101)
Economic variables				1.02	1.24
				(0.368)	(0.298)
Policy variables					3.11**
					(0.023)

Robust standard errors are given in parentheses under coefficients and p-values are given in parenthesis under F-statistics

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

coefficient for the key independent variable indicates the change in the number of SNAP participants per 100,000 people in a state that is associated with a one percentage point increase in the unemployment rate of the low-skilled population, holding constant all factors included in the model.[13]

The key coefficients for the low-skilled unemployment rate, shown in Table 4, indicate a consistently positive and statistically significant relationship between this variable and SNAP participation. As shown in columns 1 through 4, the coefficient for the low-skilled unemployment rate remains positive and statistically significant across partial model specifications. The full model in column 5, which contains all the control variables and employs fixed effects, shows a statistically significant coefficient of 138.56, indicating that a one percentage point increase in the unemployment rate of the low-skilled population is associated with an increase of about 139 SNAP participants for every 100,000 people in a given state, holding constant state and year fixed effects and the control variables included in the model. The results of the joint significance tests, shown in the bottom panel of Table 4, indicate that the demographic and economic variables may have no effect on SNAP participation. In contrast, as a group, the policy variables may have an effect on SNAP participation.

The results in Table 5 indicate that SNAP participation may have a non-linear relationship with the contemporaneous low-skilled unemployment rate and with the

regressions the minimum wage is expressed as a logarithm, rather than as the absolute dollar amount shown in Table 2.

[13] In a sensitivity test, I also estimate a version of the regression model that uses the untransformed minimum wage variable as a control. The results of this alternative specification are comparable to those of the main regression model in this study and can be found in the Appendix.

Table 5. Regression Results for Alternative Specifications

Dependent Variable	SNAP participation per 100,000 people in a state							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
State and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Key Independent Variable								
Low-skilled	138.56***	-73.15					74.48*	
Unemployment Rate	(34.11)	(75.49)					(40.05)	
Squared Low-skilled		6.99**						
Unemployment Rate		(2.71)						
1-year Lagged			179.03***	-61.02				
Low-skilled Unemployment Rate			(58.56)	(100.02)				
Squared 1-year Lagged				8.01**				
Low-skilled Unemployment Rate				(3.39)				
2-year Lagged Low-skilled					141.80*	-104.79		
Unemployment Rate					(74.76)	(114.02)		
Squared 2-year Lagged						9.04**		
Low-skilled Unemployment Rate						(3.41)		
Low-skilled Unemployment * After 2008							78.81*	
							(45.57)	
Overall State Unemployment Rate								103.69
								(140.39)
Demographic Variables								
Percent Population without	270.7	311.79	303.3	330.33	151.31	146.05	281.44	
High School Degree	(197.02)	(197.35)	(205.73)	(208.04)	(233.53)	(222.4)	(200.09)	
Percent Households with Children	45.36	103	114.2	230.2	-83.97	-19.32	102.46	13.53
	(176.53)	(164.22)	(182.18)	(185.02)	(194.19)	(190.7)	(163.36)	(190.95)

Table 5 Continued

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Percent Population that is Elderly	1,243.04**	998.94*	1,185*	945.11	1,083.51	933.63	995.01*	1,487.68**
	(560.62)	(506.47)	(649.12)	(592.57)	(770.08)	(758.89)	(539.55)	(644.41)
Percent Population that is Immigrant	-5,292.90	-17,893.73	3,731	-6,580.23	-14,343.06	-16,226.18	-16,130.45	-6,608.19
	(40,843.43)	(38,514.57)	(43,580.1)	(38,786.51)	(43,264.35)	(39,886.71)	(38,573.36)	(37,639.82)
Percent Population that is Disabled	-69.9	-70.81	-146.5	-144.89	-153.59	-164.33	-50.4	24.47
	(226.91)	(215.58)	(248.51)	(232.62)	(262.55)	(239.4)	(221.05)	(216.83)
Economic Variables								
Percent Households on Cash Assistance	601.92	576.24	614.6	624.96*	604.11	671.35*	581.4	730.63*
	(394.82)	(390.47)	(385.)	(368.26)	(372.49)	(348.78)	(401.77)	(388.04)
Minimum Wage (logarithm)	-270.55	-222.49	-199.7	-130.29	-190.75	-158.73	-163.46	-204.62
	(670.39)	(686.05)	(830.44)	(813.25)	(935.87)	(870.42)	(671.61)	(723.37)
Policy Variables								
Simplified Reporting	-1,133.49**	-1,072.52**	-1,349**	-1,337.65**	-1,978.97**	-1,904.91**	-1,075.01**	-1,057.21*
	(494.16)	(479.27)	(606.71)	(594.61)	(753.98)	(751.06)	(494.36)	(536.36)
Transitional Benefits	318.64	325.02	394.6	451.35	-28.08	203.56	330.05	259.14
	(249.94)	(238.56)	(305.48)	(320.5)	(229.93)	(248.02)	(234.88)	(255.59)
Vehicle Exclusion	364.76	413.79	478.6	623.89**	776.79*	944.27**	407.17	509.68
	(295.34)	(284.27)	(295.23)	(288.63)	(415.79)	(395.02)	(289.15)	(352.29)
Categorical Eligibility	-186.12	-203.6	-257.8	-262.47	-406.83	-359.87	-204.05	-203.17
	(356.73)	(347.28)	(326.87)	(297.46)	(326.12)	(305.89)	(355.18)	(394.98)
Constant	-12,135.06	-9,410.29	-14,472	-13,496.43	-2,230.41	-944.92	-10,155.14	-10,646.55
	(11,233.25)	(10,728.18)	(12,578.42)	(12,048.2)	(14,164.01)	(14,472.77)	(11,092.86)	(12,787.6)
Observations	342	342	291	291	240	240	342	342
R-squared	0.925	0.928	0.934	0.937	0.936	0.939	0.926	0.919

Table 5 Continued

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
F-statistics and p-values of Joint Hypotheses								
Squared and Linear Untransformed		8.88***						
Unemployment Rates		(0.001)						
1-Year Lagged Squared and Linear				5.81***				
Untransformed Unemployment Rates				(0.005)				
2-Year Lagged Squared and Linear						4.76**		
Untransformed Unemployment Rates						(0.013)		
Low-skilled Unemployment * After 2008							7.65***	
And Untransformed Unemployment Rate							(0.001)	
Inflection point		5.23		3.81		5.79		
		(3.652)		(4.982)		(4.903)		

Robust standard errors are given in parentheses under coefficients. Standard errors are given in parenthesis under the inflection points. P-values are given in parenthesis under F-statistics

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

low-skilled unemployment rates of the previous two years. Column 1 replicates the full model from column 5 of Table 4. Column 2 shows that the relationship between SNAP participation and the unemployment rate is non-linear. The correlation is initially negative then becomes positive, and the "inflection point" occurs at the imprecisely estimated unemployment rate of 5.2 percent, below the average rate of 13 percent shown in Table 2.[14]

The results from columns 3, 4, 5, and 6 suggest that SNAP participation is correlated with the low-skilled unemployment rate of the previous two years. The relationship between participation and the lagged unemployment rates is non-linear, and the direction of the correlations with the one-year and two-year lagged rates change at the imprecisely estimated rates of 3.8 percent and 5.8 percent, respectively. The results from column 7 suggest that the effect of low-skilled unemployment on SNAP participation was stronger after the 2008 Farm Bill was enacted.[15]

The model in column 8 uses overall state unemployment rates as an alternate key independent variable, in order to examine how the results differ from previous studies,

[14] I also estimated a regression model with a cubed low-skilled unemployment rate. The coefficient for this variable was statistically insignificant at the 10 percent level. The results for this specification are therefore not included here.

[15] The 2007–2009 recession is not mentioned as a potentially confounding factor, because it applied commonly to all states and is therefore captured under state and year fixed effects.

given my choice of a different key independent variable.[16] The results confirm the positive correlation found in the existing literature between state unemployment rates and SNAP participation. However, this relationship is weaker than that found in the full model in column 1, possibly because overall unemployment includes individuals who are unlikely to use SNAP benefits.

Tables 4 and 5 show that the controls are, for the most part, individually statistically insignificant across the different model specifications. One puzzling exception is simplified reporting, which has a consistently negative and statistically significant coefficient in Table 5. This policy is intended to simplify the process of certifying SNAP benefits for recipients and so, in theory, should have a positive impact on SNAP participation. This unexpected finding presents an interesting potential avenue for future research.

In summary, the main regression analyses show that low-skilled unemployment has a non-linear relationship with, and contemporaneous and lagged effects on, SNAP participation. Robustness checks appear in the Appendix, and show that the relationship is consistent across different variations of the main model. These findings confirm the measurable response of SNAP to low-skilled unemployment.

[16] State unemployment rates were obtained from the US Bureau of Labor Statistics, available at http://www.bls.gov/lau/.

VIII. DISCUSSION

Major Findings

The main regression results confirm my hypothesis that SNAP participation and the state-level unemployment rates of the low-skilled population are positively correlated. As predicted, food stamp participation rises when state-level low-skilled unemployment rates increase. Given that the per capita average number of SNAP participants in the sample, as shown in Table 2, is 10,649 per month for every 100,000 people in a state, the estimated increase of 139 participants represents an increase in average participation of less than two percent. This suggests that an increase in low-skilled unemployment does not, by itself, result in a notable take-up of food stamps by the low-skilled population.

In addition, the relationship between SNAP participation and low-skilled unemployment rates is also non-linear. In an ordered distribution of the low-skilled unemployment rates in the sample, the rate at the 25th percentile is 9.6 percent; the median rate is 11.9 percent; and the rate at the 75th percentile is 16.2 percent. At these three percentiles, the non-linear model predicts that SNAP participation increases in response to a one percentage point increase in the low-skilled unemployment rate by about 61 participants, 93 recipients, and 153 participants, respectively, for every 100,000 people in a state. This indicates that the rate at which low-skilled individuals take up SNAP benefits increases as the contemporaneous unemployment rate of this group rises.

The regression results also indicate that SNAP participation is positively correlated with the low-skilled unemployment rate one and two years prior. This suggests that some low-skilled individuals do not immediately act on the effects of unemployment, even though they may experience them. They may wait a period of time as their savings and other resources dwindle before turning to SNAP for assistance. In addition, the results show that the higher the initial level of the lagged unemployment rates, the greater the increase in SNAP participation in response to changes in low-skilled unemployment in the previous two years.

Finally, the regression results suggest that the effect of low-skilled unemployment was larger after the passage of the 2008 Farm Bill. This may reflect the impact of the 2007–2009 recession during which economic need increased among the population as a whole. It may also reflect the effects of policy changes in the 2008 Farm Bill and the 2009 American Recovery and Reinvestment Act, which expanded eligibility for SNAP benefits. These findings appear to confirm that these legislative changes facilitated an increase in the responsiveness of SNAP to the unmet needs of low-skilled individuals.

Comparison with Previous Literature

The results of my analysis are not directly comparable to the findings of previous research because the model specifications in previous studies use overall state unemployment rates. Nevertheless, the effect of low-skilled unemployment estimated in my analysis is consistent with the findings of other studies that use fixed effects specifications with state-level panel data. Mabli et al. (2009) estimated an increase in the SNAP participant count per capita of four percent for a one percentage point increase in the overall unemployment rate. Klerman and Danielson (2011) and Bitler and Hoynes (2010) estimated an increase in SNAP caseloads of about four percent and nearly five percent, respectively. My results are consistent with the signs of the coefficients estimated in those studies.

Since my analysis uses the unemployment rates of a group that is likely to use SNAP, it is conceivable that the effect of low-skilled unemployment would be at least as strong as that found in studies that use overall unemployment rates. In order to determine this, I use the coefficients from the two studies by Mabli et al. (2009) and Mabli and Ferrerosa (2010) to construct rough estimates of the elasticity of SNAP participation with respect to overall unemployment rates. To construct my estimates, I use unemployment data from the US Bureau of Labor Statistics to calculate the percent change produced by a one percentage point increase from the average unemployment rate over the time periods in the two studies. Combining these percent changes with the estimated effects on SNAP participation from the two studies, both studies yield an elasticity of roughly 0.20. Employing the same method for the results of my analysis, I estimate an elasticity of roughly 0.17. This confirms that the effect of changes in the low-skilled unemployment rate on SNAP participation is roughly comparable to the effect of changes in the overall unemployment rate on participation.[17]

In addition, my findings are largely consistent with the results of previous studies that show a lagged effect of unemployment on SNAP participation. Rough estimates of the elasticity of participation in response to lagged unemployment rates, calculated from Mabli et al. (2009), Mabli and Ferrerosa (2010), and my analysis, indicate that the effect of changes in the one-year lagged low-skilled unemployment rate on participation is nearly two-thirds as large as the effect of the lagged overall unemployment rate. Similarly, the effect of changes in the two-year lagged low-skilled unemployment rate on participation is nearly half as large.

Policy Implications

The results of my analysis offer a mixed bag for advocates and critics of the SNAP program. The small effect of low-skilled unemployment on SNAP participation might reflect the fact that unemployed individuals may simultaneously take advantage of SNAP and other options available for public assistance. This may raise questions among critics about the relative utility of SNAP when compared to other government programs designed to assist unemployed individuals. Yet, at the same time, my findings show that SNAP adapts to some extent to the level of economic need among the low-skilled population. For advocates, this flexibility may provide evidence that SNAP has value in alleviating unmet needs. From this perspective, the small effect of low-skilled unemployment may reflect individuals' lack of awareness of SNAP and their eligibility for benefits.

Despite this ambiguity, the evidence from my analysis of a delay between changes in unemployment and participation suggests the need for restraint among policymakers in drawing hasty conclusions about the utility of the program. These findings show that much of the shift in SNAP participation does not immediately follow changes in economic conditions for low-skilled individuals, so the effects of benefit receipt, positive or negative, are unlikely to emerge for a period of time. Furthermore, my analysis leads to the conclusion that the current efforts to cut SNAP funding during an economic recovery may weaken the program and erode a source of support for those individuals who have been hard-hit by the recession. Consequently, policymakers should be cautious about proposing reductions in program funding.

[17] Using SNAP participation of low-skilled individuals rather than overall SNAP participation as the dependent variable in my regression would arguably demonstrate a stronger relationship between participation and low-skilled unemployment. However, the US Department of Agriculture does not publish such data.

Analytical Limitations

Although my analysis includes a wide range of controls, it may nevertheless be subject to omitted variable bias. Several time-varying factors that determine SNAP participation are not easily measurable, and therefore they are not included as control variables in my regression model. These include individuals' awareness of the existence of the SNAP program, their perception of eligibility for benefits, the ease of applying for benefits, the level of stigma associated with food stamp benefits, and individual expectations about future income. Federal outreach spending, which is provided to states to encourage SNAP participation, is also excluded from my analysis. This variable is excluded from the regression analysis due to the practical difficulty in accurately measuring it.[18] The exclusion of these factors may bias the key coefficient in my regression results. Nonetheless, my model specification follows the practice of previous studies of controlling for policy, economic, and some demographic factors, thereby lending credibility to my findings.

Conclusion

While the above analysis demonstrates that participation in SNAP increases as the unemployment rate rises among low-skilled individuals, it also suggests that the growth rate of SNAP participation increases as low-skilled unemployment rises and that there is some lag time between the change in unemployment and that in participation. Furthermore, the responsiveness of SNAP participation to the economic conditions of the low-skilled population increased after the passage of the 2008 Farm Bill. Finally, the estimated effect of the low-skilled unemployment rate on SNAP participation is comparable to the effect of the overall unemployment rate on participation as reported in previous studies. These results exemplify the continuing need to study the factors that are associated with SNAP participation, as understanding the dynamics of participation can help to pinpoint more precisely how SNAP receipt is related to disadvantage. Further research along these lines can improve the targeting of SNAP benefits toward those individuals who may benefit the most from this form of public assistance.

IX. APPENDIX

Sensitivity Analysis

Tables 6 and 7 present the results of sensitivity analyses that test the robustness of the findings from the main regression analyses. Column 1 in Table 6 replicates the full model

[18] The USDA makes no distinction between the expenditures that are devoted to outreach efforts and those that are devoted to other operational costs, making the accurate measurement of state outreach efforts difficult. Mabli and Ferrerosa (2010) acknowledge this challenge when constructing their measure of state outreach funding and caution that inaccuracies in their categorization of expenditures as outreach spending may bias their results.

from column 5 of Table 4 for ease of comparison. Column 2 in Table 6 shows the results of a regression that omits the variable controlling for the percentage of the state population without a high school degree, because this variable and the low-skilled unemployment rate are mechanically correlated. The model in column 3 of the same table includes overall state employment rates as a control variable, in order to control for employment changes that may affect SNAP participation for population groups other than those without a high school degree.[19] This control variable was previously excluded from the main regression model due to its mechanical correlation with the low-skilled unemployment rate. Finally, the model in column 4 of Table 6 omits the control variable that measures the percentage of the state population that is disabled. After 2007, the American Community Survey modified the way that it measures the percentage of disabled civilians; the model in column 4 therefore tests whether the inclusion of this control variable in the main regression model affects the key coefficient despite the change in measurement.

In Table 7, columns 1 and 2 show the results of the full regression models with the original policy dummy variables before imputation and without population weights, respectively. Column 3 shows the results of the full regression model when the actual adjusted minimum wage is included, rather than its logarithm. In summary, the coefficients for the low-skilled unemployment rate in these analyses are comparable to the estimates from the main regression model. This reinforces the findings from the main analysis and indicates that they are robust to reasonable changes in the regression model.

[19] The employment rate is obtained from the US Census Bureau and measures the percentage of the state civilian population 16 years and older that is employed. The denominator for this variable reflects a count of all individuals 16 years and older and is not limited to labor force participants (US Census Bureau 2012).

Table 6. Sensitivity Analysis

Dependent Variable	SNAP participation per 100,000 people			
	(1)	(2)	(3)	(4)
State and Year Fixed Effects	Yes	Yes	Yes	Yes
Key Independent Variable				
Low-skilled Population	138.56***	135.30***	79.91**	138.38***
Unemployment Rate	(34.110)	(37.160)	(32.150)	(34.440)
Demographic Variables				
Percent Population	270.7		252.33	244.29
without High School Degree	(197.020)		(208.030)	(162.620)
Percent Households	45.36	34.22	18.03	46.33
with Children	(176.530)	(178.860)	(175.690)	(176.870)
Percent Population	1,243.04**	1,157.52*	849.6	1,225.45**
that is Elderly	(560.620)	(581.100)	(662.250)	(575.630)
Percent Population	-5,292.90	-9,094.60	4,354.42	-4,652.43
that is Immigrant	(40,843.430)	(39,135.610)	(39,836.400)	(39,993.710)
Percent Population	-69.9	35.65	-46.04	
that is Disabled	(226.910)	(197.710)	(221.970)	
Economic Variables				
Employment Rate			-251.55*	
Percent Households	601.92	643.11	515.84	587.94
on Cash Assistance	(394.820)	(412.640)	(378.660)	(389.770)
Minimum Wage	-270.55	-464.81	-292.13	-303.32
(logarithm)	(670.390)	(698.920)	(645.550)	(656.490)
Policy Variables				
Simplified Reporting	-1,133.49**	-1,110.48**	-1,058.13**	-1,153.85**
	(494.160)	(509.900)	(462.790)	(492.420)
Transitional Benefits	318.64	253	254.51	308.54
	(249.940)	(260.290)	(247.570)	(239.280)
Vehicle Exclusion	364.76	411.72	338.03	375.63
	(295.340)	(297.000)	(285.610)	(287.590)
Categorical Eligibility	-186.12	-126.95	-184.76	-178.56
	(356.730)	(386.920)	(369.320)	(363.750)
Constant	-12,135.06	-7,534.58	8,952.30	-12,513.07
	(11,233.250)	(11,781.380)	(19,555.330)	(10,872.170)
Observations	342	342	342	342
R-squared	0.925	0.924	0.927	0.925

Robust standard errors in parentheses

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

Table 7. Additional Sensitivity Analysis			
Dependent Variable	**SNAP participation per 100,000 people**		
	(1)	**(2)**	**(3)**
State and Year Fixed Effects	Yes	Yes	Yes
Key Independent Variable			
Low-skilled Population	140.90***	130.45***	138.44***
Unemployment Rate	(37.030)	(27.310)	(34.080)
Demographic Variables			
Percent Population	232.65	154.84	271.49
without High School Degree	(216.540)	(123.800)	(193.340)
Percent Households	66.66	141.42	45.31
with Children	(188.810)	(103.180)	(175.920)
Percent Population	1,227.89**	1,010.90***	1,244.71**
that is Elderly	(588.180)	(337.210)	(561.610)
Percent Population	-1,457.23	-39,850.33*	-5,600.47
that is Immigrant	(42,002.750)	(22,414.090)	(40,627.300)
Percent Population	-73.99	27.91	-70.53
that is Disabled	(251.920)	(142.110)	(226.430)
Economic Variables			
Percent Households	642.23	640.52**	602.97
on Cash Assistance	(423.150)	(279.650)	(394.470)
Minimum Wage	-525.46	-98.99	
(logarithm)	(668.960)	(585.510)	
Minimum Wage			-37.2
			(120.750)
Policy Variables			
Simplified Reporting		-440.83	-1,129.96**
		(463.390)	(497.280)
Transitional Benefits		194.59	323.54
		(336.370)	(253.720)
Vehicle Exclusion		116.13	365.62
		(242.030)	(295.410)
Categorical Eligibility		294.29	-183.54
		(243.470)	(355.150)

Table 7. Additional Sensitivity Analysis			
	(1)	(2)	(3)
Policy Variables Before Imputation			
Simplified Reporting	-1,151.37**		
	(498.660)		
Transitional Benefits	388.22		
	(271.460)		
Vehicle Exclusion	358.42		
	(312.470)		
Categorical Eligibility	-152.53		
	(361.210)		
Constant	-11,965.86	-11,152.26*	-12,404.88
	(11,504.720)	(5,951.360)	(11,258.140)
Observations	306	342	342
R-squared	0.926	0.916	0.925

Robust standard errors in parentheses

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

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