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Note from the Editor

On behalf of the *Comparative Advantage* Editorial Board, we are honored to present the sixth volume, Summer Issue of the Stanford Undergraduate Economics Journal.

Through the years, Comparative Advantage has changed and expanded to include an online blog in addition to our bi-yearly issues, staying true to our mission of providing students a platform to showcase their economic prowess. We are proudly run by undergraduate students here at Stanford: our multi-disciplinary staff works tirelessly to edit and publish submissions from university students across the world, in order to make economic research more accessible to all audiences.

The latest volume discusses a wide range of topics: the impact of the ACA Medicaid expansion on bankruptcy; the effect of agribusiness political campaign contributions on individual product producers; VAR modelling of interest rate shocks on bank balance sheets; and the economic impact of psychological distress on former child soldiers. Our blog further covers topics such as quantifying the value of a Michelin star, the employment effects of minimum wage increases, and many more. With a growing number of submissions contributing to a more competitive submission process, we are excited to publish these stellar papers.

Last but certainly not least, we would like to express our gratitude to the Stanford Economics Department and the Stanford Economics Association (SEA) for their continued support.

Ruru (Juan Ru) Hoong and Daniel Wainstock
2018-19 co-Editors-in-Chief

Editors and Staff

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Vector Autoregressive Modeling of Interest Rate Shocks on Bank Balance Sheets: A Comparative Study

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***Abstract*— Di Tella and Kurlat (2017) and Drechsler, Savov, and Schnabl (2017a) study the effects of a nominal interest rate shock on various bank balance sheet variables. I study the same relationships using a vector autoregression (VAR) model, to understand them over multiple periods of time, without assumptions of exogeneity, and with clear interactions between variables through impulse response functions (Hamilton, 1994). I find that an increase in the nominal interest rate is associated with a much smaller increase in the rate banks pay on deposits and other expenses. Furthermore, an interest rate shock does not have much effect on bank net worth. This finding is important because previously banks were thought to be very sensitive to interest rate movements through their maturity transformation business model (Drechsler et al., 2017a).**

I. Introduction

Studying the effects of interest rate shocks on bank balance sheets gives insight into monetary policy transmission as well as into the workings of banks (Kashyap & Stein, 1994). Understanding monetary policy transmission is critical to understanding the impacts of the Federal Reserve's monetary policy. The transmission of this policy through bank balance sheets is relevant since banks play an oversized role in the economy by acting as financial intermediaries between all other industries and households (Kashyap & Stein, 1994). My research is inspired by Di Tella and Kurlat (2017) and Drechsler, Savov, and Schnabl (2017a). Both study the effects of interest rate shocks on bank balance sheet variables using theoretical models and ordinary least squares (OLS) regressions. Drechsler et al. (2017a) and Di Tella and Kurlat (2017) come upon similar ideas by looking at different balance sheet variables. Drechsler et al. (2017a) find that banks do not face interest rate risk. They explain that banks are able to conduct "maturity transformation without interest rate risk" by matching the sensitivity of their interest income and their interest expense (Drechsler et al., 2017a, p. 2). They state that this results in interest rate shocks not having an effect on banks' return on assets (ROA). Di Tella and Kurlat (2017) argue that if banks are risk averse, they hedge in order for their net worth to decrease after interest rates increase, and vice versa. They state that "banks' exposure to interest rate risk is part of a dynamic hedging strategy" and that "the size of banks' exposure to interest rate risk is consistent with a dynamic hedging strategy by highly risk averse agents" (Di Tella & Kurlat, 2017, p. 2; Di Tella & Kurlat, 2017, p. 4). In summary, Drechsler et al. (2017a) argue banks do not face interest rate risk, and if they are risk averse, according to Di Tella and Kurlat (2017), they hedge their risk exposure using derivatives. All of these authors test most of these claims with OLS regressions, and I will test them using a vector autoregressive (VAR) model. My contribution to this area is that I study both papers' claims with a VAR model in order to see how the variables interact in a dynamic setting over multiple periods of time, without assumptions of exogeneity, and with isolated shocks of one variable on another variable (Hamilton, 1994).

My VAR model of the relationship between the London Interbank Offered Rate (LIBOR) and the deposit spread (defined by Di Tella and Kurlat (2017) as the difference between LIBOR and the rate banks pay on deposits) shows that a 100 bps increase in LIBOR is associated with around a 57 bps change in the deposit spread, which is similar to the findings of Di Tella and Kurlat (2017) and Drechsler et al. (2017a). This confirms Drechsler et al. (2017a), who state that bank deposit rates are sticky, leading to interest rate sensitivity matching. However, I show that when LIBOR increases by 100 bps, bank net worth increases by 6 bps: a result that is very different from Di Tella and Kurlat (2017), who find a 30 percent decrease in bank net worth. My VAR model states that there is a small positive effect on bank net worth rather than a large negative effect.

My VAR models of the relationships between the federal funds rate, interest income, interest expense and ROA show similar results to Drechsler et al. (2017a). A 100 bps increase in fed funds is associated with a 22 bps change in interest expense and 23 bps change in interest income, which confirms Drechsler et al. (2017a) in their claims that: 1) deposit rates are sticky, and 2) banks match their interest income and interest expense sensitivities. A 100 bps change in fed funds is associated with a 3 bps change in ROA which also matches Drechsler et al. (2017a). This means that interest rate shocks do not affect banks' ROA.

The conclusions about the effect of interest rate shocks on bank balance sheet variables matter for a variety of reasons. The conclusions of the Di Tella and Kurlat (2017) and Drechsler et al. (2017a) papers and my VAR models suggest that the stability of the financial system partially depends on banks operating as an oligopoly. Drechsler et al. (2017a) explain: the stability of a bank's returns (measured as ROA) partially depends on the matching of the interest rate sensitivity of their interest expense and interest income. Bank assets are insensitive to a shock in the short-term interest rate,

and thus liabilities must be as well (Drechsler et al., 2017a). This is possible if deposit rates are sticky—they don't move with the fed funds rate—which is only possible if the bank market has high market concentration, acting as an oligopoly (Drechsler et al., 2017a). If banks had less market power and their interest expense was less predictable, their profitability would be more volatile.

What is the future of banks' oligopoly? It may be getting stronger (Ellis, 2012). One would think that the shift to electronic payment systems has led to not just more efficient banking but that it has given households more choices of payment methods, e.g., holding credit cards with different banks, peer-to-peer payment systems, etc. However, credit and debit cards and other electronic payment systems actually reinforce the bank oligopoly when households set up automatic payments (Ellis, 2012). Automatic payment of bills, loans, subscription services, and streaming services all tie a household to a specific bank (Ellis, 2012). All of these automatic payments make it much more difficult for a household to switch banks (Ellis, 2012). This could mean that high market concentration in banking is increasing, which might continue to stabilize banks' ROA and thus their financial stability.

The findings on the relationship between the fed funds rate and bank balance sheets are relevant to monetary policy. The Federal Reserve has a dual mandate of full employment and price stability; it also seeks to promote financial stability (Board of Governors of the Federal Reserve System, 2018). Understanding how the Federal Open Market Committee's policy changes affect banks' interest income, interest expense and ROA informs the relationship between monetary policy and financial stability.

II. Literature Review

The relationship between monetary policy, the deposit channel of monetary policy transmission, and bank balance sheets has been studied extensively. Di Tella and Kurlat's (2017), Drechsler et al.'s (2017a) and my research depend on the existence of a bank lending channel of monetary policy transmission (Kashyap & Stein, 1994). In the study of the relationship between monetary policy and bank balance sheets, Kashyap and Stein (1994) have found that when the Federal Reserve increases the fed funds rate, the substitution from deposits to other higher-yielding liabilities is not frictionless. This causes a decrease in bank liabilities, impacting banks' balance sheets and affecting their lending opportunities (Kashyap & Stein, 1994). This finding is important because it exhibits how "capital market imperfections" lead to a contradiction of the Modigliani-Miller theory, which would predict a seamless substitution from deposits to other liabilities (Kashyap & Stein, 1994, p. 152).

Di Tella and Kurlat (2017) and Drechsler et al. (2017a) agree that an increase in the nominal interest rate leads to a much smaller increase in the deposit rate. Di Tella and Kurlat (2017) defined the deposit rate as the rate banks paid on deposits. Drechsler et al. (2017a) analyzed the variable titled interest expense rate (a ratio of interest expense to assets), but since deposits are 70 percent of liabilities, the interest expense rate is similarly useful for analyzing how much banks pay on deposits. Di Tella and Kurlat (2017) used LIBOR as the nominal interest rate while Drechsler et al. (2017a) used the federal funds rate, and both have the same results on the sensitivity of the deposit rate or interest expense rate to an interest rate shock. Specifically, Drechsler et al. (2017a) found that a 100 bps increase in the fed funds rate leads to a 36 bps increase in the interest expense rate. Di Tella and Kurlat (2017) found that a 100 bps increase in LIBOR leads to a 38 bps increase in the deposit rate. Drechsler et al. (2017a) explain that banks only raise the deposit rate by 36 bps for every 100 bps LIBOR increase because they have market power (high market concentration), which is supported by the fact that bank market concentration is high at the local level throughout the United States.

The deposit rate is relevant because retail deposits are more than 70 percent of banks' liabilities (Drechsler et al., 2017a). This exposure to deposits naturally leads to a discussion of the relationship between the nominal interest rate and bank balance sheets. Drechsler et al. (2017a) studied the relationship between the fed funds rate, the interest expense rate, the interest income rate, and ROA. They found that interest rate changes do not alter banks' ROA (Drechsler et al., 2017a). This is possible because banks match the sensitivity of their interest income (to fed funds shocks) to the sensitivity of their interest expense (Drechsler et al., 2017a). Drechsler et al. (2017a) found that when fed funds increases by 100 bps and the interest expense rate goes up by 36 bps, the income interest rate also goes up by 38 bps. They explain it as following: interest income comes from assets that are generally long-term and fixed-rate. Liabilities are mostly deposits that are zero maturity (Drechsler et al., 2017a). How can banks have maturity mismatch without interest rate risk? Drechsler et al. (2017a) explain that to maintain their high market concentration and stay high up in financial markets' social strata, banks administer many branches. This high market concentration, or oligopoly, allows banks to keep their deposit rates insensitive to fed funds changes, and it creates high operation costs for the banks (Drechsler et al., 2017a). These costs are fixed and thus the deposits, and so liabilities, are fixed-rate (Drechsler et al., 2017a). Therefore, the bank has fixed-rate assets as well as fixed-rate liabilities (Drechsler et al., 2017a). This allows for the matching of the sensitivity of the interest expense to the interest income, making it possible "to engage in maturity transformation without interest rate risk" (Drechsler et al., 2017a, p. 2).

Di Tella and Kurlat (2017) looked at the relationship between interest rates and bank net worth. They built a theoretical model that displays how risk-averse banks build a dynamic hedging mechanism, which leads to their net worth increasing when interest rates decrease. Their paper shows an inverse relationship between LIBOR and bank net worth; when LIBOR increases by 100 basis points, bank net worth decreases by around 30 percent. Bank net worth, the variable z, is, more precisely, measured as the share of banks'

net worth out of the U.S. economy net worth.[1] This poses the question: could a change in z be just a change in the economy net worth? Di Tella and Kurlat (2017) reassure that since the economy net worth is unaffected by the Brownian motion that is responsible for the model's monetary policy shocks, the shifts in z are equivalent to shifts purely in bank net worth. The dynamic hedging mechanism outlined and modeled by Di Tella and Kurlat (2017) shows that bank net worth actually decreases when rates increase. Di Tella and Kurlat's (2017) empirical work does not include a regression of bank net worth on LIBOR; they only depict this relationship though the theoretical model they construct. Apart from my VAR model of this relationship, I will fill that gap with an OLS regression of bank net worth on LIBOR. It will be in the same vein as the authors' other OLS regressions. This is an especially interesting inquiry because Di Tella and Kurlat (2017) use book values in their existing empirical analysis; an interest rate shock would not lead to a change in book values, so banks' book value net worth should be unchanged after an interest rate shock.

The Di Tella and Kurlat (2017) OLS regressions have an underlying problem. The variables they use are non-stationary (see Table 8 in Appendix), and an OLS regression of nonstationary variables is a spurious regression, meaning its results are invalid (Granger & Newbold, 1974). Granger and Newbold (1974) explain that is because if you regress y on x, and both x and y are nonstationary, the coefficients will be nonzero even if there is no relationship between x and y. They found that even if there is a relationship between x and y, the estimated coefficient will likely be incorrect. I will use the VAR to model these variables because it takes account of the fact that the variables' residuals are correlated (Hamilton, 1994).

In their dissertation, Sung-Eun Yu studied the difference between how banks and non-bank financial institutions react to changes in the fed funds rate (Yu, 2015). They found that an increase in the fed funds rate is associated with a large decrease in bank net worth, which matches the Di Tella and Kurlat (2017) finding (Yu, 2015). They used a log of the net worth variable, and did not convert the IRF standard deviation effects into basis point units (Yu, 2015). They also used Quarterly Flow of Funds data, but their date range is 1954 to 2010 (Yu, 2015).

The high market concentration in the banking industry as an explanation for sticky deposit rates which Drechsler et al. (2017a) cited is supported by Hannan and Berger (1991). Hannan and Berger (1989) say that it is banks' "non-competitive pricing behavior" that is behind this concentration (Hannan & Berger, 1989, p. 291). Hannan and Berger (1991), differ from Drechsler et al. (2017a) in their conclusion about the relationship between the interest rate (in Hannan and Berger's case, the three-month Treasury bill rate) and the deposit rate after doing a multinomial logit regression. Hannan and Berger (1991) found that if the independent variable is the square of the change in the security rate, and the dependent variable is the deposit rate, the coefficient is 2.36 for instances where the security rate increases, and 3.26 when the security rate decreases. However, their analysis is less rigorous than Drechsler et al. (2017a), as they use data on only a select group of 300 banks from 1983-1986 (Hannan & Berger, 1991).

This line of research was continued by Neumark and Sharpe (1992). The 1980s saw structural changes in the deposits market caused by the deregulation of the market in the early part of the decade (Neumark & Sharpe, 1992). Neumark and Sharpe (1992) found that market concentration explains why banks in concentrated markets are quicker to lower deposit rates and slower to raise deposit rates. "Since the interest rate is the inverse of the price charged by the bank for deposits, this suggests more generally that downward price rigidity and upward price flexibility are the consequence of market concentration" (Neumark & Sharpe, 1992, p. 660). Neumark and Sharpe (1992) use a dynamic OLS model. Like Hannan and Berger (1991), Neumark and Sharpe (1992) chose a small sample of 255 banks over a period in the mid-1980s, and they have similar results for the relationship between market concentration and deposit rates.

Driscoll and Judson (2013) concluded that menu costs can explain the price stickiness of deposit rates. "Rates are downwards-flexible and upwards-sticky" (Driscoll & Judson, 2013, p. 1) is a similar finding to Neumark and Sharpe (1992). They clearly point out why this area of research matters in depositor dollar terms because "depositors would have received as much as $100 billion more in interest per year during periods when market rates were rising," and also in the monetary policy setting world because "deposit rates are likely to lag increases in policy and market rates in future tightening cycles" (Driscoll & Judson, 2013, p. 1). The Driscoll and Judson (2013) aggregate-level regressions of the weekly change in the 6-month certificate of deposit (CD) rate on the change in the fed funds rate have similar results to the comparable regressions of Drechsler et al. (2017a), which regress the interest expense rate on the fed funds rate. In summary, Driscoll and Judson (2013) find that for a 100 basis point change in the fed funds rate, the rate on 6-month CD moved by 38 basis points. Drechsler et al. (2017a) find a 36 basis point change in the comparable aggregate deposit rate. Both Driscoll and Judson (2013) and Drechsler et al. (2017a) use OLS regressions. Their branch level results are more difficult to compare because they analyze the deposit rates of different product categories separately (CD rate, MMDA rate, interest checking rate), instead of using one aggregate asset-weighted average deposit rate (Driscoll & Judson, 2013).

Driscoll and Judson (2013) and Yankov (2014) find an asymmetry between the response of the deposit rate to an increase versus a decrease in the fed funds rate. When the fed funds rate increases, it takes around seven weeks for the deposit rates to increase, while when the fed funds rate decreases, it takes around three weeks for the deposit rate

[1] The variable Di Tella and Kurlat (2017) and I study is the ratio of bank net worth to economy net worth. For brevity, I will use "bank net worth" to describe the entire ratio, so "z" is interchangeable with "bank net worth."

to decrease (Yankov, 2014). Yankov (2014) attributes this incongruity to heterogeneous search costs on behalf of the investors. He creates "an asset pricing model with investors who are heterogeneous with respect to their search costs of obtaining information on the best returns on their savings" (Yankov, 2014, p. 3). This builds on the work of Burdett and Judd who show that it is possible to have "price dispersion in equilibrium with fully rational and identical agents on both sides of the market" (Burdett & Judd, 1983, p. 967). The heterogeneity of investor information costs leads to "the degree of interest rate pass-through of market interest rates onto retail deposit rates" (Yankov, 2014, p. 3). In contrast, Driscoll and Judson (2013) attribute this asymmetry to the bank's menu costs. Banks will change their deposit rate more aggressively when it is worth the menu cost. When the fed funds rate rises, banks are reluctant to raise the rate they pay on deposits, but they are quicker to adjust when the fed funds rate falls and they have the chance to pay less on deposits (Driscoll & Judson, 2013). They also believe that in general, stickiness of deposits may contribute to financial stability (Driscoll & Judson, 2013).

III. Methodology

I start with a replication of the regression results in Di Tella and Kurlat (2017) and Drechsler et al. (2017a). This will allow me to seamlessly test and extend their findings with a VAR model. Both papers provided good documentation of their methodology. In the Appendix, macroeconomic variables refers to the variables (LIBOR, the deposit spread, and z) inspired by Di Tella and Kurlat (2017), and bank balance sheet variables refers to the variables (Fed Funds rate, interest income rate, interest expense rate, ROA) inspired by Drechsler et al. (2017a).

From Di Tella and Kurlat (2017), I replicated their original Table 2 and Table 3 (as seen in Appendix Table 3 and Table 5). Table 2 in Di Tella and Kurlat (2017) is an OLS regression of the deposit spread on LIBOR and bank net worth (variable z) (Di Tella & Kurlat 2017). Table 3 in Di Tella and Kurlat (2017) is a regression of the deposit spread on LIBOR and bank maturity mismatch; even though I do not discuss maturity mismatch in this paper, this replication is useful to further confirm the validity of my data replication overall (Di Tella & Kurlat 2017).

From Drechsler et al. (2017a) I replicate parts of their Table 1, which is Table 8 in this Appendix, and I discuss their original Table 2 and 3. Table 1 of Drechsler et al. (2017a) is a cross-sectional analysis of the interest income, interest expense betas, and ROA betas; the interest income beta is the bank interest income divided by assets and the other two betas are calculated in the same manner (Drechsler et al., 2017a). Table 2 and 3 of Drechsler et al. (2017a) hold the results of the following regressions:

Table 2 of Drechsler et al. (2017a):

$$\Delta IntExp_{i,t} = \alpha_i + \sum_{\tau=0}^{3} \beta_{i,\tau} \Delta FedFunds_{t-\tau} + \epsilon_{i,t} \qquad \text{(Stage 1)}$$

$$\Delta IntInc_{i,t} = \lambda_i + \sum_{\tau=0}^{3} \gamma_\tau \Delta FedFunds_{t-\tau} + \delta \Delta \widehat{IntExp_{i,t}} + \varepsilon_{i,t} \qquad \text{(Stage 2)}$$

(Drechsler et al., 2017a, p. 34)

Table 3

$$\Delta IntExp_{i,t} = \alpha_i + \sum_{\tau=0}^{3} \beta_{i,\tau} \Delta FedFunds_{t-\tau} + \epsilon_{i,t} \qquad \text{(Stage 1)}$$

$$\Delta ROA_{i,t} = \lambda_i + \sum_{\tau=0}^{3} \gamma_\tau \Delta FedFunds_{t-\tau} + \delta \Delta \widehat{IntExp_{i,t}} + \varepsilon_{i,t} \qquad \text{(Stage 2)}$$

(Drechsler et al., 2017a, p. 35)

Drechsler et al. (2017a) state that they test a two-stage OLS regression; however, the equations above do not fit that description. The fed funds rate cannot appear in both the first and second stage of a two-stage OLS regression (Wooldridge, 2001). If the fed funds rate is acting as the instrument variable (IV), it cannot appear in the second stage (Wooldridge, 2001). If it does, it defeats the purpose of the fed funds as an IV since it is correlated not just with $\Delta IntExp_{i,t}$, but also with $\Delta IntInc_{i,t}$ (Wooldridge, 2001).

A. VAR Model

To better understand the relationship between the nominal interest rate, the deposit spread, and bank net worth, I am using a reduced form VAR model. According to Sims (1980), VAR models show the dynamic response of a vector of variables. Hence, I can analyze the response of only bank net worth to a shock in only LIBOR over multiple periods, which is in contrast to the static models of Di Tella and Kurlat (2017) and Drechsler et al. (2017a). A VAR model also gets rid of definite classifications of exogenous and endogenous variables (Sims, 1980). Drechsler et al. (2017a) and Di Tella and Kurlat (2017) strictly look at the nominal interest rate as exogenous and the deposit spread as endogenous. Variables are sometimes considered exogenous because proving their endogeneity is not the focus of the specific study or because they are policy variables (federal funds rate in this case), however, much can be discovered if this *a priori* assumption is lifted (Sims, 1980). A VAR prevents one from running "nominally over-identified" models (Sims, 1980, p. 14). The VAR formula is:

$$\boldsymbol{y}_t = A_1 \boldsymbol{y}_{t-1} + ... + A_p \boldsymbol{y}_{t-p} + \boldsymbol{u}_t$$

(Pfaff, 2008, p. 2)

There are "K endogenous variables $\boldsymbol{y}_t = (y_{1t}, ..., y_{kt})$ for $k = 1, ..., K$. A_i are $(K \times K)$ coefficient matrices for $i = 1, ..., p$ and $\boldsymbol{u}_t$ is a K-dimensional process with

$E(\boldsymbol{u}_t) = \mathbf{0}$ and time invariant positive definite covariance matrix $E(\boldsymbol{u}_t \boldsymbol{u}_t^T) = \Sigma_{\boldsymbol{u}}$." (Pfaff, 2008, p. 2).

My endogenous variables are the weekly 6-month LIBOR rate, the quarterly aggregate deposit spread (LIBOR minus aggregate rate on deposits), and quarterly aggregate bank net worth, all of which are defined as in Di Tella and Kurlat (2017), except I use the variables' first-difference since they are nonstationary.

My endogenous variables are the weekly 6-month LIBOR rate, the quarterly aggregate deposit spread (LIBOR minus aggregate rate on deposits), and quarterly aggregate bank net worth, all of which are defined as in Di Tella and Kurlat (2017), except I use the variables' first-difference since they are nonstationary.

To test stationarity, I evaluate the Eigenvalues of the entire VAR model (Li, 2014). If they are within the unit circle—if their absolute value is below one—the model is stable and the VAR analysis can begin (Li, 2014). To test the stationarity of the individual variables, I use the Augmented Dickey-Fuller test, which is a hypothesis test; the null hypothesis is that the model is non-stationary (Said & Dickey, 1984). The VAR model of the Di Tella and Kurlat (2017) variables as a whole is stationary, and the same is true for the VAR model of Drechsler et al. (2017a) variables. However, according to the Augmented Dickey-Fuller (ADF) Test (Appendix Table 11), the Di Tella and Kurlat (2017) individual variables are non-stationary. Therefore, I use first-differences of the variables of Di Tella and Kurlat (2017). On the other hand, the ADF Test calculates that multiple of the individual Drechsler et al. (2017a) variables are stationary (Appendix Table 15). Therefore, I do not use the first-differences of the Drechsler et al. (2017a) variables. STATA and R provide the model's Eigenvalues, and the theory is explained by Hamilton (1994) as follows:

Consider multiplying the VAR equation by $\boldsymbol{\rho}$.

$$\boldsymbol{y}_t = \boldsymbol{\rho}\boldsymbol{y}_{t-1} + ... + \boldsymbol{\rho}\boldsymbol{y}_{t-p} + \boldsymbol{u}_t$$

Where $\boldsymbol{\rho}$ is a $K \times K$ matrix that satisfies:

$$\boldsymbol{\rho}\boldsymbol{y} = \boldsymbol{\lambda}\boldsymbol{y}$$

and:

$$\begin{aligned}\boldsymbol{\rho}^2 &= \boldsymbol{P}\begin{bmatrix}\lambda_1 & \dots & 0\\ \vdots & \ddots & \vdots\\ 0 & \dots & \lambda_k\end{bmatrix}\boldsymbol{P}^{-1} * \boldsymbol{P}\begin{bmatrix}\lambda_1 & \dots & 0\\ \vdots & \ddots & \vdots\\ 0 & \dots & \lambda_k\end{bmatrix}\boldsymbol{P}^{-1}\\ &= \boldsymbol{P}\begin{bmatrix}\lambda_1^2 & \dots & 0\\ \vdots & \ddots & \vdots\\ 0 & \dots & \lambda_k^2\end{bmatrix}\boldsymbol{P}^{-1}\end{aligned}$$

When the set of non-current-error terms of $\boldsymbol{y_t}$ is continuously multiplied by $\boldsymbol{\rho}$:

$$\begin{aligned}\boldsymbol{y}_t &= \boldsymbol{\rho}\boldsymbol{y}_{t-1} + ... + \boldsymbol{\rho}\boldsymbol{y}_{t-p} + \boldsymbol{u}_t\\ &= \boldsymbol{\rho}(\boldsymbol{\rho}\boldsymbol{y}_{t-1} + ... + \boldsymbol{\rho}\boldsymbol{y}_{t-p} + \boldsymbol{u}_{t-1}) + \boldsymbol{u}_t\\ &= \boldsymbol{\rho}^2\boldsymbol{y}_{t-1} + ... + \boldsymbol{\rho}^2\boldsymbol{y}_{t-p} + \boldsymbol{\rho}\boldsymbol{u}_{t-1} + \boldsymbol{u}_t\\ &= \boldsymbol{\rho}^n\boldsymbol{y}_{t-n} + \sum_{s=0}^{n-1}\boldsymbol{\rho}^s\boldsymbol{u}_{t-s}\end{aligned}$$

If $|\boldsymbol{\rho}| < 1$, then:

$$\begin{aligned}y &= \sum_{s=0}^{\infty}\boldsymbol{\rho}^s\boldsymbol{u}_{t-s}\\ &= E[\sum_{s=0}^{\infty}\boldsymbol{\rho}^{2s}\boldsymbol{u}_{t-s}^2]\\ &= \sum_{s=0}^{\infty}\boldsymbol{\rho}^{2s}\boldsymbol{\sigma}_u^2\\ &= \frac{\boldsymbol{\sigma}_{\boldsymbol{u}}^2}{\mathbf{1}-\boldsymbol{\rho}^2}\end{aligned}$$

(Hamilton, 1994)

$\boldsymbol{y}_t$ is stable and is a function of the Eigenvalue (Hamilton, 1994). If $|\boldsymbol{\rho}| \geq 1$ then the first term of the equation ($\boldsymbol{\rho}^n\boldsymbol{y}_{t-n}$) would not go to zero as n goes to infinity; the system of equations would be unstable, and the VAR model would not function correctly (Hamilton, 1994).

The VAR model automatically determines an appropriate lag length but also provides the metrics from which the researcher can choose the lag length. To determine the lag length of the VAR model I evaluate the Akaike (AIC), Hannan-Quinn (HQIC), and Schwarz Bayesian (SBIC) information criterion values which are determined within the bounds of a specific maximum lag and an information criterion (Pfaff, 2008). AIC suggests a lag length that will give the best forecast (Lutkepohl, 2005). HQIC and SBIC are a good fit for large data sets and fit the true order of the data better (Lutkepohl, 2005).

"Autoregressive systems like these are difficult to describe succinctly. It is especially difficult to make sense of them by examining the coefficients in the regression equations themselves. The estimated coefficients on successive lags tend to oscillate, and there are complicated cross-equation feedbacks" (Sims, 1980, p. 20). To filter through the cross-equation feedback, I will analyze the VAR's impulse response functions (IRFs) (Hamilton 1994). IRFs depict how the shock of one variable has an effect on itself and, separately, on another variable(s) (Hamilton 1994). This is possible by the Cholesky decomposition of the system's error terms. If the VAR's matrix form is:

$$\begin{matrix}y_t\\ x_t\end{matrix} = \begin{pmatrix}\phi_{11} & \phi_{12}\\ \phi_{21} & \phi_{22}\end{pmatrix}\begin{matrix}y_{t-1}\\ x_{t-1}\end{matrix} + \begin{matrix}u_t\\ v_t\end{matrix}$$

$$w_t = \begin{pmatrix}u_t\\ v_t\end{pmatrix}$$

(Li, 2014, p. 4)

The error vector's variance-covariance matrix is

$$\Omega = Ew_t w_t' = \begin{pmatrix} \sigma_u^2 & \sigma_{u,v} \\ \sigma_{u,v} & \sigma_v^2 \end{pmatrix}$$

(Li, 2014, p. 6)

Which is transformed

$$\widehat{\Omega} = T^{-1} = \begin{pmatrix} \Sigma \hat{u}_t^2 & \Sigma \hat{u}_t \hat{v}_t \\ \Sigma \hat{u}_t \hat{v}_t & \Sigma \hat{v}_t^2 \end{pmatrix}$$

$$\hat{u}_t \text{ and } \hat{v}_t = \text{ residuals}$$

(Li, 2014, p. 6)

In general, u_t and v_t are contemporaneously correlated (not-orthogonal), i.e., $\sigma_{u,v} \neq 0$ (Li 2014, 12). Under this condition, if there is a shock on y_t, it wont be clear if x_t responds to y_t or x_{t-1} Hamilton 1994. This is remedied with the Cholesky Decomposition as explained by Hamilton (1994) and Li (2014):

There is a lower triangular matrix A such that

$$\Omega = AA^{-1}$$

(Li, 2014, p. 12)

Linearly transform w_t

$$\widetilde{w_t} = A^{-1} w_t$$

(Li, 2014, p. 12)

$$\widetilde{w_t} = \begin{pmatrix} g & 0 & 0 \\ h & \ddots & 0 \\ k & \cdots & \end{pmatrix}$$

where g is the initial error term of the first equation, h is the error term of the second equation with parts of the first equation error subtracted out, k is the error term of the third equation with parts of the first and second equation errors subtracted out and so on (Hamilton, 1994).

The error terms of $\widetilde{w_t}$ are orthogonal as its variance-covariance matrix is an identity matrix:

$$\begin{aligned} var(\widetilde{w_t}) &= A^{-1} var(w_t) A^{-1'} \\ &= A^{-1} \Omega A^{-1'} \\ &= A^{-1} A A' A^{-1'} \\ &= I \end{aligned}$$

(Li, 2014, p. 12)

The impulse responses show the shocks in the errors in the $\widetilde{w_t}$ vector over time (Hamilton 1994). Once the error terms of $\widetilde{w_t}$ are orthogonal, I am able to distinguish between the effects of an error shock (Hamilton, 1994).

The IRF graph displays a one standard deviation shock of the impulse variable's residual leading to a unit response in the response variable (Hamilton, 1994). To understand the effect of the impulse variable on the response variable in the units of the actual variables, I convert them. The effect of the impulse variable on the response variable is calculated by dividing the highest absolute value that the response variable reaches by the standard deviation of the impulse variable's residual (Hamilton, 1994).

B. Error Bands

Different programs in R draw impulse response function graphs with different types of error bands. The more classic programs use bootstrap confidence interval methods and others use the Bayesian interval method (Sims & Zha, 1999). I analyze both.

The two methods have similar results if the data is stationary, but the Bayesian method is preferred if the data is nonstationary (Sims & Zha, 1999). The bootstrap confidence interval method is a frequentist procedure which "mix[es] the information about parameter location with information about model fit" (Sims & Zha 1999, p. 1113). Confidence interval methods do not express the validity model, rather they simply show "the location of the parameter values" (Sims & Zha 1999, p. 1117). This can create inaccurate results (Sims & Zha, 1999). Likelihood-based error bands drawn from Bayesian inference use the shape of the likelihood can avoid this (Sims & Zha, 1999). "Flat-prior Bayesian $1 - \alpha$ probability intervals, based purely on the likelihood, are often very close to exact $(1 - \alpha)\%$ confidence intervals" and if the cases that they are not, one should use the Bayesian probability interval (Sims & Zha, 1999, p. 1120). In the IRF graphs, I use the intervals .68 and .90 to create the error bands (Sims & Zha, 1999).

IV. Data

Following Di Tella and Kurlat (2017), I used data from FRED, from the Federal Reserve Board, and from Drechsler et al. (2017b). The weekly 6-month LIBOR rate is from FRED, the aggregate bank net worth data is from the Quarterly Flow of Funds database provided by the Federal Reserve Board, and the aggregate deposit spread is directly from the published data of Drechsler et al. (2017b) in the Harvard Dataverse (Drechsler et al, 2017c). Aggregate bank net worth data is measured by bank equity (Di Tella and Kurlat, 2017). Di Tella and Kurlat (2017) provide the specific data series from the particular databases in their Appendix C. In the Di Tella and Kurlat (2017) Table 3 regressions, the authors used data from 1997-2016. I only used data from 1997-2013 in the replication of their Table 3 (Appendix Table 5) as most observations past 2013 had missing data. Any other missing data is also accounted for while running the VAR model with a command to omit the missing observations. Variable definitions can be found in Table 1 of this Appendix.

I ran a two variable VAR with the variables LIBOR and the deposit spread which used the weekly rates data. To run the three variable VAR with LIBOR, the deposit spread, and z, I collapse the LIBOR and deposit spread variables into a

quarterly frequency to match z which also has a quarterly frequency (Board of Governors of the Federal Reserve System). To transform the LIBOR and deposit spread weekly variables into quarterly variables, I choose the last value of the quarter to represent that quarter because the z variable is also a value measured at the end of the quarter (Board of Governors of the Federal Reserve System). Once transformed into aggregate time series, the variables are ready as inputs into the VAR model (Hamilton 1994). The summary statistics for these variables (LIBOR, deposit spread, z) are found in Table 6 of this Appendix.

Drechsler et al. (2017a) uses bank-level U. S. Call Report Data that I retrieve as a dataset from the website of one of the authors, Phillip Schnabl (Drechsler et al., 2017d). Therefore, I transform them into time series data in order to model their relationship with a VAR model (Hamilton 1994). To do this I collapse them into an average per quarter. I use data from 1984-2013 like Drechsler et al. (2017a), though some 2013 values are missing, which I account for while running the VAR model with a command to omit the missing observations.

Each variable in Drechsler et al. (2017a) is measured as a change from t to t+1 (except ROA, which is measured as a year-over-year change). The IRFs of such variables would then represent the change in a change of the variable which is not an intuitive measurement. Thus, instead of using the change variable like Drechsler et al. (2017a), I use the non-change, level variables in the VAR model.

I also transform the Drechsler et al. (2017a) variables by winsorizing them at the 1 percent and 99 percent level. Winsorizing the variable at 1 percent means that the top 1 percent of observations take on the value of the observation at exactly the 99th percentile, and the bottom 1 percent of observations take on the value of the observation at exactly the 1st percentile (Reifman & Keyton, 2012). This was done as a response to some very negative outliers, which were making the interest income, interest expense, and ROA averages negative.[2] Interest expense, interest income and ROA should not be negative, and after digging through the individual data observations, it seems that the outliers were among small banks that were not in existence for a long time and took enormous losses relative to their assets. For example, before winsorizing each of the three variables, the average overall interest income rate was -.02, while the average interest income rate if interest income rate was below zero was -11.41 and the average interest income rate if interest income rate was above zero was .02. These statistics are even more shocking considering there are only 4,000 observations with interest income rate below zero and over 1.1 million observations with interest income rate above zero. This leads to the conclusion that a small number of very negative observations were greatly affecting the sample. It would be interesting how the Drechsler et al. (2017a) results would change if they winsorized the variables in their OLS regressions.

[2]This is the ready-made bank-level data from Drechsler et al. (2017d).

Lastly, a graph of the aggregate time series that includes LIBOR, the deposit spread and z is found in Figure 1, and an aggregate time series graph of the Fed Funds rate, interest income rate, interest expense rate, and ROA is found in Figure 2.

V. Results

For analyzing the LIBOR-deposit spread relationship, I use the two variable VAR model with just those two variables. The two variable VAR model has weekly LIBOR and deposit spread data, which allows for greater granularity in calculating the relationship. The three variable VAR with LIBOR, deposit spread, and z, uses quarterly data since z (bank net worth variable) is reported quarterly (Board of Governors of the Federal Reserve System). Although the three-variable VAR does contain LIBOR and the deposit spread, it should not be used for analyzing the LIBOR-deposit spread relationship because the quarterly LIBOR and deposit spread observations gloss over much of the variation that they exhibit within a quarter.

Based on OLS regressions, both Di Tella and Kurlat (2017) and Drechsler et al. (2017a) agree that a 100 bps increase in LIBOR leads to around a 63 bps change in the deposit spread or the interest expense rate, respectively.[3] My VAR model of Di Tella and Kurlat (2017) data shows similar results; a 100 bps increase in LIBOR is associated with a 48 bps change in the deposit spread (Figure 3).[4] This means that when LIBOR increases by 100 bps, the deposit rate increases by only 52 bps, confirming the Drechsler et al. (2017a) bank oligopoly theory.

A VAR model of Di Tella and Kurlat (2017) data produces some results that match and some results that differ from the Di Tella and Kurlat (2017) OLS regressions. The Di Tella and Kurlat (2017) theoretical model states that a 100 bps increase in LIBOR is associated with a 30 percent decrease in bank net worth. Di Tella and Kurlat (2017) do not actually test this empirically. I tested this relationship using both an OLS regression and a VAR model, which have similar results to each other, and greatly differ from Di Tella and Kurlat (2017). The OLS regression of z on LIBOR indicates that a 100 bps increase in LIBOR is associated with a 2 basis point change in banks' share of net worth. Since the mean value of z is .006, a 100 bps change in LIBOR is associated with a 3 percent increase in the average bank's share of net worth. The VAR model shows that a 100 bps increase in LIBOR leads to a 6 basis point increase in banks' share of net worth (Figure 4). This is equivalent to a 100 bps increase in LIBOR being associated with a 10 percent increase in the average bank net worth. This means that an increase in LIBOR is associated with an increase in bank net worth. This contradicts Di Tella and Kurlat (2017) which found a

[3]63 bps is the average of the two papers' findings, 62 bps and 64 bps, found by Di Tella and Kurlat (2017) and Drechsler et al. (2017a), respectively.

[4]This is a result of the two-variable VAR with LIBOR and the deposit spread variables. The three-variable spread VAR shows a 100 bps increase in LIBOR leads to a 65 bps change in the deposit spread.

much larger negative impact of LIBOR on banks' share of net worth.

Drechsler et al. (2017a) concluded that a 100 bps change in the Fed Funds rate changes interest expense by 36 bps and interest income by 38 bps. In comparison to the authors' OLS regressions, my VAR model shows that a 100 bps change in the Fed Funds rate is associated with a 22 bps change in the interest expense rate. This effect is seen by quarter eight, as shown in Figure 5.[5] This is important to note: the full effect of a Fed Funds shock on interest expense is not felt until 2 years later. Thus, a VAR model is critical to understanding this relationship because an OLS regression, even with a 4 quarter lag, does not fully depict this relationship. Next, the VAR shows that the effect of a 100 bps change in the Fed Funds rate is associated with around a 24 bps change in the interest income rate (Figure 5). [6] This is similar, though slightly lower, compared to Drechler et al. (2017a) results. The full effect is also not felt until about quarter eight. Like in Drechsler et al. (2017a), the VAR model shows that interest income increases slightly more than interest expense after a Fed Funds shock. Intuitively, it makes sense that when interest expenses increase, the interest income increases by more than that to insure the bank stays profitable. The third relationship in this study shows that a 100 bps shock in the Fed Funds rate is associated with a 3 bps change in ROA (Figure 6). The VAR model shows that ROA is more or less unchanged after a shock in the Fed Funds rate, which matches the result of Drechsler et al. (2017a), who stated that ROA remains constant during changes in the Fed Funds rate.

Besides confirming to or disagreeing with the two papers, these findings show that following an increase in the nominal interest rate, the rate banks pay on deposits increases by much less than the interest rate shock. Interest expense is also sticky, and it has the same sensitivity to interest rate shocks as the interest income rate. All of these variables confirm the Drechsler et al. (2017a) theory that banks have outsized power in setting the deposit rate, and the rate paid on other expenses. Beyond these rates, a nominal interest rate shock does not have much effect on ROA nor bank net worth. This is surprising because banks have previously been seen as being exposed to interest rate risk through their maturity transformation business model (Drechsler et al., 2017a).

A future study could look at why, in the IRFs of the VARs, the relationship between Fed Funds and itself, or interest expense and itself or, interest income and itself are not each equal to one. It would make intuitive sense for them to equal one: the effect of a 1 unit shock in the Fed Funds rate should affect the Fed Funds rate by around 1 unit in every VAR. This is not exactly the case. It maybe because the data itself is "imperfect;" banks have negative values of ROA and other variables that should be positive, whether for bank-level reasons or the time period (2008Q4 or 2009Q4). This, along with outliers, greatly affects the VAR model, which was seen with the pre-winsorized data that had nonsensical IRF results. Future studies could also explore how bank risk-aversion and hedging play into Drechsler et al. (2017a) results since the authors do not mention either while Di Tella and Kurlat (2017) do.

VI. Conclusion

Studying interest rate shocks on bank balance sheet variables gives insight into both Federal Reserve monetary policy and how receptive banks are to changes in interest rates. Di Tella and Kurlat (2017) studied the relationship between LIBOR, the deposit spread, and bank net worth with a theoretical model and OLS regressions, and Drechsler et al. (2017a) studied the relationship between the Fed Funds rate, interest expense rate, interest income rate, and ROA with OLS regressions. I study the relationship between these variables with a VAR model to understand how they interact in a dynamic setting over multiple periods of time without assumptions of exogeneity, and with clear effects of shocks in one variable on another through impulse response functions (Hamilton 1994). I find that an increase in the nominal interest rate is associated with a much smaller increase in the rate banks pay on deposits and other liabilities, which matches both papers' conclusions. Drechsler et al. (2017a) attribute this to the fact that banks have oligopoly power and can choose to make deposit rates sticky. Like Drechsler et al. (2017a), I also find that an interest rate change does not affect ROA. In contrast to Di Tella and Kurlat (2017) though, I find that an interest rate shock does not affect bank net worth either. Drechsler et al. (2017a) state how it was previously thought that interest rate risk is a necessary part of maturity transformation; my VAR result shows that the banking industry is less affected by interest rate shocks than previously thought.

VII. Acknowledgements

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[5] 22 bps is the average effect of a shock in the Fed Funds rate on the interest expense rate across the three VAR models (the first VAR included the variables Fed Funds rate, interest expense and interest income, the second VAR model included the variables Fed Funds, interest expense, and ROA, and the third VAR had the variables Fed Funds, interest expense, interest income, and ROA.)

[6] 24 bps is the average effect of the Fed Funds rate on the interest income rate across the VAR models

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APPENDIX

IX. A. Variable Definitions

Table 1. Variable Definitions

LIBOR	6-month LIBOR rate
Deposit Spread (DSpr)	Difference between LIBOR and the rate banks pay on deposits
z	Banks share of net worth= Banks Net Worth / Aggregate Economy Net Worth
Fed Funds Rate	Federal Funds Rate
Interest Expense Rate	Interest Expense/Assets
Interest Income Rate Interest	Income/Assets
ROA	Return on Assets= Net Income/Assets

IX. B. My replication of Di Tella and Kurlat (2017)

Table 2. Original Table 2 in Di Tella and Kurlat (2017)

	(1)	(2)
Constant	0.3%	−0.3%
	(0.22%)	(0.44%)
i	0.66	0.98
	(0.028)	(0.17)
i^2	–	−4.07
		(2.02)
z	−0.99	−0.71
	(0.25)	(0.32)
R^2	0.89	0.90
N	430	430

(Di Tella and Kurlat, 2017, p. 19)

Table 3. My replication of Table 2 in Di Tella and Kurlat (2017)

Variables	(1) Deposit Spread	(2) Deposit Spread
LIBOR	0.639***	1.055***
	(0.0111)	(0.0605)
LIBOR^2		-5.416***
		(0.775)
z	-0.756***	-0.255
	(0.195)	(0.200)
Constant	0.00221	-0.00750***
	(0.00149)	(0.00199)
Observations	511	511
R-squared	0.870	0.881

Standard errors in parentheses
***p<0.01, ** p<0.05, *p<0.1

Table 4. Original Table 3 in Di Tella and Kurlat (2017)

	(1)
Constant	4.4
	(0.1)
i	−11.7
	(6.8)
z	−1.9
	(0.4)
R^2	0.63
N	78

(Di Tella and Kurlat, 2017, p. 28)

Table 5. My replication of Table 3 in Di Tella and Kurlat (2017)

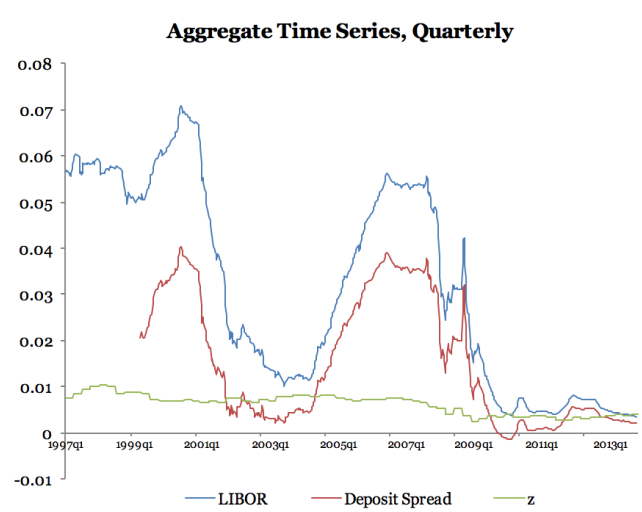
Variables	(1) Maturity Mismatch (years)
LIBOR	0.639***
	(0.0111)
z	-1.900***
	(0.255)
Constant	4.722***
	(0.179)
Observations	67

Standard errors in parentheses
***p<0.01, ** p<0.05, *p<0.1

Table 6. Summary of the replicated variables, quarterly

Variables	(1) N	(2) mean	(3) sd	(4) min	(5) max
LIBOR	68	0.0307	0.0221	0.00345	0.0692
DSpr	60	0.0149	0.0136	-0.00118	0.0388
z	68	0.00622	0.00212	0.00236	0.0104

Figure 1. Aggregate time series of macroeconomic variables, quarterly



IX. C. My replication of Drechsler et al. (2017a)

Table 7. Original Table 1 in Drechsler et al. (2017a)

Panel A: All banks	All (1)	All (2)	Low beta (3)	High beta (4)
Interest rate sensitivity				
Interest expense beta	0.360	(0.096)	0.283	0.436
Interest income beta	0.379	(0.147)	0.320	0.437
ROA beta	0.039	(0.150)	0.038	0.040
Bank characteristics				
Asset repricing maturity	3.360	(1.580)	3.588	3.088
Liabilities repricing maturity	0.441	(0.213)	0.462	0.416
Log assets	4.232	(1.275)	3.969	4.494
Loans/Assets	0.585	(0.132)	0.566	0.603
Securities/Assets	0.246	(0.131)	0.267	0.225
Core Deposits/Assets	0.732	(0.115)	0.751	0.713
Equity/Assets	0.097	(0.036)	0.101	0.091
Observations	18,552		9,276	9,276

(Drechsler et al., 2017a, p. 33)

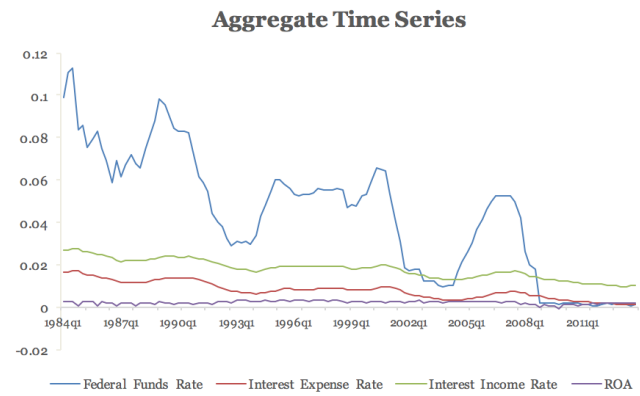
Table 8. My replication of Table 1 in Drechsler et al. (2017a)

Variables	Bank Characteristics (all)
Interest rate sensitivity	
Interest expense beta	0.356 (0.109)
Interest income beta	0.372 (0.177)
ROA beta	.028 (0.779)
Bank Characteristics	
Loans/Assets	0.575 (0.162)
Securities/Assets	0.272 (0.157)
CoreDeposits/Assets	0.852 (0.100)
Equity/Assets	0.103 (0.063)
Observations	15,309

Table 9. Summary Statistics of the replicated variables, Level Variables, Winsorized

Variables	(1) N	(2) mean	(3) sd	(4) min	(5) max
Fed Funds	120	0.0433	0.0292	0.000700	0.113
Interest Expense Rate	120	0.00786	0.00408	0.00115	0.0169
Interest Income Rate	120	0.0179	0.00460	0.00967	0.0276
ROA	120	0.00210	0.000782	-0.000641	0.00315

Figure 2. Aggregate time series of bank balance sheet variables, quarterly



IX. D. Regression of z on LIBOR and deposit spread

(Di Tella and Kurlat (2017) did not test this.)

Table 10. Regression of z on LIBOR and deposit spread

Variables	(1) z
LIBOR	0.0181*** (0.00677)
DSpr	-0.0379*** (0.00979)
Constant	0.00710*** (0.000109)
Observations	511
R-squared	0.041

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

IX. E. VAR model of LIBOR, Deposit Spread, and z

Table 11. Augmented Dickey-Fuller tests of the individual macroeconomic variables

Alternative hypothesis: stationary

Variable	Dickey-Fuller	Lag order	P-value
LIBOR	-1.1451	2	0.9153
DSpr	-1.9257	2	0.6046
z	-1.9136	2	0.6095

We cannot reject the null hypothesis that the variables have a unit root.

IX. F. VAR IRFs of LIBOR, Deposit Spread, and z

Table 12. IRF of LIBOR and Deposit Spread

Impulse	Response	Effect
2 variable VAR		
LIBOR	LIBOR	.98
LIBOR	DSpr	.48
DSpr	LIBOR	.27
DSpr	DSpr	.43

Table 13. IRF of LIBOR, Deposit Spread, and z

Impulse	Response	Effect
LIBOR	LIBOR	1.16
LIBOR	DSpr	0.65
LIBOR	z	0.06
DSpr	LIBOR	0.21
DSpr	DSpr	0.46
DSpr	z	-0.08
z	LIBOR	-0.41
z	DSpr	0.41
z	z	0.76

Table 14. Standard deviation of the error of each macroeconomic variable

2 variable VAR, weekly	
LIBOR	0.00178
Deposit Spread	0.00093
3 variable VAR, quarterly	
LIBOR	0.00775
Deposit Spread	0.00483
z	0.0008

IX. G. VAR model of Fed Funds Rate, Interest Expense Rate, Interest Income Rate, and ROA

Table 15. Augmented Dickey-Fuller tests of individual bank balance sheet variables

Alternative hypothesis: stationary

Variable	Dickey-Fuller	Lag order	p-value
Fed Funds Rate	-3.6887	2	0.03
Interest Expense Rate	-3.8338	2	0.02
Interest Income Rate	-3.1943	2	0.09
ROA	-2.4878	2	0.37

The original Drechsler et al. variables were all measured as a change from t to $t+1$. For use in my VAR model, I modified the variables into levels. The statistics above reflect the level variables.

The fed funds rate, and interest expense rate are stationary.

IX. H. VAR IRFs of Fed Funds, Interest Expense, Interest Income, and ROA

Table 16. IRF of Fed Funds, Interest Expense, and Interest Income

Impulse	Response	Effect
Fed Funds	Fed Funds	2.09
Fed Funds	Interest Expense	0.22
Fed Funds	Interest Income	0.23
Interest Expense	Fed Funds	-3.57
Interest Expense	Interest Expense	1.64
Interest Expense	Interest Income	1.79
Interest Income	Fed Funds	-4.55
Interest Income	Interest Expense	-0.91
Interest Income	Interest Income	0.68

Table 17. IRF of Fed Funds, Interest Expense, and ROA

Impulse	Response	Effect
Fed Funds	Fed Funds	2.27
Fed Funds	Interest Expense	0.23
Fed Funds	Interest Income	0.03
Interest Expense	Fed Funds	-7.69
Interest Expense	Interest Expense	1.92
Interest Expense	Interest Income	-0.77
ROA	Fed Funds	12.50
ROA	Interest Expense	1.50
ROA	Interest Income	1.45

Table 18. IRF of Fed Funds, Interest Expense, Interest Income, and ROA

Impulse	Response	Effect
Fed Funds	Fed Funds	2.19
Fed Funds	Interest Expense	0.22
Fed Funds	Interest Income	0.24
Fed Funds	ROA	0.02
Interest Expense	Fed Funds	-16.67
Interest Expense	Interest Expense	1.67
Interest Expense	Interest Income	1.67
Interest Expense	ROA	-1.17
Interest Income	Fed Funds	-5.26
Interest Income	Interest Expense	-1.05
Interest Income	Interest Income	0.53
Interest Income	ROA	0.74
ROA	Fed Funds	13.16
ROA	Interest Expense	1.32
ROA	Interest Income	1.32
ROA	ROA	1.42

Table 19. Standard deviation of the error of each bank balance sheet variable

IRF of Fed Funds, Interest Expense, and Interest Income	
Fed Funds Rate	0.0043
Interest Expense Rate	0.00014
Interest Income Rate	0.00022
IRF of Fed Funds, Interest Expense, and ROA	
Fed Funds Rate	0.0044
Interest Expense Rate	0.00013
ROA	0.0002
IRF of Fed Funds, Interest Expense, Interest Income, and ROA	
Fed Funds Rate	0.00411
Interest Expense Rate	0.00012
Interest Income Rate	0.00019
ROA	0.00019

IX. I. VAR IRF Graphs of Macroeconomic Variables
Figure 3. Accompanies Table 12. IRF graph of LIBOR and Deposit Spread, weekly

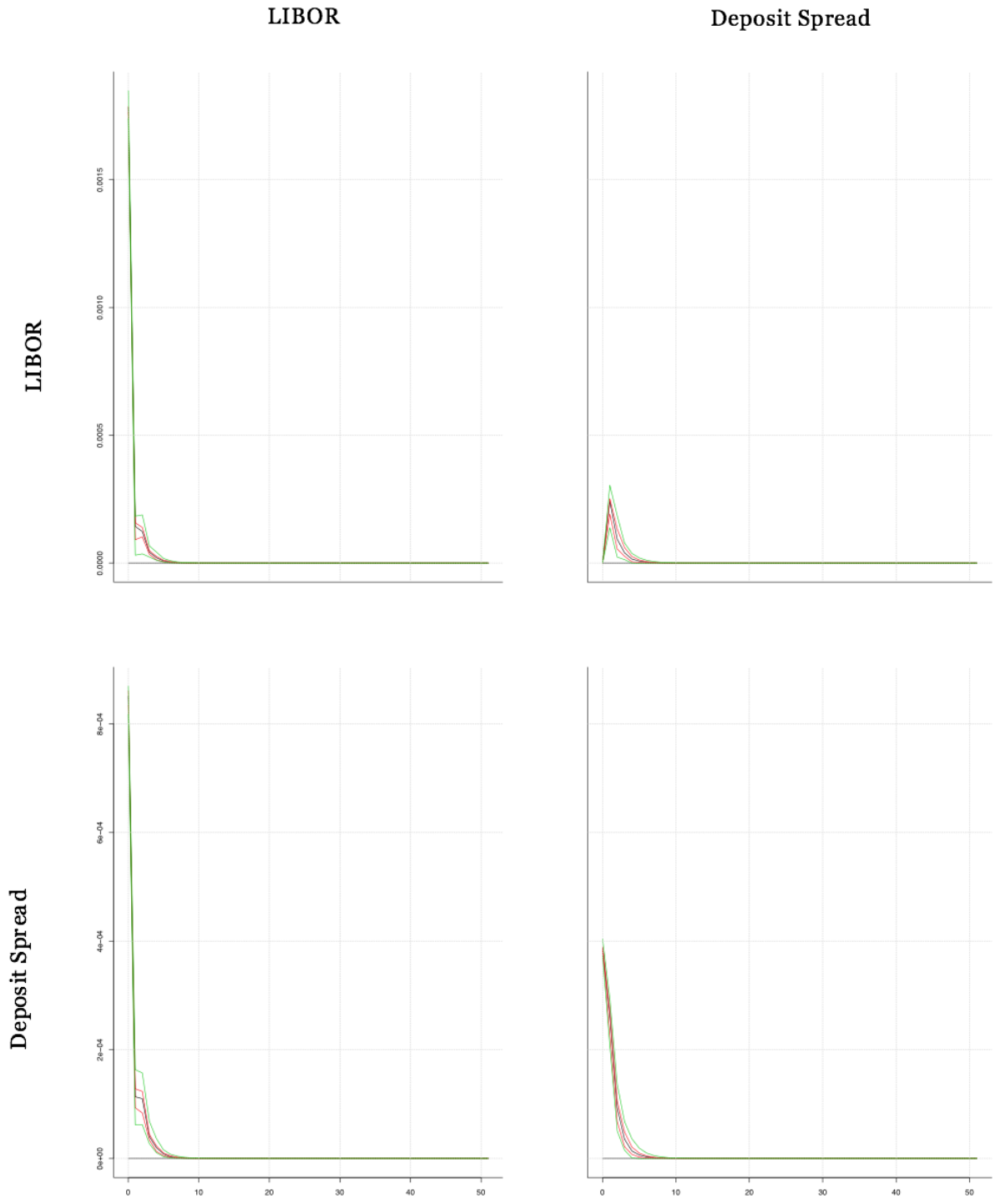
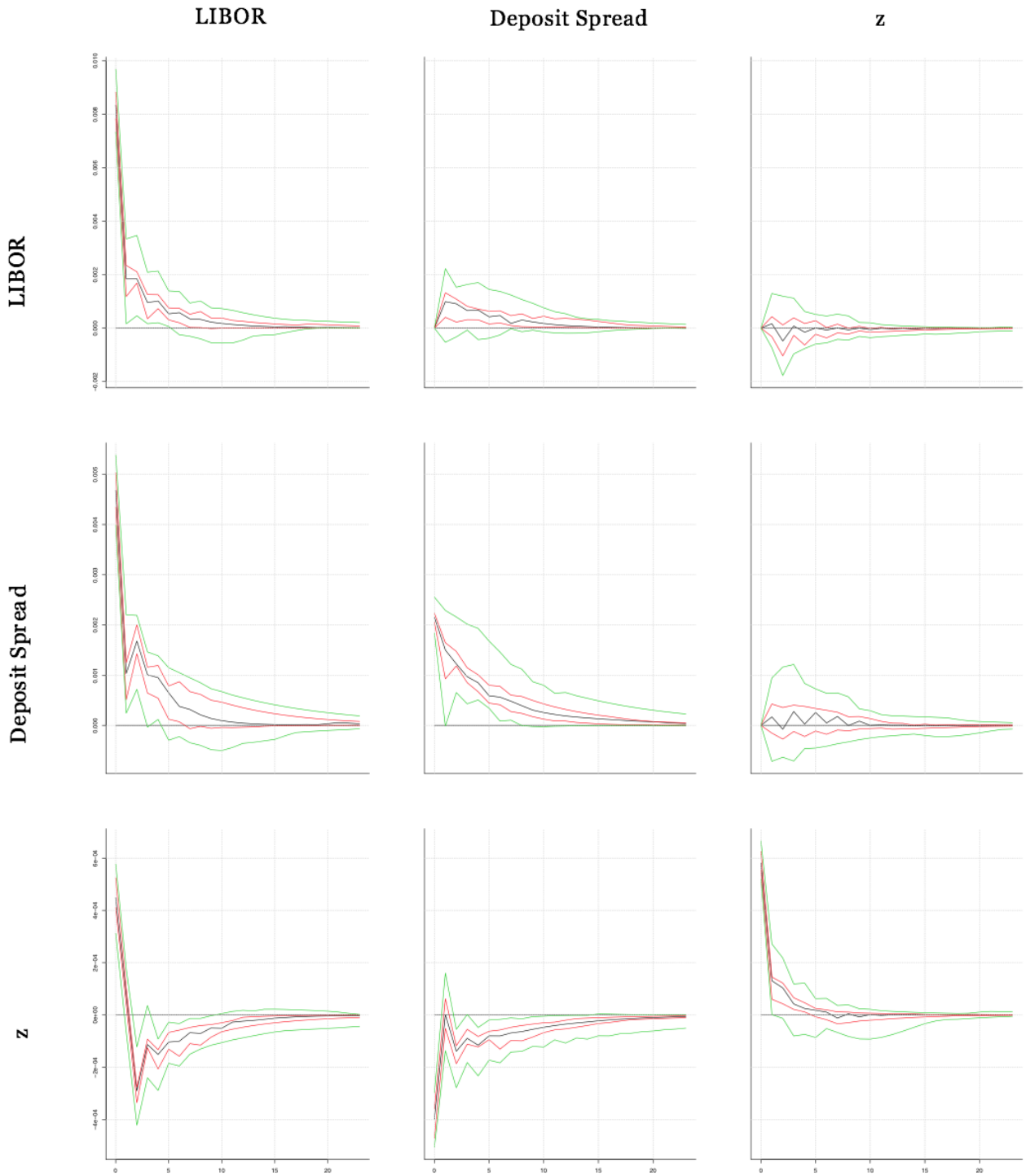


Figure 4. Accompanies Table 13. IRF graph of LIBOR, Deposit Spread, and z, quarterly



IX .J. VAR IRF Graphs of Bank Balance Sheet Variables

Figure 5. Accompanies table 16. IRF graph of Federal Funds Rate, Interest Expense Rate, and Interest Income Rate, quarterly

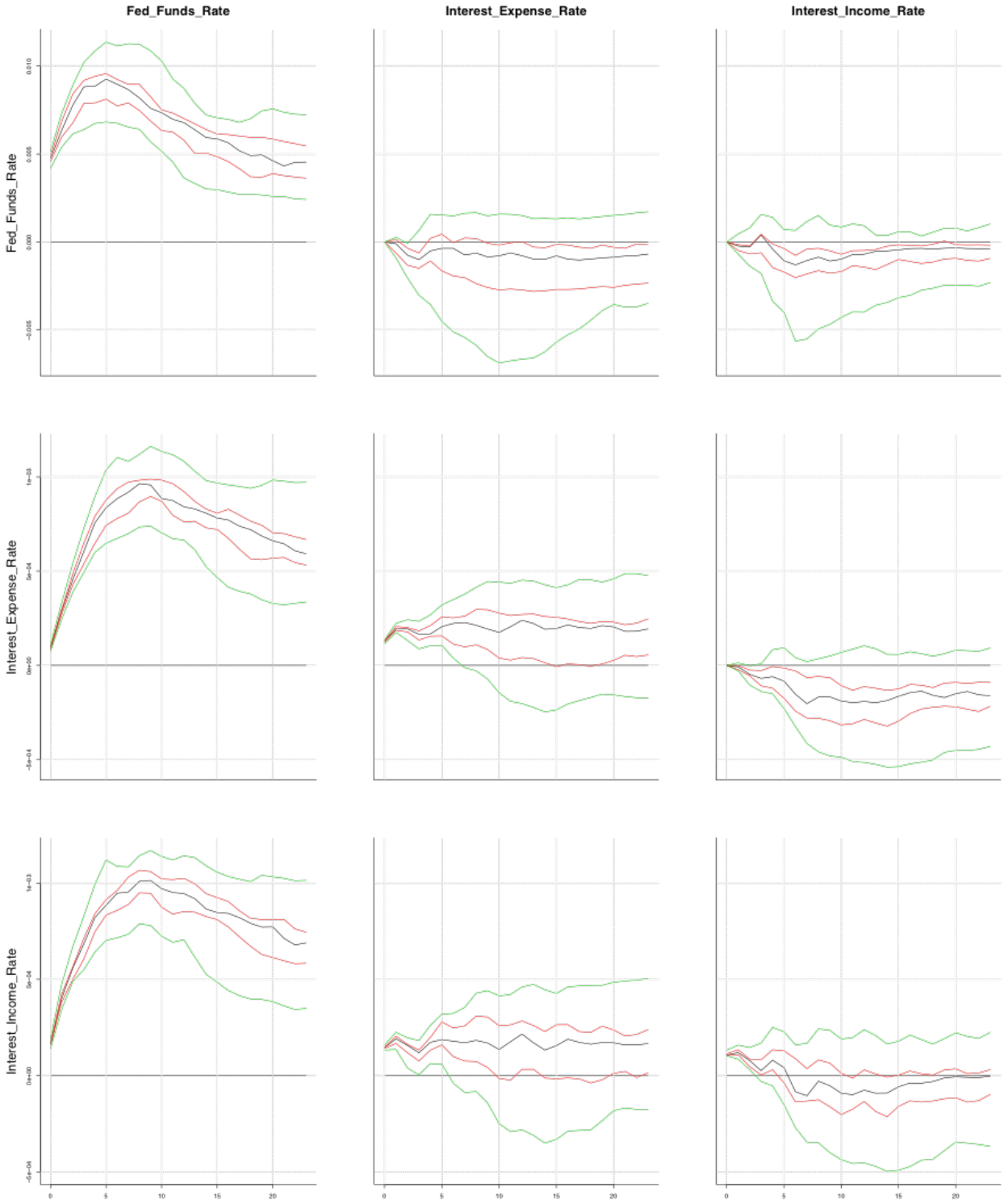


Figure 6. Accompanies Table 17. IRF graph of Federal Funds Rate, Interest Expense Rate, and ROA, quarterly

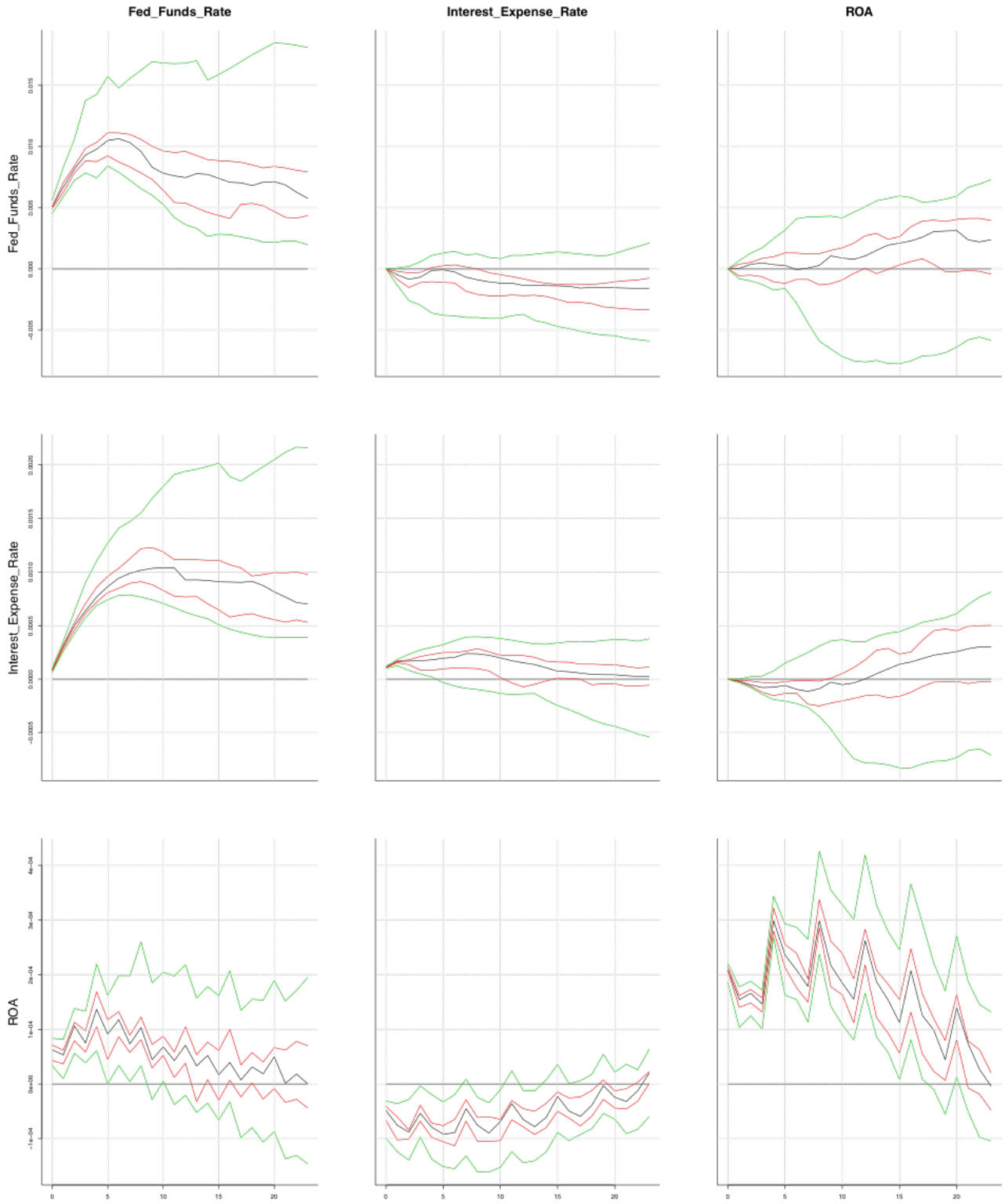
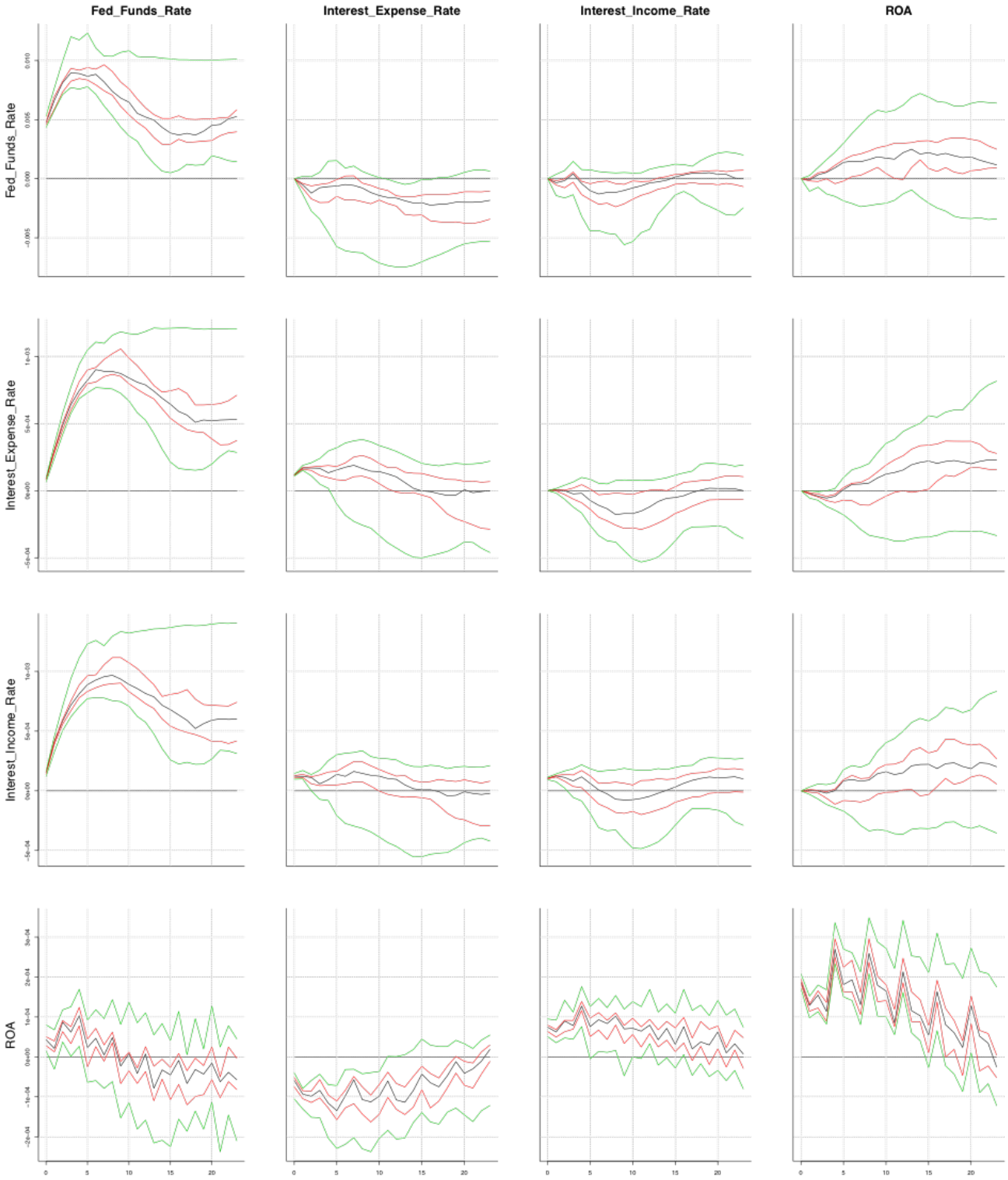


Figure 7. Accompanies Table 18. IRF graph of Federal Funds Rate, Interest Expense Rate, Interest Income Rate, and ROA, quarterly



The Economic Impact of Psychological Distress on Former Child Soldiers

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

I. Introduction

Forty percent of the worlds armed organizations employ child soldiers, counting a staggering 800,000 children among their ranks. While the majority of youth combatants are involved with groups not currently engaged in warfare, an estimated 300,000 children were active in over thirty conflicts across the globe (Kaplan 2005). Many international organizations, including UNICEF, USAID, and the Red Cross, aim to reduce these numbers through disarmament, demobilization, and reintegration (DDR) programs. DDR programs attempt to reintegrate former combatants by minimizing the impact of soldiering on their physical and mental health, as well as fostering economic and social inclusion (United Nations Disarmament, Demobilization, and Reintegration 2010). With millions of former child soldiers across the globe, this work is particularly important due to the long-term political, economic, and security consequences of failing to successfully reintegrate these young ex-combatants into society — which could lead to continued conflict, economic depression, and reductions in productivity for entire generations (Blattman & Annan 2010).

While aid organizations have been effective at treating psychological and physical trauma among former child soldiers, they have been comparatively ineffective at improving economic opportunity and social capital among recipients of reintegration assistance (T. Betancourt 2005; Humphreys & Weinstein 2007; Kaufmann 2016). This may occur because, while much is understood about the physical and psychological consequences of child soldiering, there is a dearth of literature — and an even greater paucity of data — exploring the economic impact of war on children. Scholars have examined the effects of post-traumatic stress disorder (PTSD) on post-conflict employment and earnings for adult veterans in the United States, which has supplemented knowledge about the economic impact of war on participants and may be instructive in this case (Smith, Schnurr, & Rosenheck 2005; Vogt et al. 2017). This article aims to bridge the gap between the psychological and economic literature on war-affected youth by examining the effect conflict-related psychological distress has on the income and employment status of former child soldiers.

The data for this analysis comes from the Survey for War-Affected Youth (SWAY) Phase One, a survey including 462 male former child soldiers in Northern Uganda, which was conducted in 2006 (Blattman & Annan 2010). SWAY is a particularly compelling data source due to the tragic quasi-natural experiment created by the indiscriminate abduction practices of Joseph Konys Lords Resistance Army. The major limitation of SWAY in its application to this question is that roughly 35% of respondents earned no income, introducing sample-selection bias into the models. Additionally, a wealth of literature suggests two-way causality between the explanatory and dependent variables. To control for these sources of bias, four models were constructed — an ordinary least squares regression, a two-stage least squares regression, a Heckman selection model, and an instrumental variable Heckman selection model — which indicate a negative relationship between psychological distress and income at the 90%, 95%, 99%, and 95% confidence level, respectively.

While psychological distress did have a significant negative effect on income for employed former child soldiers in the models, it did not affect the likelihood of employment in the first stage of either Heckman model. This finding adds to the literature on the impact of war on conflict participants, particularly in developing regions, and may be instructive to aid organizations seeking to successfully reintegrate children into society. This paper demonstrates how certain services — such as mental health counseling — can improve the welfare of the child in multiple domains, highlighting the importance of treating the symptoms of psychological trauma.

II. Literature Review

A. Children in Conflict

Tragically, hundreds of thousands of children are affiliated with over 85 different armed groups — including

governments — in dozens of countries across the globe (Theresa Stichick Betancourt et al. 2010). The term child soldier does not exclusively refer to those directly engaged in combat, but also includes porters, cooks, spies, and children in sexual slavery, according to the Paris Principles (UNICEF 2007). In northern Uganda, an astounding 46% of all male youth may have been abducted (Annan, Blattman, Mazurana, & Carlson 2011), though exact estimates are difficult to derive. Disturbingly, many organizations have recruited youth voluntarily. The Islamic State of Iraq and the Levant (ISIL) frequently recruited children from religious schools, orphanages, and willing parents. Dubbed the Cubs of the Caliphate, ISIL uses these children to commit gruesome terror attacks, including suicide bombings (Anderson 2016). The majority of child soldiers are under fifteen, with an average age of twelve. The youngest known child soldier was documented in Uganda at just five years of age (Singer 2006).

Though there are millions of former child soldiers estimated to be in the world today, little is known about the consequences of child soldiering. One landmark study, conducted by Blattman and Annan (2010), demonstrates that the greatest consequence of child soldiering is a reduction in human capital due to time removed from the labor market and educational system. Other studies have documented social stigma, including rejection by families and communities, who, in some contexts, believe socializing with the former child soldier may contaminate other children (Tonheim 2014). And while several studies have found former child soldiers experienced discrimination or stigma upon returning to their communities — including 73% of former child soldiers in Sierra Leone — there is virtually no stigma against former child soldiers in Uganda due to the disturbingly common abductions that took place in the northern region (Annan, Brier, & Aryemo 2009; T. S. Betancourt, Agnew-Blais, Gilman, Williams, & Ellis 2010).

Female former child soldiers, representing an estimated 24% of the total population (Spitzer & Twikirize 2013), face particular challenges to reintegration. Many are considered no longer qualified to be wives, rendering them destitute. Those who become pregnant during their experience face extreme discrimination, directed at both them and their children (Tonheim 2014). Female child soldiers also suffer up to twice as much psychological distress as their male counterparts as a result of their ordeal. While an astounding 26% of all female youth were estimated to have been abducted in northern Uganda, this paper unfortunately cannot examine the effects of psychological distress on economic opportunity among female child soldiers. Studies in northern Uganda show no impact of child soldiering on female employment or earnings due to an utter lack of economic opportunity for women in the labor market (Annan et al. 2011).

Disarmament, Demobilization, and Reintegration (DDR) programs aim to both reduce the number of children in armed conflict as well as facilitate their reintegration into society. Studies demonstrate that DDR is effective at improving mental and physical health outcomes for former child soldiers (T. Betancourt 2005). However, some scholars have called into question the use of psychotherapy, noting that it may be culturally inappropriate (Machel 2001). Rather, they suggest promoting community participation and acceptance, which has been shown to decrease psychological distress.

Humphreys and Weinstein (2007) found that DDR did not improve economic outcomes for adult ex-combatants. Kaufmann (2016) extended this finding to children and noted that societal interventions, such as the education of women and the promotion of traditional cleansing ceremonies, were more effective than individually-focused aid. Other studies similarly suggest the effectiveness of societal interventions, including widespread community desensitization efforts that increased social acceptance in Sierra Leone (Shepler 2014). More recent interventions, including microfinance loans and vocational training, have shown promise at increasing economic opportunity for former child soldiers. However, some failures should temper optimism, as widespread vocational training for hundreds of child soldiers in a single profession can result in an oversaturated market, depressing wages for all.

These failures highlight the importance of research to understand both the consequences of child soldiering and the effectiveness of specific interventions. This paper bridges the gap between economic and psychosocial literature on the effects of conflict on youth participants. Armed with a more complete appreciation of the economic externalities of the psychosocial consequences of war, policy makers will be better able to construct well-informed interventions that improve reintegration outcomes.

B. Income, Employment, and Mental Health

While no studies have examined the relationship between socioeconomic status and mental health in high-conflict regions, a wealth of literature indicates the increased prevalence of psychiatric disorders and psychological distress among impoverished individuals in the United States compared to their wealthier peers. One scholar even asserts that the negative relationship between socioeconomic position and psychological distress is one of the most firmly established associations in epidemiology (R.C. Kessler 1982), highlighting the potential for endogeneity in this study. Langner and Michael (1963) were the first to demonstrate this relationship. Over the last fifty years, numerous studies have supported this finding, which has been replicated using many alternate metrics for income and psychological distress.

McMillan, Enns, Asmundson, and Sareen (2010) explored the relationship between income quartiles and psychological distress, suicidal ideation, mood and anxiety disorders, and substance abuse using data from the early 2000s. Those in the lower quartiles of income experienced a significant increase in suicidal ideation, suicide attempts, anxiety disorders, substance abuse, and psychological distress, based on the Kessler Psychological Distress Scale K10 (R. C. Kessler et al. 2002). Gresenz, Sturm, and Tang (2001), by contrast, utilized the Mental Health Inventory-5 and the Composite International Diagnostic Interview from the 1990s Healthcare

for Communities survey, which measured psychological distress and probability of psychiatric conditions in the subjects. The study showed a clear and consistent relationship between family income quintile and distress and anxiety or depressive disorders. R.C. Kessler (1982) performed sixteen replications using eight different measurements of psychological distress and came to the same conclusion, additionally finding that income was the strongest determinant of psychological distress among employed males. Jarvis (1971) even found evidence for this relationship dating back to 1855; Massachusetts records indicated that the pauper class was 64 times more likely to be insane than the wealthy.

The literature shows that the negative association between income and psychological distress is consistent across a multitude of time periods and operationalizations, providing robust support for the theory. One explanation for this relationship is that the stress of being impoverished and the associated disadvantages — including crime victimization, illness, and death of children — increases psychological distress. Poverty can also render fulfilling social roles difficult, limit social capital, and otherwise erode social support networks, thereby decreasing psychological resilience (Belle 1990).

The presence of this relationship has substantial implications for this paper. The literature, however, is limited in its application to this context. No study could be found which replicated these findings among developing or conflict-laden regions. Additionally, the literature focuses on common mental health conditions or general psychological distress rather than the extreme forms of psychological trauma present among former child soldiers, which are more likely due to abduction-related experiences than poverty. Several studies have suggested an alternative theory for the relationship between income and distress, focusing on selection: an argument that individuals who are less emotionally or psychologically resilient are more likely to drift towards lower socioeconomic status and less likely to rise out of poverty (R.C. Kessler 1982). This claim provides support for the direction of the association posited in this paper. All the literature found utilized cross-sectional datasets; none used time-series or instrumental variable approaches which would refute this theory.

The negative association between income and distress may also be inapplicable due to the close to uniformly low income levels of northern Uganda, where the data for this study was collected. The abject poverty of the region may nullify the differential impact of socioeconomic status, as virtually no individuals can be labeled high-income — which constituted the control group in the aforementioned studies. However, Gresenz et al. (2001) found no difference in the impact of income on distress between states in America, regardless of average income or income inequality. Additionally, Barnett, Marshall, Raudenbush, and Brennan (1993) found substantially similar effects of income on distress on male and female partners, dispelling the theory that the discourse — which often focuses exclusively on women — does not apply to men. While the above arguments highlight the limitations of previous studies, they do little to refute the robust association demonstrated by the literature. It is therefore reasonable to assume the relationship explored in this paper is inherently endogenous, highlighting the importance of controlling for two-way causality between the explanatory and dependent variables using an instrumental variable approach.

C. Veterans and PTSD

While this paper is the first statistical analysis of the impact of psychological distress on income and employment status for former child soldiers, other studies have examined the same relationship in the United States for veterans of the Vietnam and Iraq wars. Although the literature is somewhat conflicted and limited in its ability to generalize to the context studied in this article, it is useful to examine as a related case.

Though scholars have long explored the effects of a variety of mood disorders on occupational function and income, Zatzick et al. (1997) was the first to examine this association for PTSD. The authors utilized the Mississippi Scale for Combat-Related Post-Traumatic Stress Disorder (Keane, Caddell, & Taylor 1988) to diagnose subjects with PTSD in a nationally representative sample of male Vietnam veterans. The results indicated that psychological distress significantly reduces the probability of employment and increase functional impairment, though the study did not explore effects on income. Savoca and Rosenheck (2000) confirmed and extended this conclusion by demonstrating a 50% reduction in the probability of employment with a lifetime diagnosis of PTSD, as well as a decrease in hourly wages based on a diagnosis of PTSD and major depression — which are often comorbid — of 16% and 45%, respectively.

Smith et al. (2005) identified a significant correlation between distress and the probability of employment, but no relationship with earnings: a finding consistent across three different metrics of distress. However, their sample size was small, consisting of only sixty full-time workers. Nonetheless, McCarren et al. (1995) found that in a study of monozygotic twins involved in the Vietnam war, PTSD affected only employment probability and not education, income, or occupational status. Vogt et al. (2017) further complicates the discourse by finding that while PTSD negatively impacts the probability of working for female veterans, it had no effect on males, which conflicts with prior research. This study additionally indicated that PTSD negatively impacts occupational functioning among both male and female veterans, a result which may imply lower productivity levels.

This theory is supported by Schult (2011), who found that PTSD had no impact on employment probability but was significantly negatively associated with reported occupational functioning among a sample of National Guard and Reserve veterans who were deployed to Iraq, nearly all of which were male. Similarly, Adler et al. (2011) demonstrated that PTSD and psychiatric conditions significantly increased work-related impairments and decreased productivity. These findings support the theory that PTSD negatively affects

income through the mediating effect of a reduction in productivity.

While this literature does bolster the posited relationship between psychological distress and economic outcomes, it may not be applicable to the context of this study. United States veterans and Ugandan former child soldiers have obvious differences, both in their conflict experiences and in the labor markets they seek to enter. None of the literature used an instrumental variables approach to the problem, failing to account for the potential for endogeneity supported by previous research. Additionally, Smith et al. (2005) indicated that one explanation for the effects of distress on employment probability was that American veterans who experienced increased symptoms of PTSD — and therefore a higher disability rating — could receive progressively more financial assistance from the Department of Veterans Affairs, and that their disability rating may be reduced if they choose to work full- or part-time. The authors found that higher disability ratings significantly reduced the probability of employment at the 99.9% confidence level, illustrating the strength of this relationship. As Ugandan former child soldiers receive no such assistance, it is possible that psychological distress will have no impact on employment status.

III. Data

The data used for this research is from the Survey for War-Affected Youth Phase One (SWAY)[1], a simple random sample of 741 males between 2005 and 2006. The survey was conducted primarily in two districts in Northern Uganda, Kitgum and Pader, which were heavily affected by the conflict between Joseph Konys Lords Resistance Army (LRA) and the Ugandan government that devastated the region for decades. Among the respondents were 462 former child soldiers, who represent the sample for this study.

SWAY is a cross-sectional dataset constructed through the random selection and tracing of numerous abducted and non-abducted children, and is compelling due to extensive interviews with the subjects, as well as the nature of the conflict with the LRA. The gruesome tactics of the LRA rendered recruitment impossible, so the group abducted approximately 60,000 children during the war. Due to the lack of volunteers and the LRAs arbitrary abduction practices, child soldiers in Northern Uganda represent a tragic quasi-natural experiment, in which there are no significant differences between abductees and their peers other than year of birth (Blattman & Annan, 2010).

Of the 462 former child soldiers surveyed in this study, 169 — or 36.6% — earned no income. It is highly unlikely that the decision to work is random, and so the models are probably biased by individuals non-randomly selecting out of the sample population. New models must thus be constructed to control for this expected bias.

[1]Survey conducted by Chris Blattman and Jeannie Annan (Blattman & Annan, 2010)

A. Variables

In this study, psychological distress was operationalized as an additive index of 15 indicators of traumatic stress, scaled for intensity. Employment status was indicated by a lack of earnings, and income was defined as earnings per month in Ugandan Shillings. While hourly wages would have been preferable, most respondents do not work on an hourly wage system, and are paid through selling products they produce or by the day, A logarithmic transformation of income was performed to make interpretation more meaningful.

This article proposes two instrumental variables to control for the endogeneity likely present in the models, as described by the literature review. The first instrument is violence witnessed during the abduction, defined as an additive index of six violent acts the respondents may have witnessed while involved with the LRA. The second instrument was a binary variable indicating if, during abduction, the child soldier ever felt their experiences were a result of a lack of courage or strength on their part. In effect, this variable measures self-blame and feelings of helplessness during the abduction. While these variables are clearly linked to psychological distress, as they are a root cause of trauma, they are unlinked to current employment status or income. Because the only reasonable avenue through which these variables would affect income or employment status is through the development of psychological distress, these instruments were selected for use in this study.

IV. Empirical Models

To analyze this data, four models were constructed. [2] first model, an ordinary least squares regression, was constructed using only the uncensored, or nonzero, observations of a logarithm of earnings per month in Ugandan Shillings, which was used as the dependent variable. The independent variables in this model include psychological distress, years of education, age, and a binary variable indicating if the individual is employed by the military.

$$\begin{aligned} log(EPM) =& \beta_0 + \beta_1 DISTRESS + \beta_2 EDUC \\ &+ \beta_3 AGE + \beta_4 MILITARY \end{aligned} \tag{1}$$

Of the independent variables, only distress is expected to be negatively associated with income; all others are expected to be positively correlated. However, the coefficients in the model are likely to be biased due to the reasons enumerated in the literature review and data sections of this paper. For that reason, additional models were formed to minimize the influence of these biases.

The second model constructed was a two-stage least squares regression (2SLS). In the first stage of this model, violence witnessed and feelings of helplessness during abduction, along with education, age, and military-based employment, were used to predict psychological distress. These predictions were then substituted for the observed values

[2]All models in this paper additionally utilize Huber-White standard errors or gmm methods to correct for heteroskedasticity

of distress in the 2SLS second stage, which is identical from the first model apart from this substitution, to compute the impact of psychological distress on income without the potential for two-way causality.

$$\begin{aligned} DISTRESS =& \gamma_0 + \gamma_1 WITNESS + \gamma_2 EDUC \\ &+ \gamma_3 AGE + \gamma_4 MILITARY + \\ &+ \gamma_5 SELFBLAME \end{aligned} \tag{2.1}$$

$$\begin{aligned} log(EPM) =& \beta_0 + \beta_1 DISTRESS + \beta_2 EDUC \\ &+ \beta_3 AGE + \beta_4 MILITARY \end{aligned} \tag{2.2}$$

However, the nonrandom selection out of the labor market — and therefore out of the sample — presents additional concerns, since these censored observations are not represented in the previous models. Heckman (1976, 1979) described a two-step model designed to counteract this bias. In the first stage, a probit model is used to predict the probability of employment. Explanatory variables in this stage included psychological distress, education, education^2, age, age^2, and marital status. The predictions of employment status are used to form the inverse Mills ratio (IMR)[3], which is then added to the second stage, an OLS regression, as an explanatory variable. The second stage is identical to the first model, except it includes the IMR as an additional explanatory variable, where the coefficient of the IMR will demonstrate sample-selection bias is present if it is significantly different from zero. The use of the two-step Heckman procedure should correct for the sample-selection bias expected to be present in the previous models.

$$Prob(EMP = 1|Z) = \Phi(Z\gamma) \tag{3.1}$$

$$\begin{aligned} log(EPM) =& \beta_0 + \beta_1 DISTRESS + \beta_2 EDUC \\ &+ \beta_3 AGE + \beta_4 MILITARY \\ &+ \beta_5 IMR \end{aligned} \tag{3.2}$$

However, because both reverse causality and sample-selection are likely to be present in the sample, these models may still be inconsistent, as they do not account for both sources of bias simultaneously. For this reason, an instrumental variable Heckman selection model was constructed, a procedure described by Wooldridge (2010). In the first stage, a probit model is formed, where the instrumented variable (distress) is replaced with the instrumental variables (violence witnessed and feelings of helplessness during abduction). The IMR is calculated from this model. The second stage mirrors the first stage of 2SLS but includes the IMR as an explanatory variable. In the third stage, observed psychological distress is replaced with the predicted values and the IMR is included as an additional explanatory variable, and an endogeneity chi-square test — which is numerically equivalent to the Wu-Hausman test but robust under heteroskedasticity (Hayashi, 2000) — can be used to confirm the presence of endogeneity. However, because of the high multicollinearity of the instrumental variables and the absence of the explanatory variable in the probit model, the first stage is uninterpretable. The interpreted first stage is therefore constructed as an instrumental variable probit model, which mimics 2SLS by predicting values for distress using the instruments, and then replacing the observed values with these predicted values in the probit model, which can be evaluated for effect. The use of the instrumental variable Heckman procedure should remove both endogeneity and sample-selection bias from the results.

[3]The IMR was calculated using STATA: `https://www.stata.com/support/faqs/statistics/mills-ratio/`

V. Analysis

A. Comparative Statistics

In order to understand the variables and their division based on employment status, Table 1 sets out descriptive statistics and descriptions of all variables used in this study. According to the World Bank (World Development Indicators: PPP Conversion Factor, GDP, 2006), USh 512.18 was equivalent to $1 (USD) at the time of this survey. The average respondent earned USh 17,200.54 per month, which can purchase the equivalent of $33.58, placing the population well below the poverty line. Employed respondents earned an average of USh 27,121.67, equivalent to $52.95. Only 58 respondents earned more than the World Bank international poverty line of $1.90 per day, representing a mere 19.8% of those employed and 12.6% of total respondents, illustrating the deep poverty of the region.

Similarly, there is a dearth of education among the population. Nearly half (48.7%) of respondents did not complete primary school, the end of traditional schooling in Uganda. Just 30.7% of the sample attended secondary school, and 80.9% of those individuals dropped out before completing all four years, staying an average of just over two. Together, these metrics serve to demonstrate the impact of conflict on traditional economic measures of individual wellbeing, and highlight the importance of research and well-informed interventions in these regions.

B. Results

The results of the analysis can be found in Table 2, where the probit and instrumental variable probit results of the Heckman models are reported under selection. In the first model, an ordinary least squares regression, there is a negative relationship between distress and income (p = 0.053). This model, however, is likely to be biased due to endogeneity and sample-selection, necessitating further analysis.

In the second model, a two-stage least squares regression, there is a significant negative relationship between psychological distress and income (p = 0.033). A Kleibergen-Paap under-identification test and the Hansen J-statistic reveal that the instruments are not underidentified and do not violate overidentification restrictions. The Cragg-Donald F-statistic for the first stage was 13.565, above the threshold for relevance suggested by Staiger and Stock (1994). These

tests indicate the instruments were both strong and relevant. However, the endogeneity chi-square test reports a p-value of 0.105, which does not permit a conclusion that endogeneity is present.

In the third model, a Heckman selection model, no relationship was observed between psychological distress and employment status in the first stage. While education and education2 were not individually significant in the model, an F-test indicates they are jointly significant. In the second stage of the model, psychological distress significantly diminishes income (p = 0.009), but this result is potentially confounded by two-way causality between explanatory and dependent variable. Additionally, the inverse Mills ratio (IMR) was significant (p = 0.018), permitting a conclusion that sample-selection bias is present, and that the Heckman model more accurately describes the trends in the data.

In the fourth model, an instrumental variable Heckman selection model, very similar results were produced between the instrumental variable probit and the first stage of the third model. Once again, psychological distress did not significantly affect employment status, and education and education2 were jointly significant. In the final stage, there is a significant negative relationship between psychological distress and income (p = 0.021). A significant coefficient of the IMR (p = 0.019) indicates sample-selection bias. While age was no longer significant in this model (p = 0.159), this may be due to the dominance of low-skill and unskilled labor in the population, whose income would not significantly grow with experience. Like the 2SLS model, a Kleibergen-Paap underidentification test and the Hansen J-statistic reveal that the instruments are not underidentified and the overidentification restriction is not violated, and an F-statistic of 11.932 is still above the threshold for relevance. In this model, however, the endogeneity chi-square test reveals a p-value of 0.0789, which indicates endogeneity is present at a 90% confidence level.

C. Discussion

The first stages of the Heckman models show that distress has no significant effect on employment status, which conflicts with the findings of McCarren et al. (1995), Zatzick et al. (1997), Savoca and Rosenheck (2000), and Smith et al. (2005), but is supported by more recent research, including Schult (2011) and Vogt et al. (2017). These studies, however, focused on veterans in the United States, who have substantially different conflict and return experiences than the population explored in this study. Veterans suffering from PTSD in the United States may choose not to work and instead receive support from the Department of Veterans Affairs, other government programs, and their families or spouses. The finding of Smith et al. (2005) and Rosenheck et al. (1995) that increased disability rating — and the monetary compensation that accompanied it — reduced the likelihood of employment may explain the difference between the results in this article and prior research.

Additionally, these studies postulate that a reduction in employment may be partially caused by stigma against psychiatric disorders, causing employers to reject distressed applicants or fire employees who exhibit symptoms of distress. However, because of the high frequency of abductions and displacement in Uganda, it is likely that the population is understanding and inclusive of those involved in the conflict. Supporting this view, Annan (2009) found that only 7% of returnees reported any social exclusion in Northern Uganda, because the community — whose family members had often also been abducted — recognized that these children were forced to commit violence, and therefore did not stigmatize them for it. However, this is not the case in other contexts, such as Sierra Leone, where Betancourt et al. (2010) found that 73% of former child soldiers had experienced some level of discrimination. It is possible that in these contexts, where the community is far less understanding, the results would be different. These findings cannot be generalized to such a case, and more research is required to determine the relationship between distress and employment in regions where stigma associated with conflict participation is more prevalent.

The significance of the inverse Mills ratios in both Heckman models strongly indicates the presence of sample-selection bias in the data, indicating that the Heckman models more accurately reflect the true nature of the relationship between psychological distress and income. However, this is where clarity on model selection ends. The endogeneity chi-square test for the fourth model did not produce a conclusive result, offering a subjective model choice. The substantial body of literature demonstrating the presence of endogeneity bias would reasonably lead to a conclusion that endogeneity is present, even if the statistical support is only weakly significant. It is unclear, however, if the literature applies in this context, and given the ambiguous result of the endogeneity test, one cannot conclude with a high degree of confidence that the fourth model most accurately describes the population.

For this reason, it is difficult to determine the magnitude of effect that distress has on income. Since this paper is the first study to examine this relationship in this or any related context, no instruction can be drawn from the literature on the magnitude of effect that should be observed. However, even the lower bound presented by this articles third model — a seven percent decrease in earnings per symptom of psychological trauma — is substantial. Under this assumption, an individual one standard deviation above the mean, with 2.6 more symptoms of distress, would experience more than an 18% reduction in income when compared to the average case. This decrease rises in severity using the upper bound found in the fourth model — a 24% reduction in income per symptom. Regardless of which model is more reflective of the population, these effects clearly demonstrate a sizeable impact of distress on earnings. Both models also identify a significant negative relationship between the explanatory and dependent variable, permitting a conclusion this relationship exists without requiring a definitive selection between models. The finding is further supported by the results of the OLS and 2SLS models, which show a similar

direction and magnitude of effect.

This relationship is likely explained by the findings of Vogt et al. (2017), which suggested that psychological distress decreases occupational functioning. Similarly, Schult (2011) and Adler et al. (2011), provide support for the theory that this decrease in income stems largely from losses in productivity, rather than hours worked. While it was impossible to evaluate differences in hours worked because of the Ugandan model of employment, there was no significant difference in days worked per month in those above the median in distress and those below (8.4 vs 8.3 days worked, respectively), which persisted after removing those who did not work (11.9 vs 12.4 days worked, respectively). The apparent lack of difference in time spent working bolsters the theory proposed in the literature, and suggests that the observed negative relationship between psychological distress and income is a result of losses in productivity on the job, rather than fewer hours performing the job.

VI. Conclusion

This paper further explores the consequences of child soldiering and extends research that examines the relationship between psychological distress and employment outcomes among veterans to new populations. While the finding that distress did not significantly affect the probability of employment conflicts with the literature, this may largely be explained by contextual differences stemming from VA benefits dis-incentivizing for employment among high-disability American veterans. The observed negative relationship between psychological distress and income, which is likely due to losses in productivity, further demonstrates the importance of psychosocial post-conflict interventions in these contexts and describes the positive externalities these interventions can have in other domains. This paper also shows that the posited relationship is robust for sample-selection and endogeneity biases, which no study has demonstrated to date.

While SWAY has been generalized to the entire population of child soldiers before (Blattman & Annan, 2010), largely due to its robustness as a dataset and the quasi-natural experiment presented by the conflict in Northern Uganda, such a claim would be inappropriate in this case. The same arbitrary abduction practices which make Uganda a compelling case study also serve a mediating role in the communities relationships towards former child soldiers — attenuating the stigma associated with forcible participation in the conflict that is present in many analogous regions. This fact limits the generalizability of this papers findings. Further research should aim to replicate this study in populations where higher stigma associated with child soldiering is present to determine the differential effects of this stigma on the proposed relationships between psychological distress, employment, and income. These findings may also be applicable to the consequences of psychological distress among former youth gang members or abused children in the Western world — a relationship which should be examined in the future.

This study has substantial implications for aid organizations seeking to successfully reintegrate millions of former child soldiers into society. While DDR programs have largely been ineffective at improving the economic outcomes of former combatants in the past, this paper identifies new avenues for intervention. Based on this analysis and the supporting literature, even small reductions in warfare-related psychological traumas can have a substantial positive impact on the wages of a former child soldier, highlighting the importance of treating the mental — and not just the physical — scars of war.

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APPENDIX

Table 1

Variable	Description	Range	All Observations	Employment Status: Employed	Employment Status: Unemployed
EPM	Earnings Per Month in	Continuous	17200	27121	—
	Uganda Shillings		(2166)	(3280)	—
EMP	1/0 Employment Status	0 – 1	0.634	—	—
			(0.022)	—	—
DISTRESS	0-15 Additive Index of	0 – 15	4.352	4.506	4.085
	Traumatic Stress		(0.121)	(0.148)	(0.206)
WITNESS	0-6 Additive Index of	0 – 6	4.026	4.018	4.031
	Violence Witnessed		(0.067)	(0.107)	(0.086)
HELPLESS	1/0 Feelings of	0 – 1	0.818	0.857	0.749
	Helplessness, Self-Blame		(0.018)	(0.021)	(0.034)
EDUC	Years of Education	Continuous	6.820	6.594	7.213
			(0.130)	(0.159)	(0.221)
AGE	Years Since Birth	Continuous	21.366	22.58	19.26
			(0.233)	(0.280)	(0.361)
MIL	1/0 Employment in Military	0 – 1	0.017	0.024	—
			(0.006)	(0.009)	—
MARRIED	1/0 Marital Status	0 – 1	0.437	0.573	0.201
			(0.023)	(0.029)	(0.031)
Observations			462	293	169

Standard Errors in Parentheses

Table 2

VARIABLES	(1) OLS Log(Income)	(2) 2SLS Log(Income)	(3) Heckman Log(Income)	(4) IV Heckman Log(Income)
Distress	-0.0513*	-0.201**	-0.0702***	-0.240**
	(0.0264)	(0.0945)	(0.0265)	(0.104)
Education	0.136***	0.132***	0.155***	0.155***
	(0.0272)	(0.0281)	(0.0277)	(0.0293)
Age	0.0832***	0.0757***	0.0440**	0.0346
	(0.0145)	(0.0161)	(0.0220)	(0.0245)
In Military	1.339***	1.504***	1.300***	1.491***
	(0.444)	(0.482)	(0.461)	(0.531)
Mills Ratio			-0.930**	-0.978**
			(0.391)	(0.417)
Constant	6.710***	7.582***	8.032***	9.021***
	(0.431)	(0.707)	(0.714)	(1.043)
Observations	293	292	293	292
R-squared	0.189	0.108	0.203	0.081
SELECTION				
Distress			0.0270	0.0778
			(0.0260)	(0.133)
Education			0.0715	0.0648
			(0.0879)	(0.0889)
Education^2			-0.00748	-0.00705
			(0.00568)	(0.00594)
Age			0.404***	0.377**
			(0.134)	(0.171)
Age^2			-0.00829***	-0.00767**
			(0.00303)	(0.00385)
Married			0.687***	0.686***
			(0.191)	(0.194)
Constant			-4.739***	-4.666***
			(1.430)	(1.538)
Observations			462	459

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

Purchasing Policy: The Effect of Political Action Committee Campaign Contributions from the Agribusiness Sector on Support Mechanisms for Individual Crop and Product Producers

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***Abstract*— In this paper, I analyze data on agricultural producer support mechanisms and agribusiness Political Action Committee campaign contributions from 1998 to 2016 to determine the extent to which lobbying on behalf of any particular crop or agricultural product is translated into government transfers back to its producers. I proceed with a review of the existing literature about purchasing support mechanisms and the agriculture industry, and a discussion of the data, which is sourced from USDA, OECD, and The Center for Responsive Politics. Guided by Hausman test results, I run a fixed effects regression to demonstrate that campaign contributions do not have a statistically significant impact on transfers to producers in the agribusiness industry, despite findings in the literature.**

I. Introduction

It is common discourse within the field of economics to emphasize the deadweight loss to society as a result of lobbying, campaign contributions and the protectionist measures these actions seek to encourage. Considering a scenario without market failures such as externalities and asymmetric information, the general social welfare is mathematically higher in the absence of tariffs, quotas, and producer subsidies, which are often applied to uniform products that can be easily substituted by imports. In recent years, there has been much discussion about the impact of campaign contributions, and the need for campaign finance restrictions and policy reformations, due to the unfair outcomes they presumably create. There is an implicit assumption that the economic distortion of price and market supports is a result of campaign finance from the private sector, and that donations to political campaigns are directly reflected in the passage of donors preferred policies.

This topic is of particular importance in the agriculture industry, which maintains a historical legacy of support and a persistent anti-trade bias (Anderson, Rausser, and Swinnen, 2013). In 2016 alone the agribusiness sector spent over $26.4 million on campaign contributions to federal candidates in 2016, a slight rise from the $25.6 million spent in the 2014 congressional election cycle. (The Center for Responsive Politics, 2017) In fact, political action committee contributions to federal candidates have been increasing steadily since 1998, at a rate of $1-3 million each election cycle. These contributions are seemingly not without purpose; there are extensive agricultural supports in the United States. With a $13 billion estimate of total producer single commodity transfers from consumers and taxpayers to agricultural producers in 2016, it seems that the enormous campaign contribution expenditures are reflected in the policies chosen by the legislative officials whom they finance (OECD, 2017). However, the total support received by single commodity producers has actually declined from the $20.3 billion of transfers in 2014. In fact, this number has fluctuated considerably since 1998, ranging from $7.7 billion to $27.4 billion (USDA, 2017). This lack of consistent trend begs the question: do agribusiness campaign contributions directly translate into producer supports for donors?

It is worth noting that these transfers are not received uniformly by agricultural product producers and the agribusiness sector does not lobby as a single entity, indifferent with regard to which particular producers receive support mechanisms. From 1998 to 2016, approximately 275 political action committees made congressional campaign contributions in each 2-year period. Of the nearly 3000 PACs, more than half of them are product-specific, meaning that they contribute on behalf of a particular crop or set of crops (e.g. wheat, or eggs and poultry). As mentioned, these contributions are made with the assumption that they will "purchase policy"—industry support for a candidate begets reciprocal support via subsidies quid pro quo.

However, to what extent is the lobbying on behalf of any given crop reflected in the transfers to its producers? Are agribusiness contributions aggregated mentally and reflected in the presence of a general farm bill, which is approved every 5 years, with insignificant distinctions between the extent of producer support mechanisms with respect to individual crops? Or are campaign contributions a primary determinant of the quantity of transfers to specific crop producers? This paper will provide insight into the significance of both general and crop-specific campaign contributions in policy production. More specifically, this study will test the hypotheses that either crop-specific contributions, as a percentage of the production value of the particular crop, and/or total yearly contributions from the agribusiness sector determine producer support mechanisms, controlling for social welfare of the policy (the extent to which the policy hurts consumers of that product).

This paper uses data which range over 14 crops and 18 years, from 1998-2016. This research is particularly

relevant at present; there is a lack of research on agricultural campaign contributions impact on policy over the last twenty years, during which there has been a changing climate of agricultural protection. In 1995, the implementation of the WTOs Uruguay Round Agriculture Agreement began, and developed countries incorporated new restrictions into their policies over a six-year period (World Trade Organization, 2001). One particular facet of this goal was increasing agricultural market access and reducing market price distortions created by high tariffs, export subsidies, and domestic transfers to producers (World Trade Organization, 2001). Domestic support programs were classified into groups, including "amber-," "green-," and "blue-" box-policies. The first category includes domestic policies that have direct effects on production and trade and therefore must be cut back. The second category includes policies that have "minimal impact on trade" and can therefore be used without restriction. These include "government services such as research, disease control, infrastructure and food security" (World Trade Organization, 2001). Of particular interest is the inclusion of direct payments to farmers that "do not stimulate production, such as certain forms of direct income support" (World Trade Organization, 2001). In light of this deal, agricultural producer support is declining, and the implementation of support policies is shifting.

Therefore, the weight of campaign contributions in determining the allocation of withstanding funds for transfers to producers has emerged as a more salient issue for agribusiness PACs nationally, and in determining what incentives must be adjusted in order to make future market access negotiations successful. If global negotiations have only marginally decreased barriers and contributed significantly to furthering inequality among domestic producers, these are valuable observations in evaluating the efficacy of the policies. Further, billions of taxpayer dollars are used to bolster agricultural producers; the impact of lobbying, in comparison to social welfare metrics, in shaping their distribution is crucial in assessing the value of support mechanisms in helping United States citizens as a whole.

In this paper, I analyze data on agricultural producer support mechanisms and agribusiness Political Action Committee campaign contributions from 1998 to 2016 to determine the extent to which lobbying on behalf of any particular crop or agricultural product is translated into government transfers back to its producers. I proceed with a review of the existing literature about purchasing support mechanisms and the agriculture industry, and a discussion of the data, which is sourced from USDA, OECD, and The Center for Responsive Politics. Guided by Hausman test results, I run a fixed effects regression to demonstrate that campaign contributions do not have a statistically significant impact on transfers to producers in the agribusiness industry, despite findings in the literature.

II. Literature Review

The majority of the studies regarding campaign contributions and their abilities to "purchase" policy are based on the Grossman-Helpman model of "Protection for Sale" (Grossman and Helpman, 1992). In 1992, Grossman and Helpman modeled special interest groups impact on policy, using *n* lobbies and a single policymaker (the federal government). The concept captured by the G-H model is that the special interest groups within an industry are a collection of various profit maximizers who intend to maximize rents, represented by government transfers from protectionist policies net of campaign contributions that motivate the creation of such policies (Grossman and Helpman, 1992). Politicians who create this policy are maximizing their own welfare functions—weighted sums of PAC contributions and social welfare, which push in opposite trade-policy directions. In the Grossman Helpman model, PACs present a menu of auctions or a contribution schedule, with options for a series of policies and the contributions the PAC will make given those policies (Grossman and Helpman, 1992).

The Grossman-Helpman Model also emphasizes import elasticities and import penetration ratio[1] as measures of the deadweight loss generated by producer support in each industry. When the import elasticity is high, the deadweight loss from protection will be higher so the government will be more reluctant to impose protectionist policies. Similarly, when there is low import volume, consumers do not oppose import restrictions as strongly, and producers do not lobby for them as extensively (Grossman and Helpman, 1992).

In their 1997 paper "Protection for Sale: An Empirical Investigation," Pinelopi Koujianou Goldberg and Giovanni Maggi find empirical evidence for the Grossman Helpman model, using data on nontariff barrier coverage ratios in 1983 (Goldberg and Maggi, 1997).[2] They complicate the model with a finding that the governments welfare function puts a much larger weight on social impact of the policy than it does on contributions. They find that differences in protection can be entirely explained by the degree of political organization within an industry (there is a PAC representing the industrys interests), campaign contributions, import elasticity, and import penetration ratio (Goldberg and Maggi, 1997). However, Goldberg and Maggi do not incorporate a campaign contributions variable directly into their regression, and instead estimate a constant that represents the weight of campaign contributions, relative to social welfare, on policy. They also test factors like employment size, sectorial unemployment rate, measures of unionization, and buyer and seller concentration, and find that none of these variables have explanatory power (Goldberg and Maggi, 1997).

In a study similar to that of Goldberg and Maggi (1997), Kishore Gawande and Usree Bandyopadhyay (2000) analyze the same 1983 nontariff barrier data and confirm the finding that campaign contributions are highly correlated with protectionist measures. They add that a dummy variable

[1]The import penetration ratio is defined as the ratio between the import value to the value of total domestic demand for the product, showing the degree to which demand is satisfied by imports (OECD 2017).

[2]The nontariff barrier coverage ratio is a metric that represents the share of a countrys imports "this is subject to a particular non tariff barrier, or any one of a specified group of non tariff barriers," for example, subsidies, or delays due to customs processing (OECD 2001).

for "the industry is politically organized" and a variable for the "quantity or concentration of similar lobbies" have statistically significant correlations with support mechanisms. Gawande and Bandyopadhyay focus on the impact of downstream industries and their counter-lobbying, but again they do not incorporate a direct campaign contributions variable. Further, the assumption that downstream industries would lobby against support is theoretically consistent when support comes by way of tariffs or quotas, which raise the price paid for the input. However, policy instrumentation has largely changed since 2000 and support mechanisms are more heavily provided through subsidies and transfers to producers. These policies theoretically lower the prices observed and paid by downstream industries. With regard to agriculture specifically, it is possible that this shift in instrumentation would alter the interests of the processed food industry and other similar agricultural product users in favor of agricultural support mechanisms.

Rigoberto Lopez applies the Grossman-Helpman model to agribusiness in particular and complicates the conversation with his finding that not only would eliminating campaign contributions significantly decrease agricultural subsidies, but that investment returns to farm PAC contributors are about $2,000 in policy transfers for every dollar of contributions (reflective of a range of returns from $14-$16,590 per additional dollar). (Lopez 2001) This points to the idea that contributions may not be analyzed as a quid pro quo exchange but rather have the potential to be amplified dramatically as they are translated into policy regarding producer transfers. This also suggests that contributions may be more significant than the social welfare changes they initiate in the eyes of the legislators creating the policies, as contributions are translated on a one-to-many dollar basis.

In 2005, Gawande added to the discussion about campaign contributions and support mechanisms with his paper "The Structure of Lobbying and Protection in US Agriculture" by running a simple regression between different support mechanism and campaign contributions, the import to output ratio, and the export to output ratio (Gawande, 2005). Unlike Lopez, who sought to analyze the extent of the welfare change initiated by agribusiness campaign contributions, this paper directly applies a rough version of the Grossman-Helpman model to agriculture in order to determine the respective weights of contributions and social welfare in policy makers welfare functions. (Gawande, 2005) In particular, Gawande studies the relationship of campaign contributions with three metrics of support: quality assurance standards, specific tariffs, and countervailing duties. In contrast with the results of Goldberg and Maggi (1997), Gawande finds no association between protectionism and the import penetration or the export-to-output ratio. However, he does find a statistically significant correlation between the contributions and protectionist policy (Gawande, 2005). The methodology of Gawandes 2005 paper is discussed in Section IV to serve as a contrast for the functional form specifications selected in this paper.

I merge the findings and methodologies of the aforementioned studies in order to determine the extent to which crop-specific campaign contributions continue to translate into protectionist policies despite the overall trend away from price distortions in trade. I will include variables to account for the possibility that crop-specific contributions impact support mechanisms for crop producers other than the ones they were contributed on behalf of. I will also include variables for the quantity of PACs contributing on behalf of the particular crop and from the industry, and for a time lag in the impacts of contributions. My methodology combines aspects of the papers of Gawande (2005) (base specification), Goldberg and Maggi (1997), Gawande (2000) (inclusion of industry organization variables) and Lopez (2001) (methods of measuring contributions and support mechanisms) to eliminate omitted variable bias present in each of the studies, and continue evaluating the empirical evidence of the Grossman-Helpman model, specifically with respect to agriculture in the late 20th and early 21st centuries.

III. Data

To complete my analysis, I use data on producer support metrics, available through OECD and USDA databases (the same data sources used by all the peer reviewed papers discussed in Section II). These data include the crop-specific value of production and the PSE, which is a measure of all policy transfers to agricultural producers as a share of gross farm receipts. These data together allow me to compute the % PSE, which represents the nontariff barrier in a way that allows for uniform comparison among crops of different values.[3] This dataset has estimates for Wheat, Barley, Sorghum, Maize, Rice, Soybeans, Sugar, Milk, Beef, Poultry, Eggs, Pig meat, Sheep meat, Wool, and Cotton, and ranges from 1998 to 2016.[4]

On the other side of my analysis, I use data on campaign contributions from OpenSecrets (The Center for Responsive Politics), which is also the data source of the papers discussed above. The contributions are listed by sector; I exclusively used data for PACs categorized as "Agribusiness Sector PACs." The database then records the contributions by PAC, so I have looked up each individual PAC and searched their sites for keywords representing each of the agricultural products listed above.[5] Each contribution is thereby attributed to a single or set of products and assumed that it was donated with the goal of protectionist policies for that

[3]The reasoning behind the use of nontariff barrier data is discussed in the Methodology section.

[4]The dataset also includes observations for wool, however the PSE data included four extreme outliers (40-80 times larger than the mean), and thus it was removed from the set.

[5]Many PACs made crop specific contributions on behalf of fruits and nuts. Due to the lacking availability of protectionist estimates for these two products, these contributions were removed from the dataset, along with the fewer than 100 combined contributions on behalf of flowers, horses, and trees/forestry. PSE data included estimates for alfalfa, but total alfalfa output, imports and exports are not reported by the USDA, so this data was also removed from the set.

particular product.[6] For lobbies that pertain to a specific crop, the entirety of the contribution is attributed to that crop. For lobbies that pertain to a set of crops, contributions are divided among crops by percentage of total value (of the set of relevant crops or products) produced. For example, if a PAC represents wheat and barley producers, the majority of the campaign contributions are defined as wheat contributions, as wheat represents a far larger percentage of total US crop production value than barley does.[7] In this case, the "total" value would be the sum of the value of wheat and barley in that year, not the sum of the value of all products studied, and the percentage value will be as a percentage of the wheat-and-barley-sum. When PACs represent a general agricultural lobby that does not specify products of interest, depending on if they cover general egg/meat production, they are indicated as "all" or "all crops." Then, as mentioned above, the contribution is divided on a weighted basis depending on the percent of total US production value each particular agricultural product represents. This dataset also ranges from 1998-2016 and is reported on a biannual basis. I also deflate the contributions to reflect 1998 dollars in each year, so that the growth of contributions is not a result of inflation over time.

Data for other control variables, used to approximate social welfare, include estimates of the export/output and import/output ratios over time. I construct these ratios based on data for gross exports, imports, and output for each product (available through the USDAs Production, Supply and Distribution database). I use metrics for social welfare of the policy due to the strong possibility that legislators choose to support a policy by investigating the change in producer and consumer surplus that it initiates. Usually, import elasticities are used as proxies for the change in producer and consumer surplus because "deadweight loss from protection is higher in industries with high import elasticities." (Gawande, 2005, p.14). However, import elasticities are often estimated with huge variance and existing data on import elasticities are inconsistent (Kee, Nicita and Olarreaga, 2008). In order to circumvent this issue, Gawande (2005) uses the import- and export-to-output ratios (which are highly correlated with import elasticities and are more consistently reported) as the proxies for the changes in producer and consumer surplus. He further explains that "the lower the import volume, the lower the social cost imposed on individuals" by support mechanisms (Gawande, 2005, p.14). Therefore, consumer opposition to support of low import volume sectors is lower (Gawande, 2005). Further, he claims "the greater the share of output exported, the greater the amount of price support" desired by consumers because "export subsidies raise their prices above the world price" but the domestic consumers are not paying this price for the good being sold abroad (Gawande, 2005, p.16). As this paper builds on Gawande (2005), it also adopts this element of his methodology and reasoning.

As demonstrated in Table 1, total contributions per year for each crop have been growing over time, despite the general trend of falling Producer Single Commodity Transfers as a percentage of value of commodity specific receipts (PSCTP, effectively PSE) and Producer Nominal Protection Coefficient (PNPC) ("an indicator of the nominal rate of protection for producers measuring the ratio between the average price received by producers at farm gate, including payments per ton of current output and the border price" [OECD, 2002]). These trends can be observed in Figures 1 and 2, which demonstrate the change in the producer support mechanisms since 1998, juxtaposed with the change in total contributions from the agribusiness sector over time. The vertical lines represent years in which congress voted on and approved Farm Bills. In Figure 1, I observe a peak in 2000, corresponding with the full implementation of the Uruguay Agriculture Agreement. Since the PSCTP covers both amber and green box policies, the decline in support suggests that green box policies were not able to compensate for the removal of amber box policies and thus producer support in general declined. Figure 2 demonstrates a kink in contributions in 2008, a presidential election year in which campaign contributions became particularly controversial and the year of a Farm Bill. After 2008, contributions continued increasing though not as rapidly as they had been in the previous decade. Interestingly, I do not observe kinks in other presidential election years or Farm Bill years.

Though the export/output ratio seems to be generally rising and the import/output ratio generally falling, there does not appear to be a strong trend.

Table 2 demonstrates that the crops with particularly large average crop-specific contributions do not necessarily receive large support transfers (e.g. beef), and products with low contributions do not necessarily receive small amounts of support (e.g. sheep). Nonetheless, some products do follow the expected trend (e.g. milk, sugar). These observations may be a result of crop value, as beef represents about 20% of the total yearly US production value, whereas sheep represents less than .1% of total yearly US production value. Therefore, the gross contributions from an industry that is creating far more value will be larger, simply as a result of budget constraints. Further, the protectionist estimates account for production value and are representations of transfers per dollar of crop value. Thus, having a high protectionism measure for wheat requires far less expenditures from the government than a high protectionism measure for beef would. This suggests that perhaps regressions should be run between contributions as a percentage of value (shown in columns 3 and 4 of Table 2) and %PSE.

IV. Methodology

For this paper, I analyze the empirical evidence for the Grossman-Helpman model of "truthful contributions" with

[6]This assumption is due to the idea that if a PAC represents the producers of a certain crop, they are attempting to maximize those producers rents net of contributions, which would occur if substantial protectionist policies were employed with respect to the specific crop they are producing.

[7]This may create bias within the data, but there are no available data on the specific intentions of the general purpose contributions. Lopez 2001 used a similar system, allocating broad coalitions contributions "to the five largest commodities in proportion to their production values." (Lopez 2001)

respect to agribusiness to determine if campaign contributions truly "buy" influence, and to what extent they are valued relative to social welfare metrics. This is evaluated by determining the relationship between agribusiness PAC contributions and the extent of protectionist measures/transfers to the producers of the particular crop being contributed (quid pro quo) on behalf of. I hypothesize that there is an economically and statistically significant positive relationship between these two factors and that total agribusiness contributions have a smaller magnitude yet still statistically significant impact on producer support. This hypothesis reflects the findings in the literature, which demonstrates that crop specific contributions have large impacts on the nontariff barrier and and agricultural subsidies (Goldberg and Maggi, 1997 and Lopez, 2001). Further, the very fact that the agribusiness continues to make large campaign contributions suggests that there is a reason doing so; it is highly likely that this reason is direct compensation by way of producer support policies.

In order to determine this relationship, I run various regressions, with two different metrics of campaign contributions (gross dollar contributions per crop and contributions as a percent of crop specific value of production in that year) regressed on two metrics of protectionism: the percent Producer Support Estimate (%PSE, which represents policy transfers to agricultural producers, measured at the farm gate and expressed as a share of gross farm receipts) and the producer Nominal Assistance Coefficient (NAC, a ratio of the value of total gross farm receipts including support and the production valued at world market prices without support). The use of %PSE and NAC is to combat the bias contributed by a trend observed in 2013, by Kym Anderson, Gordon Rausser, and Johan Swinnen, in their paper "Political Economy of Public Policies: Insights from Distortions to Agricultural and Food Markets." Anderson, Rausser and Swinnen argue that the relative importance of farm-policy instruments has changed significantly over time, with the contribution of price-distorting measures such as the nontariff barriers declining (Anderson, Rausser and Swinnen, 2013). Nonetheless, they note that the anti-trade bias within the agriculture sector has persisted, but the policies chosen to promote support of this industry are constantly altered (Anderson, Rausser and Swinnen, 2013). Producer Support Estimates and Nominal Assistance Coefficients reflect most farm-policy instruments and thus cover the broad range of options available to policy makers; using these metrics renders the changes in policy instrumentation over time insignificant. Further, the %PSE and the NAC are correlated with a coefficient of .957, significant at the .001% level. Therefore, the two estimates are virtually interchangeable for one another and I will henceforth use exclusively %PSE (PSCTP). All regressions were also run with NAC (PNPC) and yielded nearly identical results.

To determine the campaign contributions, the contributions from agribusiness PACs are analyzed and each PAC is attributed to a specific crop or a set of crops, depending on what their constituents produce (as described in the data section).

Initially I will run the following simple OLS regression, similar to that of Gawande (2005):

$$Y_{it} = \beta_0 + \beta_1 X_{it} + \beta_2(\frac{\text{Ex}_{it}}{\text{Output}_{it}}) + \beta_3(\frac{\text{Im}_{it}}{\text{Output}_{it}}) + \epsilon_{it} \tag{1}$$

where all variables range over *i* agricultural products and *t* years. *Y* is the protectionism estimate (as %PSE and as NAC, in separate regressions), *X* is the campaign contributions from agribusiness PACs (as gross dollar contributions and first differences of contributions, in separate regressions). *Ex/Output* is the export to output ratio, and *Im/Output* is the import to output ratio, both of which will be used to represent Social Welfare of the policy, as discussed in Section III.

Campaign contributions are measured on a two-year cycle, and protectionist measures are observed yearly. To adjust for this inconsistency, the protectionist measures will be averaged between the two-years covered by a single data point for campaign contributions, to compensate for the fact that the contributions may have taken a year to take effect or be reflected in actual policy. Therefore, the *t* years in this regression (and all future specifications) include each two-year period from 1998-2016.

Then, I reform the model to the following regression, which utilizes the fact that the data are longitudinal to test a random effects model (2) and a fixed effects model (3):

$$\begin{aligned}(Y_{it} - \frac{1}{T}\sum_{t=1}^{T} Y_{it}) = \beta_0 + \beta_1(X_{it} - \frac{1}{T}\sum_{t=1}^{T} X_{it}) \\ +\beta_2(\frac{\text{Ex}_{it}}{\text{Output}_{it}} - \frac{1}{T}\sum_{t=1}^{T}\frac{\text{Ex}_{it}}{\text{Output}_{it}}) \\ +\beta_3(\frac{\text{Im}_{it}}{\text{Output}_{it}} - \frac{1}{T}\sum_{t=1}^{T}\frac{\text{Im}_{it}}{\text{Output}_{it}}) \\ +\epsilon_{it}\end{aligned} \tag{2}$$

$$\begin{aligned}(Y_{it} - \frac{1}{T}\sum_{t=1}^{T} Y_{it}) = \beta_0 + \beta_1(X_{it} - \frac{1}{T}\sum_{t=1}^{T} X_{it}) \\ +\beta_2(\frac{\text{Ex}_{it}}{\text{Output}_{it}} - \frac{1}{T}\sum_{t=1}^{T}\frac{\text{Ex}_{it}}{\text{Output}_{it}}) \\ +\beta_3(\frac{\text{Im}_{it}}{\text{Output}_{it}} - \frac{1}{T}\sum_{t=1}^{T}\frac{\text{Im}_{it}}{\text{Output}_{it}}) \\ +(\alpha - \frac{1}{n}\sum_{t=1}^{N}\alpha_i)) + \epsilon_{it}\end{aligned} \tag{3}$$

The primary change in methodology is that I will be using time panel data, from 1998-2016, whereas Gawande (2005) utilized data from 1991-2000 and ran separate regressions for each year of data (he took averages of 1991-1993, 1994-1996, and 1997-1999 and ran a "1993" "1996" and "1999" regression with each of these new datasets). Because there are many factors that are constant within a crop and that do not change substantially over time but do differ

between crops, such as the number of producers, historical relationship with the government, perception by the public, etc., using fixed effects, represented by a_i in regression 3, will allow for the removal of omitted variable bias. I also run a Hausman test to test whether fixed effects dominate random effects (expressed in regression 2), which is to say they allow for a model that better fits the data. When fixed effects dominate, this implies that a time panel model dominates an OLS regression as the correct functional form, so the Hausman test also allows for a judgment between models (3) and (1).

Secondarily, the metric I have chosen to represent support differs from Gawande (2005), in which six regressions were run for each year, with producer supports represented by quality assurance standards, specific tariffs, and countervailing duties. The use of a different protectionism metric can be attributed to an issue cited in Goldberg and Maggis paper (1997); federal governments do not make tariff policies alone. These policies are often crafted in collaboration with foreign governments or international organizations such as the WTO. Therefore, legislators do not have the direct power to impact tariff policies as a result of lobbying expenditures and campaign contributions. Transfers to producers, however, can be influenced (though the WTO has gained influence on this metric since the Uruguay Round).[8]

I also plan on expanding this regression to:

$$\begin{aligned}(Y_{it} - \frac{1}{T}\sum_{t=1}^{T} Y_{it}) = \beta_0 + \beta_1(X_{it} - \frac{1}{T}\sum_{t=1}^{T} X_{it}) \\ +\beta_2(\frac{\text{Ex}_{it}}{\text{Output}_{it}} - \frac{1}{T}\sum_{t=1}^{T}\frac{\text{Ex}_{it}}{\text{Output}_{it}}) \\ +\beta_3(\frac{\text{Im}_{it}}{\text{Output}_{it}} - \frac{1}{T}\sum_{t=1}^{T}\frac{\text{Im}_{it}}{\text{Output}_{it}}) \\ +\beta_4(\sum_{i=1}^{N} X_{it} - \frac{1}{T}\sum_{t=1}^{T}\sum_{i=1}^{N} X_{it}) \\ +(\alpha - \frac{1}{n}\sum_{t=1}^{N}\alpha_i)) + \epsilon_{it}\end{aligned} \tag{4}$$

to include a regressor representing the total industry contributions for a given year, to account for the hypothesis that agribusiness contributions are mentally aggregated and lead to higher support for the collective farm bill and transfers to agricultural producers. This is reflective of a finding put forth by Abler (1989), which showed that many agribusiness PACs represent the interests of the agribusiness sector on a broad scale. Since the Farm Bill sets policy both for agricultural programs and food subsidies, PACs support amendments that do not help the producers of their particular crop but rather benefit a potential coalition between farm PACs that represent various crops, working to push a mutual agenda and pass the bill as a whole (Abler, 1989). This implies that campaign contributions for any given product are not actually just so, but rather have an impact on policy for all other products as well. (Abler, 1989 in Callahan, 2016). I therefore investigate if the coefficient on this variable, $\sum_{i=1}^{N} X_{it}$, is more significant and of higher magnitude than that of X_{it}, the crop specific contributions.

Next, I run the following equation:

$$\begin{aligned}(Y_{it} - \frac{1}{T}\sum_{t=1}^{T} Y_{it}) = \beta_0 + \beta_1(X_{it} - \frac{1}{T}\sum_{t=1}^{T} X_{it}) \\ +\beta_2(\frac{\text{Ex}_{it}}{\text{Output}_{it}} - \frac{1}{T}\sum_{t=1}^{T}\frac{\text{Ex}_{it}}{\text{Output}_{it}}) \\ +\beta_3(\frac{\text{Im}_{it}}{\text{Output}_{it}} - \frac{1}{T}\sum_{t=1}^{T}\frac{\text{Im}_{it}}{\text{Output}_{it}}) \\ +\beta_4(\sum_{i=1}^{N} X_{it} - \frac{1}{T}\sum_{t=1}^{T}\sum_{i=1}^{N} X_{it}) \\ -\beta_5(p_{it} - \frac{1}{T}\sum_{t=1}^{T} p_{it}) - \beta_6(m_{it} - \frac{1}{T}\sum_{t=1}^{T} m_{it}) \\ +(\alpha - \frac{1}{n}\sum_{t=1}^{N}\alpha_i)) + \epsilon_{it}\end{aligned} \tag{5}$$

which includes a variable, p, to reflect the number of PACs lobbying on behalf of the specific crop, and a variable, m, representing the number of PACs lobbying for other crops in that year (due to the alternative possibilities that the other PACs are competitors or that they are furthering the same goal). I later refer to these variables as the "concentration" of the crops lobby and of the other-crop agribusiness lobbies. These variables will account for the finding in Gawande and Bandyopadhyays paper (2000) finding that the concentration of industry PACs and competing PACs is significant in determining support metrics.

As a final step, I will run the following regression:

$$\begin{aligned}(Y_{it} - \frac{1}{T}\sum_{t=1}^{T} Y_{it}) = \beta_0 + \beta_1(X_{it} - \frac{1}{T}\sum_{t=1}^{T} X_{it}) \\ +\beta_2(X_{i(t-1)} - \frac{1}{T}\sum_{t=1}^{T} X_{i(t-1)}) \\ +\beta_3(\frac{\text{Ex}_{it}}{\text{Output}_{it}} - \frac{1}{T}\sum_{t=1}^{T}\frac{\text{Ex}_{it}}{\text{Output}_{it}}) \\ +\beta_4(\frac{\text{Im}_{it}}{\text{Output}_{it}} - \frac{1}{T}\sum_{t=1}^{T}\frac{\text{Im}_{it}}{\text{Output}_{it}}) \\ +\beta_5(\sum_{i=1}^{N} X_{it} - \frac{1}{T}\sum_{t=1}^{T}\sum_{i=1}^{N} X_{it}) \\ +\beta_6(\sum_{i=1}^{N} X_{i(t-1)} - \frac{1}{T}\sum_{t=1}^{T}\sum_{i=1}^{N} X_{i(t-1)}) \\ +(\alpha - \frac{1}{n}\sum_{t=1}^{N}\alpha_i)) + \epsilon_{it}\end{aligned} \tag{6}$$

[8]The change in metric for support applies to both the first and second specifications of the model. The use of fixed and random effects applies exclusively to the second model, which will be compared to the first.

to include a time lagged variable for each contribution based variable $(X_{it}, \sum_{t=1}^{T} X_{it})$ and account for the possibility that contributions in a given year do not impact policy immediately. This is because campaign contributions will affect the future legislators, not necessarily the present ones, and because the Farm Bill which determines support quantities is passed every five years and so policies may not be able to meaningfully change on a two-year basis. I have represented this time lagged variable with a subscript of $t-1$ but each period t represents a two-year congressional cycle, and thus subtracting one actually indicates the former period, or two years prior.

V. I. Results and Discussion

Table 3 below demonstrates the results of various specifications of the relationship between PAC contributions and Producer Single Commodity Transfers as a percent of Production Value (the Y variable in all specifications). [9]

The most basic OLS specification of the regression yields the expected sign of the coefficient on the contributions variable, at a high significance level. The model suggests that for every 1% increase in the campaign contributions to crop value ratio is associated with a 23,451% increase in the Producer Single Commodity Transfers as a percent of value of commodity specific receipts. This magnitude of this coefficient is extremely large, which suggests that small changes in contributions can have enormous impacts on the support mechanisms that the interest group receives. This is reflective of the model observed in the literature. The size of the coefficient can be explained by the fact contributions are received when legislators are vulnerable and small contributions have the power to make large changes in legislators election prospects. However, there is a degree of uncertainty because the legislators are not guaranteed to win the election. Therefore, in order to ensure these contributions are made, legislators must sufficiently compensate the donors for the inherent risk in their investments.

The next specification of the model demonstrates that when the impact of total agribusiness contributions and the amount of PACs representing both the crop and the agribusiness industry aside from the crop are considered, the crop specific campaign contributions still have a statistically significant large positive association with support mechanisms for the particular crop. This impact is marginally lower, suggesting that the first specification may have picked up some omitted variable bias. The statistically significant negligible impact of the total agribusiness contributions is fascinating; it suggests that industry wide contributions do not have any impact on support mechanisms. Perhaps this is because each set of campaign contributions is competing with the other contributions for a higher allocation of the total funds available for agricultural support, while simultaneously working to pass the Farm Bill as a whole, which leads to a cumulative zero effect on crop-specific support. The concentration/quantity of PACs representing other crops has a statistically significant positive impact on the support mechanisms received; perhaps this suggests that when other crops are not well organized the crop of interest is more successful in receiving support mechanisms. This seems inconsistent with the fact that the concentration of lobbies for the particular crop do not have a significant impact on the support mechanisms received. Nonetheless, the crop specific contributions variable is associated with PSE as expected; every small contribution is magnified into large transfers to its producers. This initially suggests that the relationship between crop-specific campaign contributions and support mechanisms has not changed since Gawandes 2005 paper, which used data from the 1990s. Even running a regression for the years after 2000, at which point the Uruguay Round Agriculture Agreement had been fully implemented by developed countries including the United States, yields very similar coefficients.

However, when the support and contributions data is used as a panel, this historical relationship seems to erode. The random effects model yields an insignificant negative coefficient (opposite that which is predicted by theory and existing literature). However, a Hausman test between the fixed and random effects models indicates that the fixed effects model dominates with a p-value ¡ 0.0001. Given that the fixed effects very significantly fits the data better than a random effects level, the OLS regression is also implicitly dominated. For this reason, the random effects models are not displayed in Table 3, and the OLS specifications remain for the sake of comparison to existing literature.

The first specification of a fixed effects model, which controls for differences that are constant over time but differ per crop, yields a statistically insignificant enormous negative coefficient on the crop-specific contributions variable. The second specification yields a less negative and less statistically significant coefficient on the crop specific contributions variable, in addition to a highly significant zero coefficient on the total agribusiness contributions variable. Adding the concentration of the PACs representing the crop of interest and the concentration of PACs for other products in the industry has a negligible impact on the coefficients of interest, and the added variables are statistically insignificant. The next specification of the fixed effects model exclusively includes the time-lagged version of the crop specific contributions variable and the total agribusiness contributions variable. The current equivalents of these variables are excluded due to the high correlations between these variables and the time-lagged versions. The crop specific contributions have a correlation of .9542 with their non-lagged equivalents, and the total contributions have a correlation of .9838 with the non-lagged equivalent. This level of correlation would lead to a high level of co-linearity within the equation. Nonetheless, the time-lagged variable for crop specific contributions is statistically insignificant, and the total contributions variable has no correlation with support mechanisms at a highly significant level. The time-lagged crop-specific contributions variable remains negative but is of lower magnitude and

[9] All regressions shown in the table are using data in which extreme outliers have been removed; the removal of outliers had no significant effect on the coefficients displayed in Table 3.

higher statistical significance than its current equivalent. This suggests that there is a lag in the impact of contributions on policy. However, since the time-lagged variable remains statistically insignificant, no conclusive relationships can be drawn.

When the fixed effects regression is applied solely to the years after 2000, to reflect the efficacy of crop specific campaign contributions since the implementation of the Uruguay Round Agriculture Agreement, the coefficient on this variable of interest is positive but is also very highly statistically insignificant. This suggests that there is no significant correlation between crop specific campaign contributions and supports to producers of that crop since 2000. All other variables maintain similar coefficients, suggesting that prior analyses of their impacts still hold after 2000.

Taken together, the fixed effects specifications demonstrate that there is a statistically insignificant relationship between crop specific campaign contributions and supports transferred to the producers of that crop. The magnitude of the coefficients on this variable are large (and primarily negative), but this is likely due to the consistently extremely small contributions to production value ratios, which allow for very small changes to be associated with enormous changes in the far more volatile and much larger numbers represented by the support mechanisms (even as a percentage of crop production). The models consistently demonstrate a statistically significant lack of association between total agribusiness contributions and crop specific campaign contributions. The explanation for this relationship remains the same as for the OLS specification, as the significance and coefficient of the relationship do not change in the fixed effects specifications. Overall, these results show that none of the tested factors significantly predict the support mechanisms, contrary to what is demonstrated in the literature. It appears that the strong positive association between crop specific contributions and support mechanisms observed in the literature may be entirely due to omitted variable bias that is incorporated into the OLS models. Running a Ramsay Reset Test rejects the null hypothesis that the OLS specifications have no omitted variables with a significance level below .001 %, suggesting that the linear regression used in the literature does suffer from omitted variable bias.

To determine why the OLS regression yields such a significant, high magnitude, positive coefficient (even with clustered standard errors by crop, robust to heteroskedasticity), I also ran separate OLS regressions for each crop group (see Appendix). From these regressions we gather that an increase in contributions as a percent of yearly production value is only associated with an increase single commodity transfers to producers in any two of the fourteen products studied at a statistically significant level (cotton, maize); a statistically significant negative association is observed in one of the products studied (milk). However, the OLS regression contains omitted variable bias from the fact that some crops which consistently make more campaign contributions also consistently receive more transfers and vice versa. This could be due to a historical relationship with policymakers, for which the producers are paying maintenance fees, or due to specific features inherent to particular crops that are unrelated to contributions. When we hold constant for these crop-specific features, we no longer observe the relationship that exists in the literature on this topic.

This begs the question, if campaign contributions do not result in support mechanisms for the donors, then why are the contributions being made? It is possible that support mechanisms, mainly transfers, covered by the PSE are not what the lobbies desire. Perhaps there are desired standards and regulations that would benefit crop producers abilities to sell their products effectively and these are of higher priority than direct transfers. An example of these policies is the USDAs "organic" standard, which has far fewer restrictions and qualifications than that of many European economies. The lack of emphasis on transfers may also be due to the external constrains on how the United States government can support farmers, resulting from trade agreements. Another possibility is that there is merely misinformation about the efficacy of the contributions. Since contributions come from a large variety of PACs and transfers are spread among many producers, the trends between these variables may not be easily observable to those making the contributions. Finally, another possibility is that there was a statistically significant positive relationship between crop specific campaign contributions and transfers to agricultural producers prior to the full implementation of the Uruguay Agriculture Agreement. However, as implementation of support is restricted the ability to translate campaign contributions directly into transfers to the contributors has become more difficult, and this change has not yet been adjusted for by agribusiness PACs.

VI. Conclusion and Limitations

This paper observes an insignificant trend between crop specific campaign contributions and producer single commodity transfers when omitted variable bias particular to each crop is accounted for using a fixed effects regression. The total agribusiness contributions have a consistently significant near zero effect on support mechanisms. This work is in contrast with most existing literature on purchasing policy in the agribusiness sector and sheds light on the deficiencies of past models used to study this relationship. The change may also be due to a changing political climate and attitudes toward agriculture protection in the 21st century.

There are potential pitfalls with my methodology however. It is possible that crop specific contributions and total yearly contributions display multi-collinearity and follow the same trend over time. When the total agribusiness contributions variable is added, the coefficient on crop specific contributions changes significantly, and its standard error increases. Nonetheless, the coefficient remains statistically significant and of large magnitude, so I do not consider this a detriment to the study. Another potential pitfall is rooted in the allocation of general purpose agribusiness PAC contributions to specific crops. As described in Section III, when PACs represent a general agricultural lobby, depending on if they also include egg/meat production, they are indicated as "all"

or "all crops." Then, the contribution is divided on a weighted basis depending on the percent of total US production value that agricultural product represents. This is problematic when it comes to State-specific PACs (e.g. Illinois Agricultural Association, California Farm Bureau), because each crop represents a different percent of total production on a state-by-state basis than it does on a national basis. If the intuition behind the initial allocation (PACs lobby for the crops that are more prominent among their producers) holds, then allocation of contributions from State-specific PACs should be done separately from multipurpose national PACs. This is an area for future research and work in data reprocessing.

Further, when I use the concentration of competing or contributing lobbies as a variable (quantity of PACs lobbying on behalf of that product), I am only accounting for other agribusiness lobbies. This excludes processed food lobbies and other downstream industries which also have the potential to support agribusiness subsidies and oppose agribusiness tariffs (somewhat contributing and somewhat competing with the interests of the agricultural product producers).

In addition, there is problem of price endogeneity. Support mechanisms represented by the PSE are dependent on the market prices; dividing the crop specific contributions term by production value may lead to omitted variable bias in that the support mechanisms are linked with the production values. However, this relationship is not particularly evident in the data; the peak years of food price indices in this time period, including 2001, 2008 and 2011, and years in which major food price drops occur, mainly the period from 2004-2006, do not represent turning points in the trend or amount of support. (USDA, 2016, Figure 1) If there is correlation of price and support, however, this may lead the regression dependent variables to be correlated with the error term and has the potential to introduce bias into the model.

Future research should be conducted on the impact of donations to the Democratic and Republican parties, and if party affiliation impacts the efficacy of the contribution in translating into transfers. Another area for further research includes the purposes of high campaign contributions, in the case that transfers are not the desired result. Potential options include regulations, marketing controls, and laws regarding organic produce and genetically modified organisms.

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APPENDIX

Table 1: Summary Statistics by Year

	Observations	Crop Specific Total Contributions (Mean)	Crop Specific Total Contributions (SD)	Export/ Output (Mean)	Export/ Output (SD)	Import/ Output (Mean)	Import/ Output (SD)	PSCTP (Mean)	PSCTP (SD)	PNPC (Mean)	PNPC (SD)
Units		Millions of 1998 Dollars	Millions of 1998 Dollars	Ratio	Ratio	Ratio	Ratio	%	%	Ratio	Ratio
Year											
1998	12	.5292	.5135	0.18	0.16	0.20	0.57	13.13	17.61	1.21	0.37
2000	12	.5754	.5376	0.21	0.18	0.16	0.40	20.88	21.51	1.46	0.94
2002	13	.6290	.6361	0.25	0.23	0.14	0.28	16.78	17.81	1.22	0.33
2004	14	.7184	.7207	0.23	0.21	0.15	0.30	13.45	15.65	1.20	0.32
2006	13	.8501	.8479	0.27	0.25	0.17	0.30	7.30	8.94	1.07	0.11
2008	13	1.0363	1.0276	0.26	0.27	0.14	0.27	7.39	10.73	1.07	0.15
2010	14	1.0648	1.0836	0.28	0.25	0.13	0.25	6.64	9.25	1.07	0.14
2012	14	1.0929	1.0852	0.25	0.23	0.15	0.25	5.21	5.39	1.03	0.07
2014	13	1.1569	1.1386	0.28	0.24	0.16	0.30	6.69	7.26	1.05	0.10
2016	14	1.1998	1.1072	0.28	0.26	0.15	0.33	6.69	8.50	1.05	0.13

Sources: USDA, OECD, OpenSecrets
Notes: PSCTP indicates Producer Single Commodity Transfers as a percent of value of commodity specific receipts
PNPC indicates Producer Nominal Protection Coefficient

Table 2: Summary Statistics by Agricultural Product

	Ob ser- vati ons	Crop Specific Contributions (Mean)	Crop Specific Contributions (SD)	Crop Specific Contributions / Production Value (Mean)	Crop Specific Contributions / Production Value (SD)	Export Output Ratio (Mean)	Export Output Ratio (SD)	Import Output Ratio (Mean)	Import Output Ratio (SD)	PSCTP (Mean)	PSCTP (SD)	PNPC (Mean)	PNPC (SD)
Units		Millions of 1998 Dollars	Millions of 1998 Dollars	Ratio * 1000	Ratio * 1000	Ratio	Ratio	Ratio	Ratio	%	%	Ratio	Ratio
Crop													
Barley	10	.0452	.0159	.0544	.0186	0.08	0.05	0.08	0.03	6.57	3.93	1.04	0.06
Beef	9	1.3066	.3594	.0329	.0062	0.08	0.03	0.11	0.02	0.62	1.67	1.01	0.02
Cotton	10	.5799	.1578	.1219	.0398	0.68	0.20	0.00	0.01	21.52	10.58	1.22	0.18
Soybeans	9	.5313	.2865	.0184	.0036	0.34	0.05	0.00	0.00	5.24	6.12	1.04	0.08
Sheep	9	.0305	.0052	.0801	.0227	0.05	0.03	0.91	0.27	9.51	3.54	1.09	0.03
Sorghum	10	.0905	.0507	.0713	.0343	0.48	0.15	0.00	0.01	10.80	4.73	1.03	0.05
Eggs	10	.2138	.0553	.0359	.0083	0.03	0.01	0.00	0.00	0.00	0.00	1.00	0.00
Poultry	10	.8315	.1372	.0342	.0071	0.16	0.02	0.00	0.00	0.02	0.03	1.00	0.00
Sugar	10	3.0299	1.0438	1.2084	.3376	0.02	0.01	0.33	0.11	37.29	12.54	1.64	0.33
Maize	9	.9012	.4588	.021	.0048	0.16	0.04	0.00	0.00	5.50	3.64	1.03	0.05
Pig	9	.4619	.1481	.0311	.0053	0.15	0.06	0.04	0.01	0.14	0.42	1.00	0.00
Wheat	10	1.4484	.4834	.1467	.0261	0.47	0.06	0.05	0.01	7.85	3.22	1.03	0.05
Rice	8	.5301	.2317	.2297	.0489	0.48	0.05	0.08	0.02	9.52	14.91	1.14	0.26
Milk	9	2.0455	.5224	.0693	.0224	0.00	0.00	0.00	0.00	23.87	17.03	1.40	0.38

Sources: USDA, OECD, OpenSecrets
Notes: PSCTP indicates Producer Single Commodity Transfers as a percent of value of commodity specific receipts
PNPC indicates Producer Nominal Protection Coefficient

The total number of observations for both Table 1 and Table 2 is 132; this reflects the removal of extreme outliers. The outliers were beef in 2014, soybeans in 2000, sheep in 1998, maize in 2006, pig in 1998, rice in 2000 and 2002, and milk in 2008, all of which were more than three standard deviations away from their crop specific PSCTP mean. Tables 1 and 2 reflect the quantity of observations after the removal of outliers. The removal of outliers had the effect of largely normalizing the residuals between predicted and observed Y variables.

Table 3: OLS, Random and Fixed Effects Regression Coefficients

Dependent Variable:	OLS	OLS	OLS After 2000	Panel Fixed	Panel Fixed	Panel Fixed	Panel Fixed	Panel Fixed After 2000
Crop Specific Contributions/ Crop Production Value	**23451.220*** **	**22607.95*** **	**21218.99*** **	**-10385.260**	**-4312.117**	**-4477.492**		**1293.533**
	0.000	0.000	0.000	0.114	0.471	0.462		0.845
Import/Output Ratio	**0.153**	**3.607**	**4.362*** **	**-23.122*** **	**-10.748**	**-11.203**	**-17.875**	**-19.941** **
	0.978	0.386	0.004	0.009	0.188	0.179	0.066	0.031
Export/Output Ratio	**2.652**	**4.485**	**7.609**	**-7.475**	**2.366**	**2.320**	**-.005**	**0.377**
	0.825	0.730	0.115	0.334	0.742	0.748	1.000	0.963
Total Agribusiness Contributions		**0.000*** **	**0.000*** **		**0.000*** **	**0.000*** **		**0.000*** **
		0.006	0.002		0.000	0.000		0.000
Crop Specific PAC concentration		**.208**	**0.128**			**-0.106**		**0.010**
		0.441	0.147			0.740		0.976
All-Other-Crop PAC Concentration		**.015** **	**3.456**			**0.020**		**0.014**
		0.012	0.722			0.448		0.596
Time-lagged Crop Specific Contributions/ Crop Production Value							**-9915.458**	
							0.123	
Time-lagged Total Agribusiness Contributions							**0.000*** **	
							0.000	
Constant	**5.380**	**3.816**	**3.456**	**15.796*** **	**22.405*** **	**15.157**	**23.646*** **	**13.831**
	0.258	0.558	0.317	0.000	0.000	0.109	0.000	0.103
N	132	132	108	132	132	132	113	108
R Squared	0.333	0.462	0.527	0.136	0.004	0.015	0.138	0.001
F-Statistic	111.5	121.22	12.87	16.46	17.43	16.4	15.32	11.07

P-values are beneath coefficients and not in bold.
*** p<0.01, ** p<0.05, * p<0.1

In the following tables we observe the insignificant correlation of campaign contribution within each crop. Each of these regressions has a small amount of observations, but seven of the fourteen regressions exhibit negative coefficients of high magnitudes on the contributions variable. Further, the total agribusiness contributions have a consistently significant coefficient of approximately zero across crops. The import and export to output ratios are rarely significant and vary in sign.

Table 4

	(1)	(2)	(3)	(4)	(5)
VARIABLES	OLS Barley	OLS Beef	OLS Cotton	OLS Soybeans	OLS Sheep
Crop Specific Contributions/ Crop Production Value	13,623	-24,773	143,133*	-237,051	37,653
	(55,811)	(21,788)	(65,208)	(259,344)	(46,303)
Export/Output Ratio	-20.12	8.757	4.852	55.81	-47.10
	(22.94)	(7.696)	(20.72)	(81.51)	(27.00)
Import/Output Ratio	-7.023	10.60	-218.0	109.5	2.919
	(27.20)	(9.074)	(249.8)	(348.2)	(4.052)
Total Agribusiness Contributions	0.000***	0.000	0.000***	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Constant	22.40**	-0.673	44.98**	1.582	13.65*
	(6.801)	(0.597)	(11.67)	(8.676)	(5.590)
Observations	10	9	10	9	9
R-squared	0.800	0.460	0.860	0.261	0.792

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

Table 5

	(6)	(7)	(8)	(9)	(10)
VARIABLES	OLS Sorghum	OLS Eggs	OLS Poultry	OLS Sugar	OLS Maize
Crop Specific Contributions/ Crop Production Value	24,085	-92.46	1,283	8,243	374,699*
	(42,747)	(99.61)	(2,776)	(14,814)	(137,648)
Export/Output Ratio	1.887	-0.548	1.281	-30.28	-65.12
	(5.351)	(0.325)	(0.993)	(192.7)	(51.47)
Import/Output Ratio	36.09	-0.294	4.289	-96.31***	-313.8
	(47.29)	(0.209)	(51.98)	(21.85)	(299.3)
Total Agribusiness Contributions	-1.20e-06**	1.40e-09	-5.77e-09	-1.05e-06	-1.31e-06*
	(3.83e-07)	(7.66e-10)	(1.63e-08)	(1.74e-06)	(5.10e-07)
Constant	22.49**	0.003	-0.171	72.23***	25.03
	(5.848)	(0.007)	(0.239)	(4.385)	(12.71)
Observations	10	10	10	10	9
R-squared	0.598	0.644	0.238	0.887	0.662

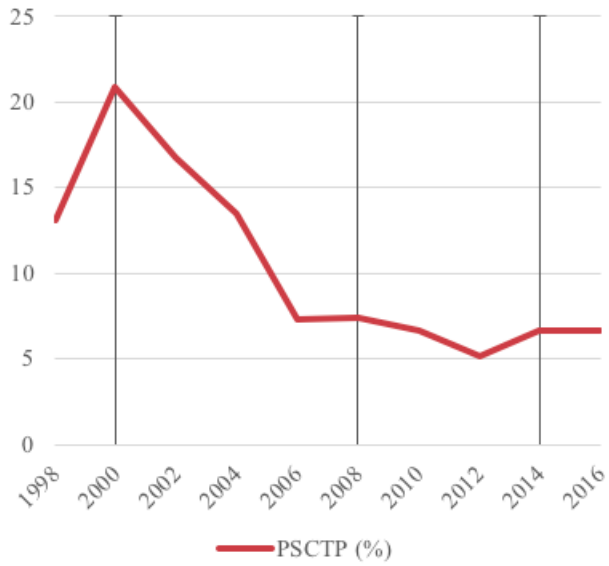
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6

	(11)	(12)	(13)	(14)
VARIABLES	OLS Pig	OLS Wheat	OLS Rice	OLS Milk
Crop Specific Contributions/ Crop Production Value	0.644	23,105	45,788	-310,166**
	(0.582)	(49,985)	(26,373)	(80,253)
Export/Output Ratio	0.000	-1.438	-3.173	-3,936
	(0.000)	(17.99)	(18.31)	(5,306)
Import/Output Ratio	0.002*	16.79	-96.95	-31,012
	(0.001)	(133.8)	(73.60)	(52,259)
Total Agribusiness Contributions	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Constant	-0.000184**	10.36	5.591	99.22***
	(0.000)	(10.97)	(7.874)	(17.91)
Observations	9	10	8	9
R-squared	0.846	0.232	0.680	0.935

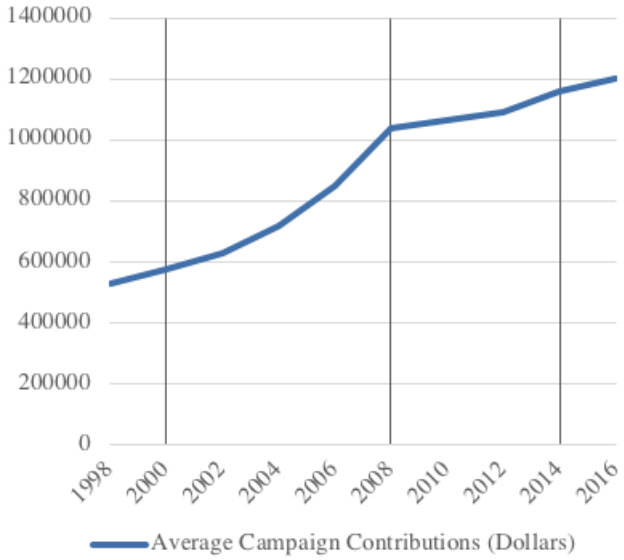
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Figure 1: Support Over Time



Sources: USDA, OECD
Notes: PSCTP indicates Producer Single Commodity Transfers as a percent of value of commodity specific receipts
Vertical lines indicate years in which Farm Bills are voted on

Figure 2: Contributions Over Time



Sources: OpenSecrets

Notes: Vertical lines indicate years in which Farm Bills are voted on

The ACA Medicaid Expansion's Impact on Bankruptcy

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***Abstract*— This paper explores the relationship between the ACA Medicaid Expansion and the bankruptcy rate in states that did and did not expand their Medicaid programs. In general, past research has found that the Medicaid expansions of the 1990s are associated with a decrease in bankruptcy rates, but as of yet, there has been little formal research into changes in the bankruptcy rate as a result of the 2014 Medicaid expansion. Utilizing a difference-in-differences analysis, I find that the bankruptcy rate fell more in states that did expand than those that did not expand in the post-expansion timeframe, lending evidence to the 2014 ACA Medicaid expansion having decreased the bankruptcy rate.**

I. Introduction

Bankruptcy has a wide variety of causes, that range from losing a job to an over utilization of credit cards, with medical costs being arguably one of the largest contributors to bankruptcy within the United States (Himmelstein 2005). In the United States, one could incur medical costs of close to $20,000 from a car accident (Haller 2015) — an unattainable amount for uninsured individuals, especially those who have little to no savings, or live paycheck to paycheck. Even with monthly payment plans, these individuals may still be forced to file for bankruptcy. There has been no shortage of studies and opinions on the ACA Medicaid expansion as a whole: DeMint of the Heritage Foundation (2015) called the Medicaid Expansion a program that will cause "permanent damage to America" and "[doom] many Americans to second-rate healthcare", and many argue that Medicaid is a job-destroying scheme that makes people lazier and destroys the economy. In more scholarly work, the Kaiser Family Foundation (Antonisse 2017) finds in a summary of 153 studies that the Medicaid expansion was associated with more positive health and economic outcomes at a state level. However, there has been little formal research into whether or not the ACA Medicaid expansion had an impact on the number of bankruptcies.

This paper uses a difference-in-differences approach to estimate the impact of the ACA Medicaid expansion on the rate of personal bankruptcies between 2012 and 2016. As falsification tests, I explore specifications with Chapter 7 and Chapter 13 bankruptcy rates and then repeat the process with the individual and business bankruptcy rates. My results offer evidence that the Medicaid expansion is associated with a decrease in the bankruptcy rate in states that expanded their Medicaid program when compared with states that did not expand.

II. Basic Theory

There are two chapters in the bankruptcy code that the majority of individuals file under: Chapter 13 and Chapter 7. Under Chapter 13, individuals must repay part of their debts for five years post-filing, but are allowed to retain most of their assets. On the other hand, under Chapter 7, individuals have all of their debts forgiven, but must give up any assets above an exemption level. To qualify for Chapter 7 bankruptcy, individuals must make less than the median income of their state. Given the benefits of Chapter 7 when compared to Chapter 13 in most cases, it does not make sense for one to file under Chapter 13 unless forced to by having high income, having assets above the exemption level (which is unlikely if one is low income), by owing debts not dischargeable in Chapter 7 bankruptcy (tax bills and traffic fines, among others), or because there is a local legal or social culture of filing under Chapter 13 — that is to say, a sense of moral obligation to repay some debts (Hackney 2015). Logically, high medical debts and being uninsured would thus be associated with filing under Chapter 7 instead of Chapter 13, and indeed, this is confirmed in the literature (Domowitz 1999). To put it simply, it is likely that any impacts on the bankruptcy rate from a Medicaid expansion would be localized in the Chapter 7 bankruptcy count.

More generally, the decision to file for bankruptcy is driven by a cost/benefit analysis. The benefits are, of course, a slate wiped clean of any debt, or being able to manage bills without the threat of legal action. However, there are downsides of filing for bankruptcy — social stigma (Gross 2002) and a mark on one's credit report. Even given the benefits of bankruptcy, we should not underestimate the impact of these two, seemingly minor downsides on real individuals. Consider the story of Stephanie, an uninsured woman in her late 20s who was forced to declare bankruptcy over $16,000 in medical bills that resulted from injuries afflicted by her abusive partner. This bankruptcy left her unable to open a credit card or rent an apartment, and cost her numerous job offers, leaving her feeling like a social outcast (Land 2016). Clearly, it is quite understandable why White (1998) found that fewer people file for bankruptcy than would actually benefit from it.

Aside from social stigma and a mark on the credit report, there are a few other reasons people would choose not to file for bankruptcy. If a lender does not take action against a borrower to collect the loan, the borrower benefits as if they

had filed for bankruptcy and avoids the difficulty and stress of filing for bankruptcy (White 1998). The value of having the option to file for bankruptcy may also be involved in the decision — having the option to file for bankruptcy in the future might be more valuable than filing for bankruptcy itself (White 1998).

III. Literature Review

Prior research often focuses on what external factors push an individual into bankruptcy. From this perspective, adverse shocks such as job loss and medical bills push some into bankruptcy, while for others, the overuse (and easy access to) credit cards is a factor of more importance (White 2007). Homeownership and higher per-capita income decrease the likelihood an individual will file for bankruptcy of any sort (Domowitz et al. 1999). Domowitz et al. (1999) also find that high medical debt is the single largest indicative of bankruptcy potential, specifically under Chapter 7 of the bankruptcy code. This strand was supported in a medical study performed by Hollingworth et al. (2007), which found a significant increase in bankruptcies after an individual had a serious brain or spinal cord injury. Medical debt seems to be different than other kinds of debt: Brevoort (2015) found that high levels of medical debt are not predictive of being behind on other payments.

Other researchers focus on the individual's decision to file for bankruptcy, suggesting that in a way, bankruptcy acts as its own version of high-deductible health insurance (Mahoney 2015). Koch (2014) links the decrease in the insurance rate after the enactment of the Emergency Medical Treatment and Active Labor Act (EMTALA) in the mid-1980s to an increase in the bankruptcy rate over the same period. Of course, it is important to keep in mind that high medical debt and/or a lack of insurance is not the only reason that bankruptcies increased — as Livshits et al. (2010) point out, Canada, which has national health insurance, also experienced a similar growth in their bankruptcy rate over the same timeframe.

Focusing more specifically on medical bankruptcy, the literature is in disagreement on the percentage of bankruptcies that are "true" medical bankruptcies. The percentage varies wildly, depending mostly on whether one uses survey data or courthouse record data. Survey data, where individuals are surveyed on what they think was behind their bankruptcy, as well as what kinds of debts they had prior to declaring bankruptcy, tends to find that 40-50% of bankruptcies are medically induced (Himmelstein et al. 2005 and 2009).[1] Meanwhile, courthouse records analysis, where individuals examine what assets and debts are actually listed in bankruptcy filings, tends to find a much lower medical bankruptcy rate: 17% (Dranove 2006) and 23.1% (Hackney 2015).

Jacoby et al. (2010) attempted to explain this discrepancy with a large data set that combines courthouse and survey data. They found that individuals with other credit options often shift medical debts to payment methods that appear generic on courthouse records. In other words, instead of debts being owed to a hospital, they are owed to a credit card company or a bank. In particular, non-African Americans and homeowners are more likely to perform this credit-rebalancing maneuver (Jacoby 2010). However, even given this explanation, there is still not a definitive answer as to what the exact medical bankruptcy rate is or which type of data is "better". My research will focus on the overall bankruptcy rate, rather than attempting to determine the exact percentage of medical bankruptcies, so as to avoid this ambiguity.

The vast majority of research into medical bankruptcies is not experimental in nature: it merely explores existing data, making it difficult to tease out cause and effect. Public health insurance expansions tend to vary by state, making these the perfect subject for quasi-experimental research. Previous natural experiments into medical bankruptcy and public health insurance within the United States focused on two shocks: the Medicaid expansions of the 1990s, and the Massachusetts 2006 health care reform. Gross (2011), following in the steps of Yarbrough (2007), used a difference-in-differences regression to analyze the impact of Medicaid expansions at a state level between 1992 and 2004. This research found that an increase in the Medicaid eligibility rate led to a decrease in consumer bankruptcies. More specifically, Gross (2011) found that a 10% increase in the Medicaid eligibility rate in a state was associated with an approximate 8% decrease in consumer bankruptcies. Upon repeating these results at a per-zip code level, he found nearly identical results.

Other researchers found similar results when studying the impacts of other public health care expansions in the United States: Arrieta (2013), in an analysis of financial statements for hospitals, found that the health care reform in Massachusetts seems to have decreased the bad debt ratio, which implies that hospitals were getting reimbursed for their services more often. However, Badding et al (2012), using a difference-in-differences analysis, compared the bankruptcy rate in Massachusetts between 2006 and 2010 with surrounding states, and found that post-Massachusetts reform, the bankruptcy rate actually increased. This is not the only ambiguous, or unexpected result found when public health care programs were expanded. Using an intent-to-treat OLS model, Finkelstein (2012) found that participants who enrolled in the Oregon Healthcare System had no statistically noticeable reduction in bankruptcy. It is important to note, however, that Finkelstein's study was performed 1 year post-expansion: in theory, public health expansions might have a lagged effect on bankruptcies.

Indeed, this lagged effect seems to be supported by more recent research, although the extent of the lag is unknown. Mazumder (2016) found less ambiguous results from the Massachusetts health expansion than did Badding (2012): using consumer credit information panel and demographic data at a zip code level, as well as a triple difference regression

[1] Beware that they reclassified individuals as medically bankrupt if they had large amounts of medical debt, even if the individual said that those debts were not behind their bankruptcy, as Dranove (2006) points out.

method to compare individual outcomes in both more and less affected Massachusetts areas, in addition to those outside of Massachusetts, Mazumder found the expected results. The probability of a bankruptcy for individuals in Massachusetts fell post-Massachusetts healthcare reform — particularly for those with low credit scores prior to the health care overhaul.

This brings us to the topic of my paper: the current Medicaid expansion. Recent research has covered numerous aspects of the expansion: Slusky (2017), using a difference-in-differences regression, finds that the ACA Medicaid expansion was associated with a decrease in the divorce rate for individuals between 50-64 years old — the main demographic group that divorces due to high medical bills, given that this group is not eligible for Medicare unless they meet strict asset requirements which are much more difficult to reach when a couple is married. Other research focuses on who the Medicaid expansion impacted the most: Courtemanche et al. (2017), using a difference-in-differences model, find that the increased eligibility for Medicaid in expansion states resulted in an increase in the insurance rate for non-white, unmarried individuals between 18-34 years old. This increase for non-white individuals is not because of the state's demographic makeup: Wherry et al. (2016) found that the individuals in expansion states were more likely to be white — that is, the expansion states consisted of mostly white individuals, and yet the majority of the direct impact of the Medicaid expansion was felt by non-white individuals. Meanwhile, the Kaiser Family Foundation summary of the research literature finds that there was a decline in individuals in Medicaid expansion states that reported having medical debt, or medical bills in collection (Antonisse 2017). Recent research from Brevoort et al. (2017), using a difference-in-differences model with data at a census-tract level from the Consumer Credit Panel, finds that the Medicaid expansion is responsible for a decrease in medical debt and past-due bills among individuals that qualify for Medicaid. Therefore, one would expect that bankruptcy rates would fall more in expansion states than in non-expansion states.

There are much circumstantial evidence and many anecdotes on this topic. However, there seems to be a dearth of research into if the ACA Medicaid expansion actually impacted the bankruptcy rate. This paper aims to fill that gap.

IV. Data

Ideally, I would have a state-level count of individuals who filed for bankruptcy due to high medical costs. However, there's no way to directly ascertain this given my resources and the data I was able to access. My data instead consists of yearly state-level bankruptcy data from the US Courts website, joined with state-specific demographic and economic data from GeoFRED and the United State Department of Energy. This data provides a count of bankruptcies for both individuals and businesses broken down by chapter of the bankruptcy code. I transformed this raw count into bankruptcies per thousand residents, to weight the bankruptcies in a state by population size. I retrieved the data on which (and when) states expanded their Medicaid programs under the ACA from the Kaiser Family Foundation.

Figure 1 presents whether or not (and when) a state expanded, to help visualize which (and when) states expanded. States that did not expand appear to be more likely to be Southern or Midwestern states, while states that did expand appear to be more likely to be Northern or coastal states. There are, of course, some clear and obvious exceptions, such as Arkansas — which expanded — and Maine, which has not expanded yet (although the Kaiser Family Foundation reports that a Medicaid expansion in Maine is imminent).

It becomes apparent when examining the data in Figure 2 that the individual bankruptcy rate for both expansion and non-expansion states fell over the data'' s timeframe. However, states that did expand had an almost unnoticeable greater decrease in the bankruptcy rate when compared with non-expansion states. This is confirmed in Figure 3. Expansion states seem to have a lower bankruptcy rate overall, both before and after the Medicaid expansion. However, the bankruptcy rate seems to have fallen more in expansion states, when compared with non-expansion states. When breaking down individual bankruptcies by Chapter of the bankruptcy code in Figures 4.1 and 4.2, it seems that the change is localized in Chapter 7 bankruptcies. It also seems that Southern, non-expansion states had a greater bankruptcy rate overall (seemingly localized in Chapter 13 bankruptcy filings).

Looking more quantitatively at the data, in Table 1 we see exactly what Figures 2 and 3 imply: the individual bankruptcy rate overall decreased, but it decreased more in expansion states versus non-expansion states. This relationship remains, even when including late expansion states. Overall then, it seems that states that expanded had a greater drop in bankruptcies than states that did not expand.

V. Model

Conceptually, my model tries to understand whether or not the ACA's Medicaid expansion decreased the bankruptcy rate in states that expanded as a function of whether or not the state expanded, whether we're in the timeframe post-expansion, the interaction between those two terms, and finally, some controls — in effect, a standard difference-in-difference model. I control for income, population, GSP, the price of oil and homeownership. Unless explicitly noted, states that expanded after 2014 are excluded. Many of these late-expanding states expanded in the middle of a year, instead of at the beginning of the year as the initial expanders did. Since my data is at a yearly level, it does not provide the granularity that is needed to accurately account for these late expansion states.

To econometrically analyze what this data is suggesting, I estimate the following difference-in-differences model:

$$\begin{aligned} Y_{it} =& \alpha + \beta_{it} Expanded + \beta_{2it} PostExpansion \\ &+ \beta_{3it} Expanded * PostExpansion + \phi_i \\ &+ \delta_{it} + \rho_{it} + \sigma_{it} + \gamma_{it} + \tau_t + \epsilon_{it} \end{aligned}$$

where Y_{it} is a bankruptcy rate in a specific state at a specific time, α is a constant, β_{it} is the coefficient on the dummy variable for being in a state that expanded, β_{2it} is the coefficient on the dummy variable for being in a period of time post-expansion, β_{3it} is the coefficient on the interaction between being in a state that expanded and being in the post-expansion time period, φ_i is a control term for state-level effects, δ_{it} is a control term for income, ρ_{it} is a control term for population, σ_{it} is a control term for homeownership rate, γ_{it} is a control term for GSP (Gross State Product), τ_t is a control term for time fixed effects, and ε_{it} is the error term.

VI. Results

I estimated 4 models with the individual bankruptcy rate per thousand as my dependent variable. I followed an incremental model building protocol, with the first model being a pure difference-in-differences model, with no controls. The second, third, and fourth models build on this, by including controls for state-specific economic variables, state-fixed effects, and time-fixed effects respectively. My results are summarized in Table 2. When controlling for state-level and time fixed-effects, in addition to state-specific economic characteristics, I found that Medicaid expansion led to a decrease in bankruptcy rates. This is consistent with much of the prior literature on the impacts of Medicaid expansion. In addition, it is what one would intuitively expect.

To more clearly illustrate this relationship, maps are presented in Figures 5, 6, and 7, which display the predicted deviation from average state-level change in bankruptcies year-over-year, based on the final specification. A negative deviation means that a state negatively deviated from trend bankruptcies per thousand, or that it had more of decrease in bankruptcies when compared with the nationwide average. A positive deviation meant that a state had less of a decrease (or even an increase) in bankruptcies when compared with the nationwide average for a state in that year. Looking specifically at Figure 5, we see that in 2013 — the year before the Medicaid expansion — there does not seem to be a relationship between whether a state was going to expand Medicaid, and how it deviated from the average change in bankruptcy year-over-year. This changes once the Medicaid expansion takes effect. In Figure 6, which displays this relationship for 2014 — the year that the Medicaid expansion took effect — we clearly see that being an expansion state is associated with a negative deviation from the average change in bankruptcy year-over-year; meanwhile, being in a non-expansion state is associated with a positive deviation from the average change in bankruptcy rate. Again, this matches with what the model tells us should happen, as well as what we would expect to happen. This case is further strengthened if we look at the 2014 and 2015 accumulated deviations from trend bankruptcy rate, presented in Figure 7. In this figure, we see that the trend seems to be cumulative. In other words, it does not seem to be a one time shock, but a sustained change in the bankruptcy rate between expansion and non-expansion states. The Medicaid expansion seems to have decreased the amount of individual bankruptcies filed within a year, when compared with non-Medicaid expansion states.

VII. Tests

To test the robustness of these results, I examined several possible re-specifications. The first was including states with a post-2014 expansion under the ACA. I marked each late-expansion state as post-expansion if the year in which the expansion had passed. For example, Pennsylvania expanded their Medicaid program at the beginning of 2015: they were therefore marked as being post-expansion for purposes of the difference-in-difference analysis from 2015 on. My results are presented in Table 3. These results seem to indicate that including these states has little effect on the model overall, aside from swapping the sign on the dummy for being in a world post Medicaid expansion, decreasing the sum of squared residuals, and modifying the statistical significance of all variables. It specifically impacts the expanded and post-expansion interaction variable, with this specification resulting in an increased t-stat of -2.01, which results in a p-value of .023 — strengthening the case for the ACA Medicaid expansion diminishing the number of bankruptcies. Residual analysis suggests that the fit of this model is similar to the fit of the original model. In other words, there does not seem to be a penalty associated with dropping states that expanded their Medicaid programs post-January 2014.

Residual analysis of both the original specification and the specification which included late-expansion states seemed to indicate that Nevada was having an outsized influence on my model, so I dropped it and reran my final specification to test if this was true. The results of this are presented in Table 3 as well. My results seem to be mostly unaffected by including (or removing) NV from my model — aside from minor changes to t-stats and the sum of squared residuals, little else has changed. In short, my specification seems to be quite robust.

As a falsification test, I reran my final specification twice: once with the state-level Chapter 7 bankruptcy rate as the dependent variable, and once with the Chapter 13 bankruptcy rate as the dependent variable. The results are summarized in Table 4. Given that individuals that file under Chapter 13 are typically high income (and thus likely to be either already insured or unqualified for Medicaid), while those that file under Chapter 7 are generally low income (and thus more likely to be uninsured, and qualify for Medicaid), if the Medicaid expansion is truly behind this decrease in bankruptcy, we would expect that the Chapter 7 bankruptcy rate model would have a noticeable decrease in the bankruptcy rate in expansion states when compared with non-expansion states, whereas the model with the Chapter 13 bankruptcy rate would be much more ambiguous. The factors that we must control for when modeling overall individual bankruptcies still must be controlled for when modeling by chapter of the bankruptcy code. Given this, all controls from the final specification have been included in this model.

Table 4 suggests exactly what we would expect to see if the Medicaid expansion were behind the decrease in the bankruptcy rate in expansion states: a decrease in the Chapter 7 bankruptcy rate in states that expanded after the expansion took effect, with an ambiguous impact on Chapter 13 bankruptcies in the same timeframe. Being in a state that expanded their Medicaid program is associated with a statistically significant decrease in Chapter 13 bankruptcies overall. However, the post-expansion timeframe and state expansion interaction term is positive (albeit close to zero), offering a mixed, ambiguous picture. It is important to note that this picture is clarified when we consider the p-value of 0.305, which implies that this relationship is not statistically significant. Simply put, given these ambiguities, my model passes this test — the relationship between being an expansion state and the change in Chapter 7 and 13 bankruptcy rates is different.

As an additional falsification test, I reran my final specification with business bankruptcies per thousand as my dependent variable. If the Medicaid expansion were in fact behind the decrease in individual bankruptcies, we would expect that rerunning the model with the business bankruptcy rate would result in a noise-filled model with a coefficient on the interaction term that is close to zero, assuming my data is unbiased — that is to say, intuitively, we would expect business bankruptcies to be relatively unimpacted by the Medicaid expansion. Indeed, this is what we see in the results presented in Table 5. Residual analysis seemed to indicate that Delaware was biasing this model, so I ran it twice: once with, and once without Delaware. In both cases, we see that the state-expanded and post-expansion timeframe dummy interaction term is close to zero and positive, with a non-statistically significant p-value of 0.101, which is consistent with the hypothesis that business bankruptcies were relatively unimpacted by the Medicaid expansion. In other words, my model passes this falsification test: it does indeed seem that there is evidence that the Medicaid expansion decreased the individual bankruptcy rate.

Taken together, these tests provide further of evidence that what my results suggest is true: that the Medicaid expansion is associated with a decrease in the individual bankruptcy rate — specifically, a decrease seemingly localized in Chapter 7 bankruptcies.

VIII. Conclusion

My research finds evidence that the Medicaid expansion has led to a decrease in the individual bankruptcy rate — to be precise, a decrease in the Chapter 7 bankruptcy rate. These results are strengthened by several robustness checks and falsification tests. Changing how this model was specified had little effect on the results; rerunning my model with the business bankruptcy rate suggests that the business bankruptcy rate was relatively unimpacted by the Medicaid expansion, while breaking down my specification by chapter of the bankruptcy code finds that Chapter 7 bankruptcies decreased after the expansion in expansion states while Chapter 13 bankruptcies were more ambiguous. These results are what one would expect to find, based on what literature suggests should happen when low income, uninsured individuals are given access to health insurance. My results are limited by my timeframe — it seems likely that these results would be persistent beyond 2016, but we cannot be certain. My results are also limited by the fact that I am measuring the overall bankruptcy filing count, and not the actual medical bankruptcy rate.

From a policy perspective, if lawmakers in states that did not expand Medicaid decided to expand their state's Medicaid program, they would see a decrease in their bankruptcy rates. A reduced bankruptcy rate would obviously be a good thing for any state — fewer bankruptcies means that more lenders are getting paid, which most would agree is good for the economy. Any policy that would decrease these rates would be something that most policymakers should be willing to support, regardless of their thoughts on welfare — and that fails to even consider the improved health outcomes, lifespans, and quality of life that the low income citizens of these states would no doubt gain from receiving health insurance coverage under Medicaid.

From this point of view, future research should focus specifically on how the Arkansas and Louisiana Medicaid expansions impacted these states when compared with surrounding states, given that many of the non-expansion states were Southern, while many of the expansion states were Northern or coastal. It is possible that we would see a somewhat different outcome in Southern states that expand their Medicaid programs, given that Southern states are quite different from Northern states in terms of demographics, culture, and beliefs.

Also of interest for future researchers would be a focus on how many people actually received coverage through the Medicaid expansion — what the take up rate was, as well as how many people became eligible for Medicaid due to the expansion. This would allow us to determine an elasticity for Medicaid expansions with respect to the bankruptcy rate — updating Gross (2011) — which would be very informative for policymakers in terms of doing a cost-benefit analysis of the Medicaid expansion.

IX. Acknowledgements

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APPENDIX

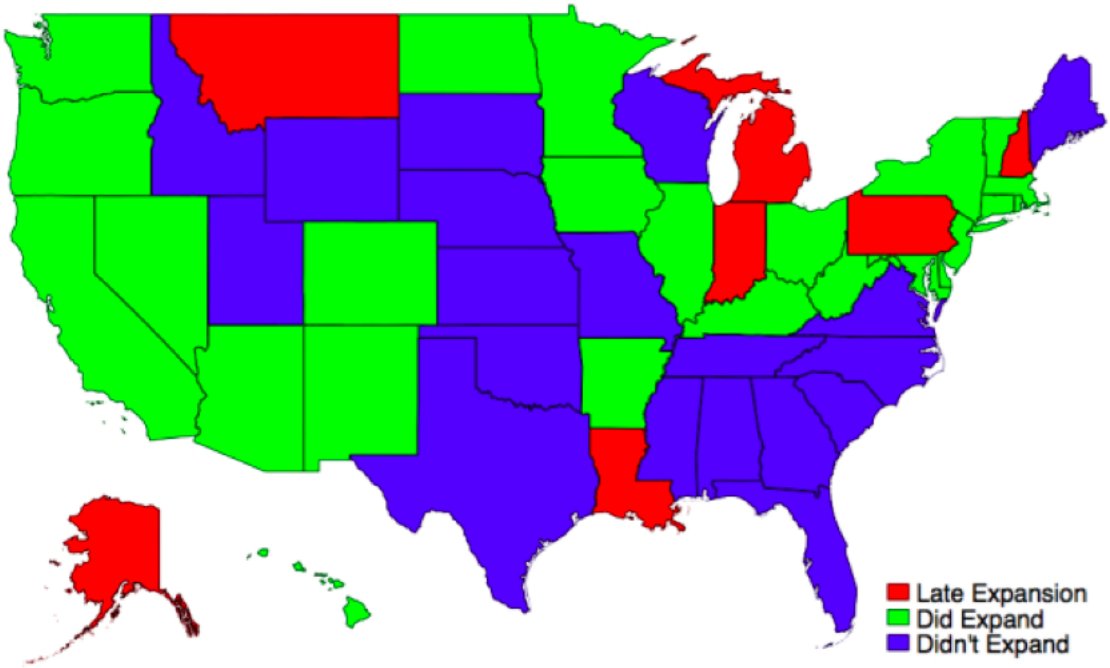

Figure 1: Which (and when) states expanded their Medicaid program under the ACA (as of 2017)

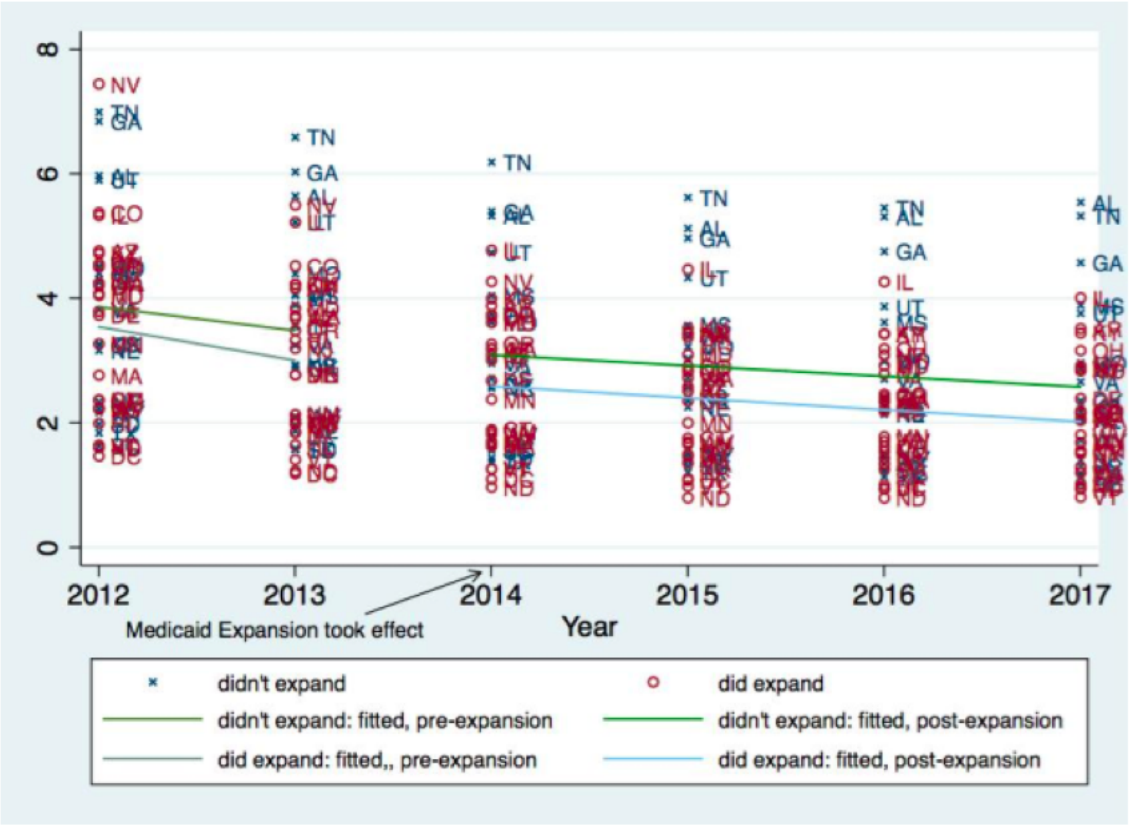

Figure 2: State level count of individual bankruptcies per thousand with fitted lines.

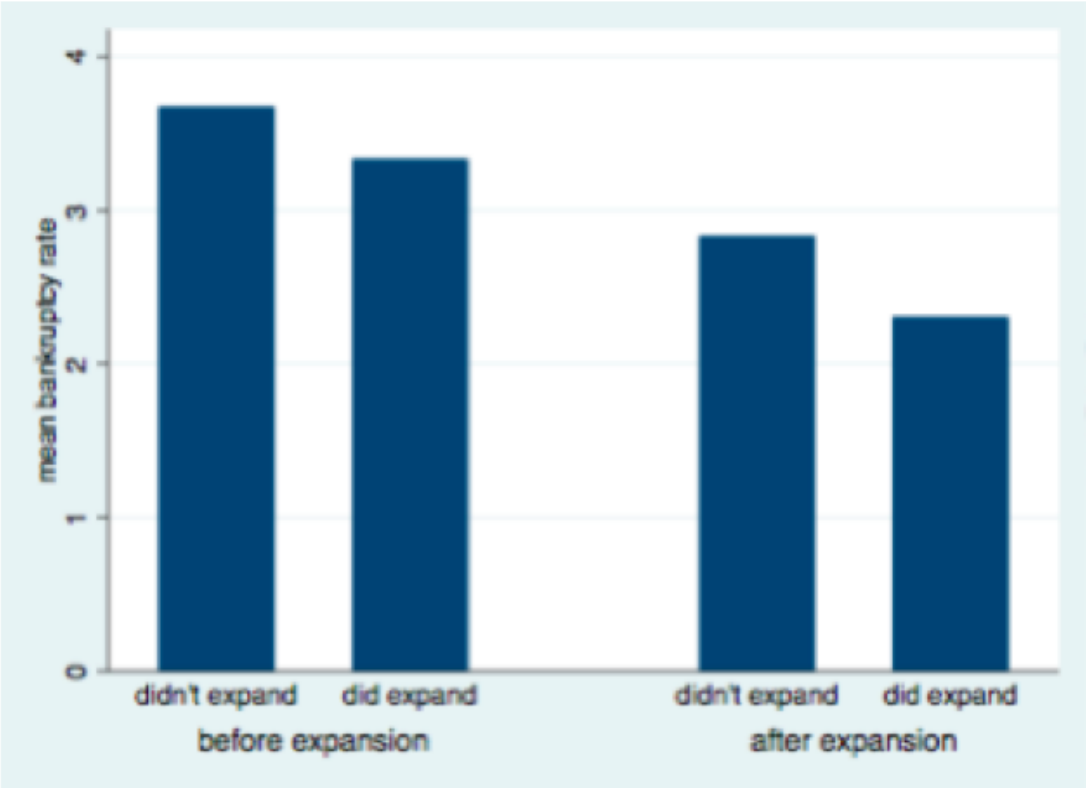

Figure 3: Mean bankruptcy rate in states that did and did not expand before and after expansion (late expansion states removed)

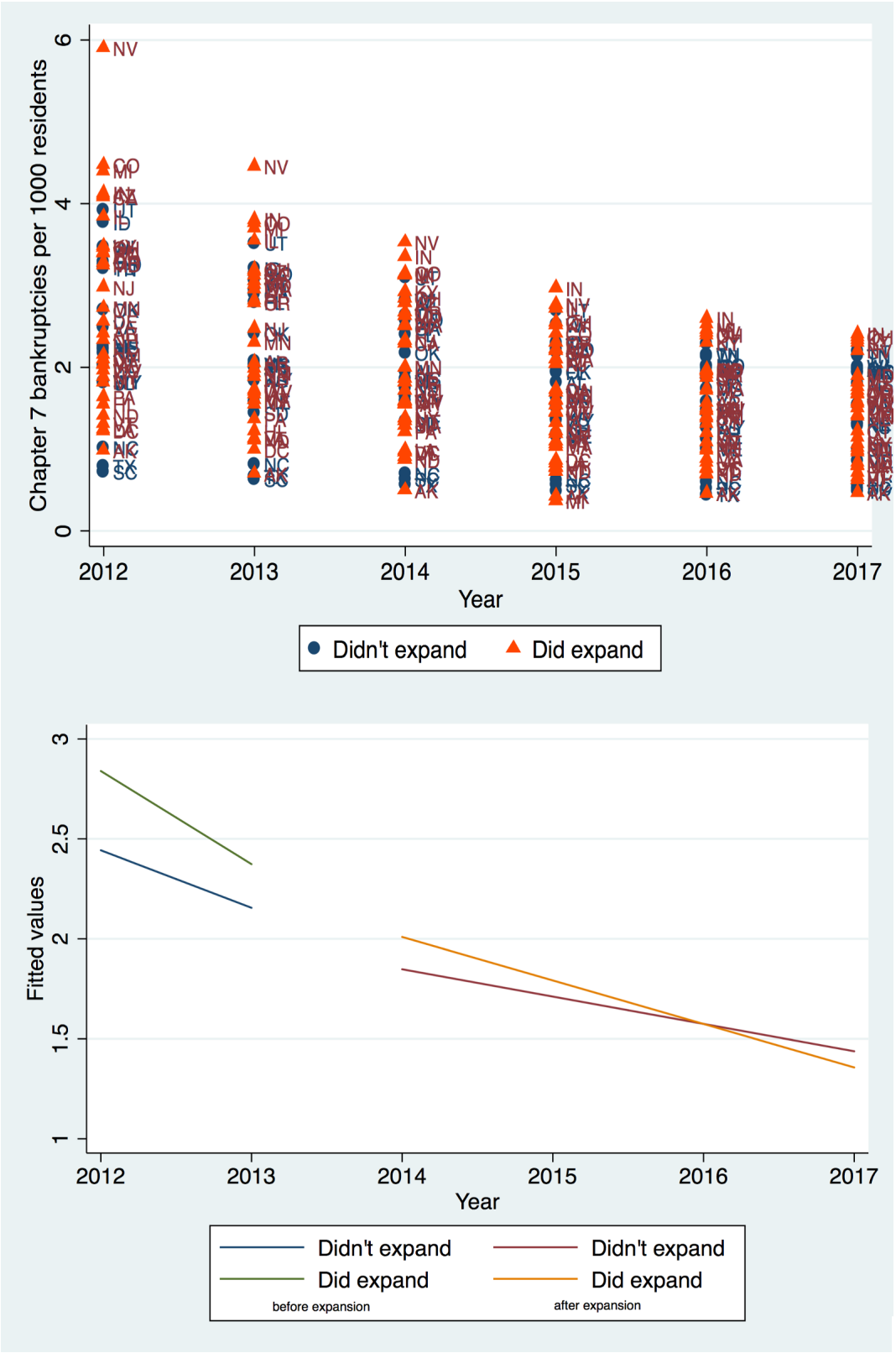

Figure 4.1: Chapter 7 Bankruptcies

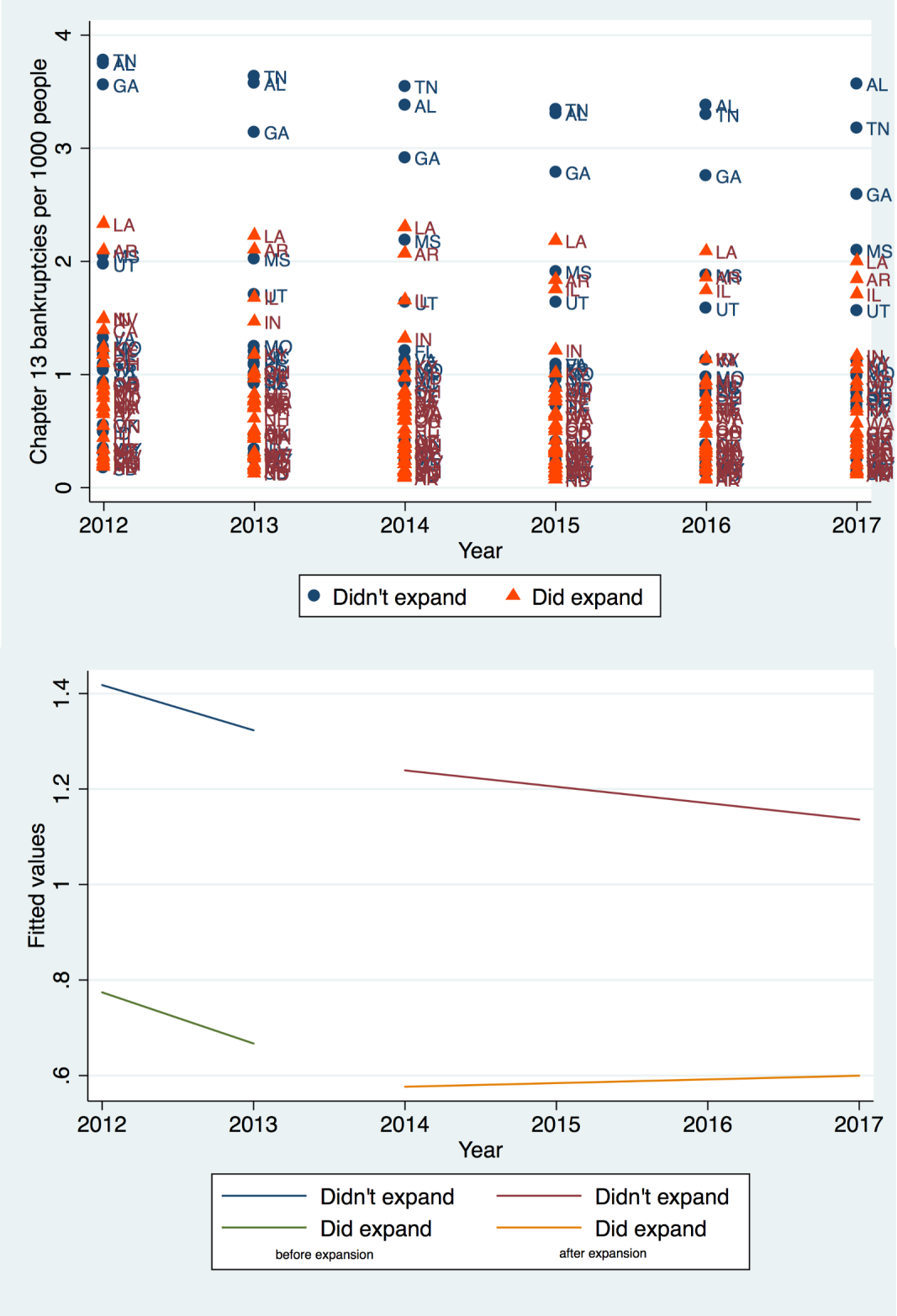

Figure 4.2: Chapter 13 Bankruptcies

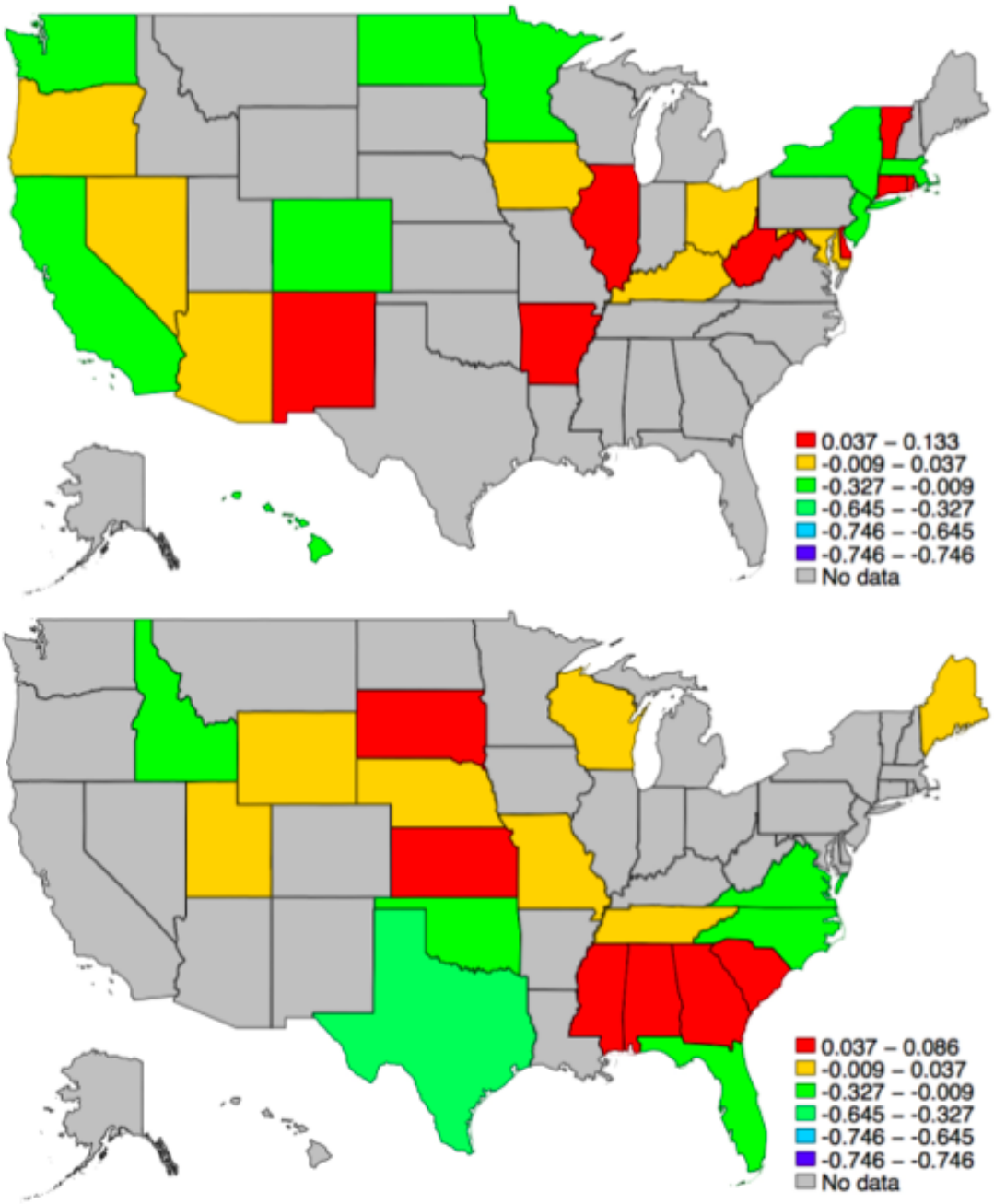

Figure 5: Predicted deviation from mean change in individual bankruptcies, per thousand residents year over year at year end 2013. Expansion states above; non-expansion states below. There does not seem to be a link between whether or not a state expanded and whether or not it positively or negatively deviated from average change in bankruptcy rate.

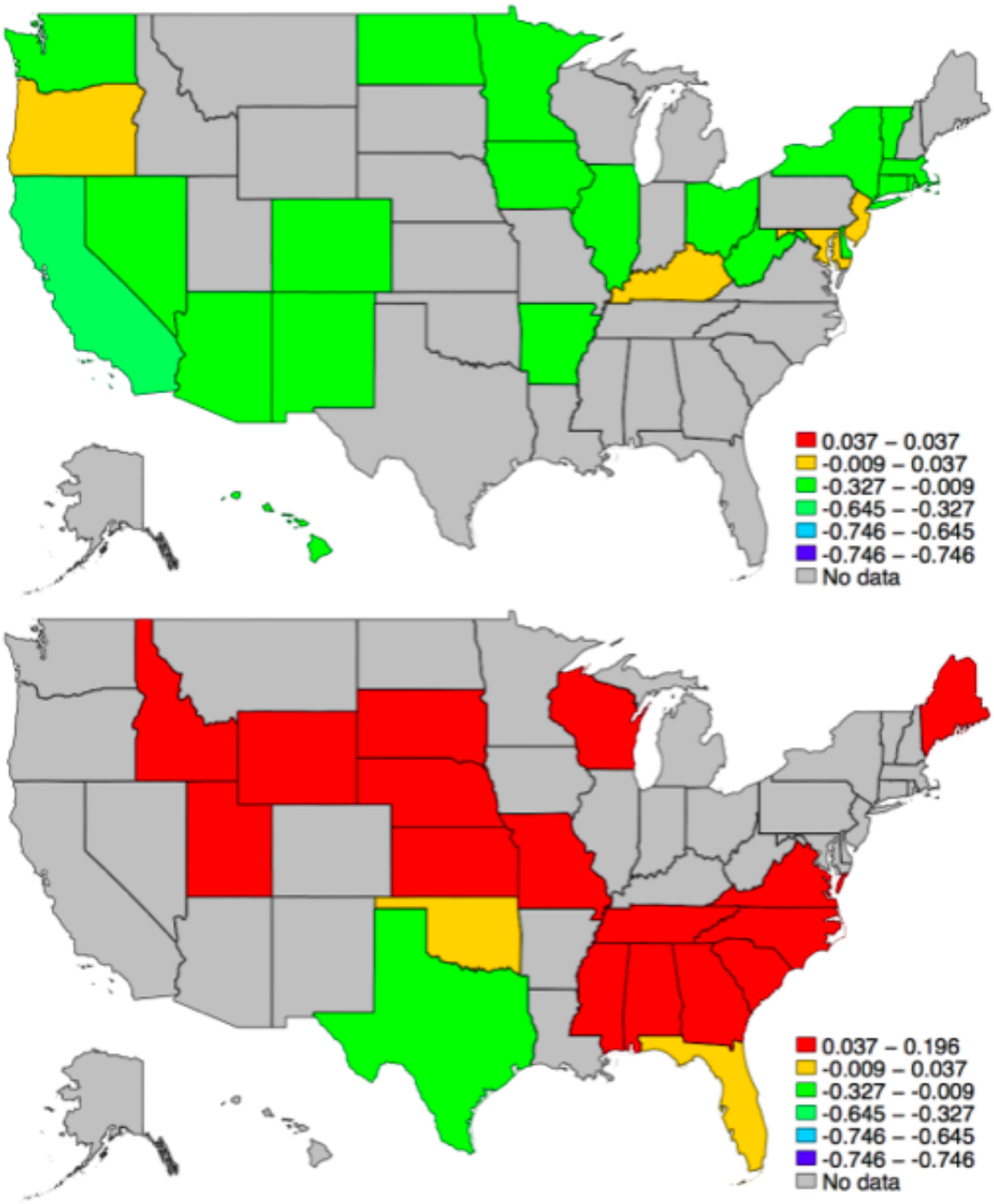

Figure 6: Predicted deviation from mean change in individual bankruptcies, per thousand residents year over year for 2014. Expansion states above; non-expansion states below. We see a much less ambiguous picture: being in states that expanded Medicaid is associated with a negative deviation from trend bankruptcies per thousand, while being in a state that did not expand is associated with a positive deviation from average change in bankruptcies.

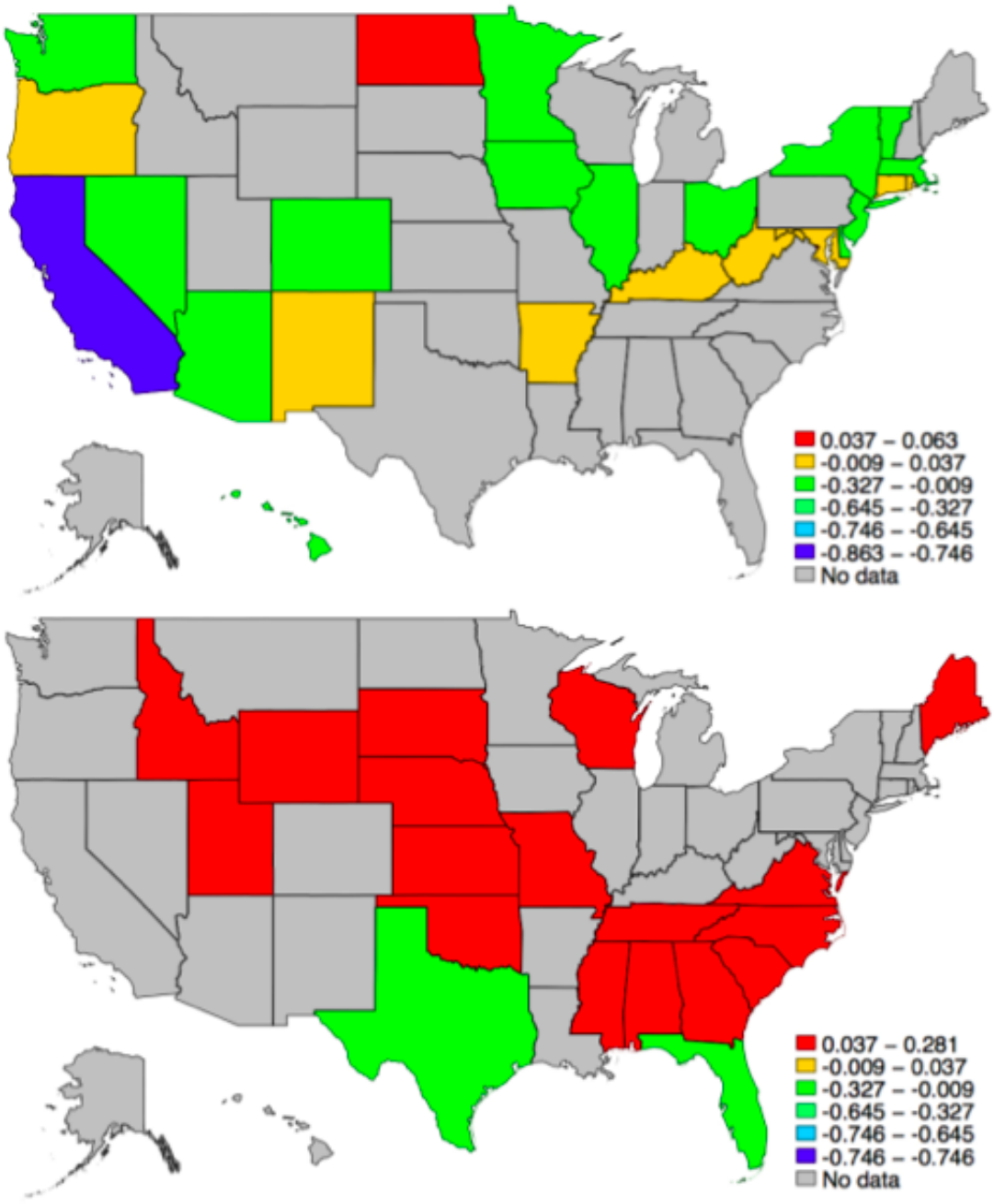

Figure 7: Summed predicted deviation from trend for individual bankruptcies per thousand, two years post expansion (2014 and 2015). Expansion states above; non-expansion states below

Table 1

Means of bankruptcy rate per 1000 residents in states that did and did not expand.

Late Expansion States Removed	Before Expansion (2012-2013)	After Expansion (2014-2016)	Percent Change
Didn't Expand	3.67	2.83	-29.6%
Did Expand	3.272	2.3	-42.3%
Late Expansion States Included	Before Expansion	After Expansion	Percent Change
Didn't Expand	3.67	2.83	-29.6%
Did Expand	3.15	2.264	-39.1%

TABLE 2

Predicting how many individual bankruptcies per thousand residents occur within a state, excluding any states that had expansion post-January 2014.

		ESTIMATES			
CONCEPT	**VARIABLE NAME**	Initial Conditions	... Plus Controls	...Plus State Fixed-Effects	...Plus Time Fixed-Effects
Base model	INTERCEPT	3.672 **(17.51)**	7.04 **(4.57)**	12.14 **(4.799)**	9.023 **(3.261)**
	Dummy for state that expanded Medicaid	-0.339 **(-1.219)** **[.112]**	0.06 **(.22)** **[.413]**	-4.835*** **(-6.617)** **[.00001]**	-4.569*** **(-6.702)** **[.00001]**
	Dummy for post-Medicaid expansion time frame	-0.840*** **(-3.27)** **[.001]**	-0.423* **(-1.43)** **[.077]**	-0.390*** **(-4.55)** **[.00001]**	-0.2 **(-0.296)** **[.284]**
	Expanded*PostExpansion	-0.188 **(-0.550)** **[.291]**	-0.085 **(-0.25)** **[.401]**	-0.104 **(-1.08)** **[.141]**	-0.148** **(-1.65)** **[.05]**
CONTROLS	Price of oil		YES	YES	YES
	Population		YES	YES	YES
	Income		YES	YES	YES
	Homeownership Rate		YES	YES	YES
	Gross State Product		YES	YES	YES
TIME EFFECTS	TIME CONTROL DUMMIES				YES
STATE EFFECTS	STATE CONTROL DUMMIES			YES	YES
	ADJ R2	0.124	0.2789	0.945	0.953
	F-STATISTIC	13.31	11.35	76.26	85.54
	SUM SQR. RESIDUALS	432.6	291.44	17.76	15.12
	Sample Size	263	245	215	215

t-stats in parenthesis: p-values calculated with 1-tailed hypothesis in brackets.

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

TABLE 3

Alternative specifications.

CONCEPT	VARIABLE NAME	ESTIMATES Original Specification	...NV excluded	...Plus Late Expansions
Base model	INTERCEPT	8.85	7.98	8.323
		(3.13)	**(3.55)**	**(3.08)**
	Dummy for being in being a state that expanded Medicaid	-4.43***	-4.21***	-4.27***
		(-6.4)	**(-7.64)**	**(-5.46)**
		[.00001]	**[.00001]**	**[.00001]**
	Dummy for being in world post-Medicaid expansion	-.2	-0.0886	0.009938
		(-0.28)	**(-0.16)**	**(.06)**
		[.3899]	**[.437]**	**[.476]**
		-0.171**	-0.0612	-.204**
	Expanded*PostExpansion	**(-1.88)**	**(-0.83)**	**(-2.01)**
		[.0307]	**[.204]**	**[.023]**
CONTROLS	Price of oil	YES	YES	YES
	Population	YES	YES	YES
	Income	YES	YES	YES
	Homeownership Rate	YES	YES	YES
	Gross State Product	YES	YES	YES
TIME EFFECTS	TIME CONTROL DUMMIES	YES	YES	YES
STATE EFFECTS	STATE CONTROL DUMMIES	YES	YES	YES
	ADJ R2	0.9504	.975	.9352
	F-STATISTIC	85.54	124.84	62.08
	SUM SQR. RESIDUALS	16.23	9.986	25.05
	Sample Size	220	210	255

t-stats in parenthesis: p-values calculated with 1-tailed hypothesis in brackets.

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

TABLE 4

Comparing Chapter 7 individual bankruptcies per thousand vs Chapter 13 individual bankruptcies per thousand.

CONCEPT	VARIABLE NAME	ESTIMATES Chapter 7	Chapter 13
Base model	INTERCEPT	4.07 **(17.51)**	4.77 **(5.4)**
	Dummy for being in being a state that expanded Medicaid	-0.647 **(-1.16)** **[.124]**	-3.78*** **(-17.46)** **[.00001]**
	Dummy for being in world post-Medicaid expansion	0.251 **(0.45)** **[.327]**	-.06 **(-.26)** **[.398]**
	Expanded*PostExpansion	-0.184*** **(-2.52)** **[.006]**	.0144 **(0.51)** **[.305]**
CONTROLS	Price of oil	YES	YES
	Population	YES	YES
	Income	YES	YES
	Homeownership Rate	YES	YES
	Gross State Product	YES	YES
TIME EFFECTS	TIME CONTROL DUMMIES	YES	YES
STATE EFFECTS	STATE CONTROL DUMMIES	YES	YES
	ADJ R2	0.9218	0.9860
	F-STATISTIC	50.63	297.39
	SUM SQR. RESIDUALS	10.51	1.43
	Sample Size	220	220

t-stats in parenthesis: p-values calculated with 1-tailed hypothesis in brackets.
*p<0.1; **p<0.05; ***p<0.01

TABLE 5

Comparing individual bankruptcies per thousand to business bankruptcies per thousand.

CONCEPT	VARIABLE NAME	ESTIMATES Individual Bankruptcies	Business Bankruptcies	Business Bankruptcies w/o DE
Base model	INTERCEPT	8.85 **(3.13)**	.652 **(2.41)**	.444 **(3.08)**
	Dummy for being in being a state that expanded Medicaid	-4.43*** **(-6.4)** **[.00001]**	-0.0703 **(-1.06)** **[.145]**	-.0734** **(-2.09)** **[.0189]**
	Dummy for being in world post-Medicaid expansion	-.2 **(-0.28)** **[.389]**	-.123** **(-1.88)** **[.031]**	-.081** **(-2.31)** **[.011]**
	Expanded*PostExpansion	-0.171** **(-1.88)** **[.031]**	0.002 **(0.22)** **[.413]**	.006 **(1.28)** **[.101]**
CONTROLS	Price of oil	YES	YES	YES
	Population	YES	YES	YES
	Income	YES	YES	YES
	Homeownership Rate	YES	YES	YES
	Gross State Product	YES	YES	YES
TIME EFFECTS	TIME CONTROL DUMMIES	YES	YES	YES
STATE EFFECTS	STATE CONTROL DUMMIES	YES	YES	YES
	ADJ R2	0.9504	0.9362	.8164
	F-STATISTIC	85.54	62.81	19.66
	SUM SQR. RESIDUALS	16.23	.148	.041
	Sample Size	220	220	215

t-stats in parenthesis: p-values calculated with 1-tailed hypothesis in brackets.

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