**Title**
The Effects of Monetary Policy, Fiscal Policy, and College Share on the U.S. Economy



**Author**
Noh, Eul

**Publication Date**
2019

Peer reviewed|Thesis/dissertation

UNIVERSITY OF CALIFORNIA SAN DIEGO

# The Effects of Monetary Policy, Fiscal Policy, and College Share on the U.S. Economy

A dissertation submitted in partial satisfaction of the

requirements for the degree

Doctor of Philosophy

in

Economics

by

Eul Noh

Committee in charge:

Professor James D. Hamilton, Chair
Professor Allan Timmermann
Professor Alexis Akira Toda
Professor Rossen Valkanov
Professor Johannes Wieland

2019

The Dissertation of Eul Noh is approved, and is acceptable in quality and form for publication on microfilm and electronically:

Chair

University of California San Diego

2019

DEDICATION

To my parents and wife

TABLE OF CONTENTS

LIST OF FIGURES

LIST OF TABLES

ACKNOWLEDGEMENTS

I would like to acknowledge Professor James D. Hamilton for his support as the chair of my committee. Through multiple drafts and many long nights, his guidance has proved to be invaluable.

Chapter 1, in full, is currently being prepared for submission for publication of the material. The dissertation author, Eul Noh, was the primary author of this chapter.

Chapter 2 is coauthored with Yifei Lyu. The dissertation author was the primary author of this chapter. And this chapter, in full, is currently being prepared for submission for publication of the material.

Chapter 3 is coauthored with Jung Hyun Choi and Richard K. Green. The dissertation author was the primary author of this chapter. And this chapter, in full, is currently being prepared for submission for publication of the material.

## VITA

| | |
|---|---|
| 2010 | B. A. in Economics, Korea University, Republic of Korea |
| 2010 | B. S. in Mathematics (double major), Korea University, Republic of Korea |
| 2012 | M. A. in Economics, Korea University, Republic of Korea |
| 2014-2019 | Graduate Teaching Assistant, University of California, San Diego |
| 2019 | Ph. D. in Economics, University of California, San Diego |

ABSTRACT OF THE DISSERTATION

**The Effects of Monetary Policy, Fiscal Policy, and College Share on the U.S. Economy**

by

Eul Noh

Doctor of Philosophy in Economics

University of California San Diego, 2019

Professor James D. Hamilton, Chair

Chapter 1 suggests an efficient and simple regression-based approach for consistent estimation of dynamic effects of structural shocks in vector autoregressions (VAR) with proxy variables for the shocks. First, we show that the existing Proxy Structural VAR (Proxy-SVAR) approach using the proxy as an instrument variable yields a consistent estimator of the shape of the impulse-response function (IRF) if and only if the proxy does not have any direct forecasting ability in the VAR. Second, we prove that in the linear model, the shape of the IRF can be consistently estimated by

adding the current and past values of the proxy variable in the VAR regardless of its direct forecasting ability or measurement error. Third, we show both theoretically and empirically that the formulation in Gertler and Karadi (2015) misestimates the effect of a monetary policy shock. Applying our unrestricted approach to GK's specification results in a substantially different conclusion from the Proxy-SVAR.

In Chapter 2, We build a Markov-switching structural VAR to estimate state-dependent government spending multipliers in the U.S. We show that the multipliers are statistically larger during recessions than during expansions. Our model has two features. First, we combine quantitative data and qualitative indicators to infer the regimes of the economy across which the multipliers differ. Second, we propose a recursive method to estimate IRFs that allows the economy to switch regimes after the shock. We argue that these two features are important for reconciling the main findings in previous studies.

Chapter 3 extends the Rosen-Roback spatial equilibrium model to show that increasing city-level college share affects welfare distribution by changing both wages and housing costs across individuals with different education levels. Using the PSID from 1980 to 2013, we confirm that high skilled workers gain greater benefits from living in cities with a rising college share, as the increase in their wage premiums outweighs their rent growth. However, earnings increase of the unskilled are completely offset by higher housing rents. In response influxes of college graduates, housing wealth also increases significantly more for college graduates, further widening the welfare gap.

# Chapter 1

# Impulses Response Analysis with Proxy Variables

## 1.1 Introduction

Estimating the effect of a structural shock on the economy is one of the key objectives of many economic studies. The most challenging task that a researcher faces is identifying the shock itself, which is supposed to be unpredictable by the market participants and caused by the exact economic force of interest. Stock and Watson (2008) and Mertens and Ravn (2013) suggest an identification strategy that employs a proxy variable for the shock in the structural vector autoregression (SVAR) framework. Known as the Proxy-SVAR, this approach uses the proxy as an instrument variable (IV) to identify the shock without leaning on further constraints on the second moment of the reduced-form errors unlike conventional SVAR. The contemporaneous effects of the shock are estimated via a two-step method that regresses the proxy on the estimated reduced-form residuals, and then the effects afterward are computed additionally with the slope coefficients in the reduced-form VAR. This method has been adopted in dozens of recent studies including Stock and Watson (2012), Mertens and Ravn (2014), Gertler and Karadi (2015), Carriero et al. (2015) and Mumtaz et al. (2018).

However, like conventional SVAR, the Proxy-SVAR relies on the strong assumption that the structural shock of interest is a linear combination of the current values of the reduced-form residuals. Hansen and Sargent (1991) note that this assumption does not hold if the VAR omits a relevant variable that the economic agent observes in the underlying structural model but the econometrician cannot. Using an example of the anticipated effect of tax news, Leeper et al. (2013) illustrate that even if the VAR includes all endogenous variables of the underlying theoretical model, it may still induce a substantial bias in the impulse-response function (IRF) due to the gap between the information set of the agent and the econometrician. Recently, Stock and Watson (2018) also point out that a failure of the invertibility condition can result in an inconsistent IRF of the Proxy-SVAR approach.

This paper makes three contributions to the literature. First, we show that given a serially uncorrelated proxy variable correlated only with current structural shock of interest, the Proxy-SVAR approach provides a consistent estimator of IRF up to its scale if and only if the proxy does not directly forecast the other variables in the VAR. This result is closely related to Forni and Gambetti (2014), who verify that the invertibility condition holds only if there exists no state variable with an extra forecasting ability if added in the VAR. We take one further step from their conclusion by demonstrating that we do not need any other state variable to test the validity of the Proxy-SVAR approach given a proxy variable for the shock of interest.[1] After removing possible correlation of the proxy variable with the past values of the endogenous variables or itself, one can test if the IRF from the Proxy-SVAR is valid by simply adding the lagged values of the proxy to the forecasting equations of the reduced-form VAR and testing whether the coefficients are all zero. Although this test does not guarantee the recoverability of all structural shocks, one can still estimate the shape of the desired IRF consistently through the Proxy-SVAR approach if the null hypothesis of the test is true.

Second, we demonstrate that regardless of whether or not the invertibility condition holds, the IRF can be estimated up to scale by controlling for current and past values of the proxy variable

[1] Plagborg-Mller and Wolf (2018) independently developed this same result

in the VAR. We prove that the serially uncorrelated measurement error in the proxy only affects the scale of the shock's impact but does not distort the dynamic shape of the IRF. Therefore, under control of scale of the shock, the IRF is consistently estimated with the augmented VAR, and the existing Proxy-SVAR is a special case that restricts the direct effects of the past shocks to be zero due to the invertibility condition. If the invertibility condition fails, a forecasting error from a reduced-form VAR depends on the past values of the structural shock. For this reason, treating the reduced-form error as a linear combination of current structural shocks misses the direct effect of past structural shocks not through the persistence of the endogenous variables. We also show that if there are missing observations of the proxy variable, the IRF can be estimated with the full length of the data simply by setting the missing observations to be zero. Since our approach employs all available information from data, it is more efficient than other regression-based approaches including local-projection with instrument variable (LP-IV, Jord, 2005), where the endogenous variables are directly projected on the past values of the proxy variable. Our approach can be extended to the case of multiple proxy variables for multiple structural shocks with the proper number of additional restrictions on the model parameters.

Third, we provide empirical evidence and theoretical reasons why the effect of a monetary policy shock cannot be estimated with existing Proxy-SVAR. Using a theoretical macro model, we analytically show that even when the number of variables in the VAR is equal to the number of true structural shocks, the VAR consisting of the endogenous variables in the standard New Keynesian model does not satisfy the invertibility condition if monetary news includes forward guidance of future monetary policy. As Leeper et al. (2013) demonstrate with a classical growth model and tax news, the econometrician's VAR cannot recover the structural shocks and the IRF is mis-estimated due to omitted information about the timing and the size of the anticipated effect from the news shock.

We test if the proxy variables for the monetary shocks used in Gertler and Karadi (2015) have additional predictive power in their VAR. We find strong evidence of the forecasting ability of the proxy, rejecting the invertibility assumption and the validity of the Proxy-SVAR approach in GK's specification. Since the proxy variable contains information on forward-guidance news, this test result is consistent with the implication of our analytical consideration of this model. Applying our unrestricted approach to GK's model results in substantially different IRFs from their estimates: the response of the short-term and long-term interest rate to monetary news is estimated to be significantly larger and more persistent. The effects on output and the consumer price index become larger in the short-term, but insignificant in the medium term, in contrast to GK's result. Relaxing the restriction also changes the sign and the magnitude of the effects on the credit market significantly, implying decreasing borrowing cost due to an unexpected monetary tightening.

Stock and Watson (2018) suggested testing the invertibility condition by comparing the IRF computed from the Proxy-SVAR and LP-IV using a Hausman (1978) type test statistics. An advantage of their approach is that it is valid when the other assumptions implicit in the VAR fail to hold. Advantages of our approach are that it is much easier to implement and has higher power since the models both under the null and alternative hypothesis are estimated with all available information from the data. Stock and Watson (2018) test the stationary version of GK's VAR and the proxy variable we use and do not reject the null hypothesis of the Proxy-SVAR specification. On the other hand, our test strongly rejects the invertibility condition and the IRFs heavily depend on this assumption.

This paper is organized as follows: Section 1.2 discusses the necessary and sufficient conditions for the validity of the Proxy-SVAR approach. Section 1.3 demonstrates that existing Proxy-SVAR mis-estimates the monetary shock effect with a theoretical model and empirical evidence. Section 1.4 shows that given a whitened proxy variable independent to the uninterested shocks, adding the current and the lagged values it in the VAR yields consistent estimator of the IRF up to scale even if the proxy has a measurement error. Section 1.5 concludes.

## 1.2 Conditions for validity of the Proxy-SVAR approach

This section presents the necessary and sufficient conditions for the validity of the Proxy-SVAR approach. Suppose we have an instrument variable (IV) or a proxy variable that is $(i)$ correlated with the current structural shock of interest, $(ii)$ serially uncorrelated, but $(iii)$ uncorrelated with the other structural shocks and the future/past structural shock of interest. We show that if and only if the proxy variable does not have any extra forecasting ability in the VAR, the Proxy-SVAR approach provides a consistent estimator of the IRF up to scale. As pointed out by Forni and Gambetti (2014), the restriction on the predictive power of the proxy stems from the invertibility condition that assumes the structural shock to be a linear combination of the reduced-form errors.

### 1.2.1 Estimation model

In this paper, we focus on linear models. Let $\mathbf{u}_t = (u_{1,t}, u_{2,t} \cdots, u_{m,t})'$ denote the vector of $m$ structural shocks. For each $i$ , $j$, $s$ and $t$, $u_{i,t}$ is a white noise with unit variance satisfying $E(u_{i,t}|u_{j,s}) = 0$ unless $i = j$ and $t = s$. The aim of the impulse-response analysis with $n$ observable variables $\mathbf{y}_t = (y_{1,t}, y_{2,t} \cdots, y_{n,t})'$ for $t = 1, 2, \cdots, T$ is to estimate the change in the expected value of $\mathbf{y}_{t+h}$ in response to a structural shock in period $t$. Without loss of generality, we focus only on the effect of the first $k$ structural shocks $\mathbf{u}_{1,t} = (u_{1,t}, u_{2,t}, \cdots, u_{k,t})'$. Then the impulse-response function (IRF) of interest is the the $m \times k$ matrix $\Theta_{1,j}$ for each $j$ in the following the MA($\infty$) representation of $\mathbf{y}_t$:

$$\mathbf{y}_t = \sum_{h=0}^{\infty} \Theta_{1,h} \mathbf{u}_{1,t-h} + \sum_{h=0}^{\infty} \Theta_{2,h} \mathbf{u}_{2,t-h}. \tag{1.1}$$

Specifically, the expected response of $\mathbf{y}_{t+h}$ to $u_{i,t}$ is

$$\theta_{1i,h} \equiv E(\mathbf{y}_{t+h}|u_{i,t} = 1, \mathbf{Y}_{t-1}) - E(\mathbf{y}_{t+h}|u_{i,t} = 0, \mathbf{Y}_{t-1}) \tag{1.2}$$

for $i = 1, 2, \cdots, k$ and $h = 0, 1, \cdots, H$, where $\mathbf{Y}_{t-1} = \{\mathbf{y}_{t-1}, \mathbf{y}_{t-2}, \cdots, \mathbf{y}_1\}$ and $\theta_{1i,h}$ denotes $i$th column of $\Theta_{1,h}$. The $l$th element of $\theta_{1i,h}$ is the expected response of $y_{l,t+h}$ to the structural shock $u_{i,t}$. If $\mathbf{u}_{1,t}$ was perfectly observable, one can simply estimate $\Theta_{1,0}, \Theta_{1,1}, \cdots, \Theta_{1,H}$ simply by regressing $\mathbf{u}_t, \mathbf{u}_{t-1}, \cdots, \mathbf{u}_{t-H}$ on $\mathbf{y}_t$. Instead of $\mathbf{u}_{1,t}$, we observe $k$ imperfect measures of the shocks of interest $\mathbf{z}_t = (z_{1,t}, z_{2,t}, \cdots, z_{k,t})'$ with scale bias and measurement error as below:

$$\mathbf{z}_t = \Gamma \mathbf{u}_{1,t} + \eta_t, \tag{1.3}$$

where $\Gamma$ is $k \times k$ nonsingular matrix and $\eta_t$ is $k \times 1$ vector satisfying

$$E(\eta_t | \eta_{t-j}) = 0_k, \forall j \neq 0, \text{ and } E(\mathbf{u}_t | \eta_s) = 0_{k \times k}, \forall s \tag{1.4}$$

(1.3) can be extended to allow nonzero mean and correlation of $\mathbf{z}_t$ with lagged values of itself or structural shocks by assuming that such correlation can be eliminated by controlling lagged values of $\mathbf{z}_t$ and $\mathbf{y}_t$. Specifically, we assume

$$\mathbf{z}_t = \bar{\mathbf{z}} + \sum_{j=1}^{p_z} \Lambda_{z,j} \mathbf{z}_{t-j} + \sum_{j=1}^{p_y} \Lambda_{y,j} \mathbf{y}_{t-j} + \Gamma \mathbf{u}_{1,t} + \eta_t \tag{1.5}$$

and redefine the residual of (1.5) as the proxy.

### 1.2.2 A Review of the Proxy-SVAR approach

#### Assumptions on reduced-form errors and structural shocks

Since the shocks are unobservable, SVAR approaches including the Proxy-SVAR identify each structural shock and its effects from a reduced-form VAR

$$\mathbf{y}_t = \mathbf{c} + \sum_{j=1}^{p} \Phi_j \mathbf{y}_{t-j} + \varepsilon_t, \tag{1.6}$$

under the assumptions that

$$\text{there exists an } n \times m \text{ matrix } \mathbf{H} \text{ with rank } n \text{ satisfying } \varepsilon_t = \mathbf{H}\mathbf{u}_t, \text{ and} \tag{1.7}$$

$$\dim(\mathbf{u}_t) = \dim(\varepsilon_t), \text{ i.e., } m = n. \tag{1.8}$$

In the literature (1.7) and (1.8) are jointly called the invertibility condition of VAR. Under these assumptions, each structural shock can be recovered from a linear combination of the reduced-from errors in $\varepsilon_t$ given the value of $\mathbf{H}$.

Given the values of $\{\Phi_j\}_{j=1}^{p}$ and $\mathbf{H}$, $\theta_{1i,h}$ in (1.2) is the $i$th column of $\Psi_h\mathbf{H}$, where $\Psi_h$ is the first $n \times n$ submatrix of $\mathbf{F}^h$ with $\mathbf{F}$ is the matrix of the slope coefficients in the VAR.[2] From the reduced-form errors, one can only estimate $\frac{n(n+1)}{2}$ parameters with the symmetric matrix $\mathbf{H}\mathbf{H}' = E(\varepsilon_t\varepsilon_t')$, while $\mathbf{H}$ has $n^2$ elements. In the Proxy-SVAR framework, the additional constraints on $\mathbf{H}$ are derived from IVs satisfying certain conditions presented below. Combined with (1.7) and (1.8), the extra constraints generated by the IVs allow the identification of $\mathbf{H}$ and the IRF of interest.

**Shock identification with proxy variables**

Since we are interested in the first $k$ structural shocks, the IRF discussed above can be computed if the $n \times k$ matrix $\mathbf{H}_1$ consisting of the first $k$ columns of $\mathbf{H}$ is known. Note that the proxy

[2] Specifically, $\mathbf{F}$ is defined as follows:

$$\mathbf{F} = \begin{bmatrix} \Phi_1 & \Phi_2 & \cdots & \Phi_{p-1} & \Phi_p \\ \mathbf{I}_n & \mathbf{0}_{n\times n} & \cdots & \mathbf{0}_{n\times n} & \mathbf{0}_{n\times n} \\ \vdots & \vdots & \ddots & \vdots & \vdots \\ \mathbf{0}_{n\times n} & \mathbf{0}_{n\times n} & \cdots & \mathbf{I}_n & \mathbf{0}_{n\times n} \end{bmatrix}. \tag{1.9}$$

variable $\mathbf{z}_t$ satisfies[3]

$$E(\mathbf{z}_t\mathbf{u}_{1,t}^{'}) = \Gamma, \text{ and} \tag{1.10}$$

$$E(\mathbf{z}_t\mathbf{u}_{2,t}^{'}) = \mathbf{0}_{k\times(n-k)} \tag{1.11}$$

The previous studies (Mertens and Ravn, 2013, Stock and Watson, 2008) show that under (1.7), (1.8), (1.10), and (1.11), $\mathbf{H}_1$ can be identified with $\frac{k(k-1)}{2}$ additional restrictions on the model parameters. In case of $k = 1$, there are no such additional restrictions, and the shock is identified without any constraint on $\mathbf{H}$ besides the presumed sign of $E(z_t u_{1,t})$. As in the conventional IV regressions, the relevance condition (1.10) requires the instrument (i.e., the proxy variable) to be correlated with the target object that we want to identify, while the exogeneity condition (1.11) requires it to be uncorrelated with the other shocks.

If the relevance and exogeneity conditions hold, they impose additional constrains on the second moment of the reduced-form residuals and make identification possible. To see how, let $\varepsilon_{1,t}$ denote the vector of the first $k$ reduced-form errors and the $n-k$ vector $\varepsilon_{2,t}$ of the remains. Consider a partition of

$$\mathbf{H} = \begin{bmatrix} \underset{(n\times k)}{\mathbf{H}_1} & \underset{(n\times(n-k))}{\mathbf{H}_2} \end{bmatrix} = \begin{bmatrix} \underset{(k\times k)}{\mathbf{H}_{11}} & \underset{(k\times(n-k))}{\mathbf{H}_{12}} \\ \underset{((n-k)\times k)}{\mathbf{H}_{21}} & \underset{((n-k)\times(n-k))}{\mathbf{H}_{22}} \end{bmatrix},$$

where the order of $y_{1,t}, y_{2,t}, \cdots, y_{n,t}$ ensures that $\mathbf{H}_{11}$ is nonsingular.[4] Then for each $i = 1, 2$, we have

[3]The conditions (1.10) and (1.11) do not restrict correlation of $\mathbf{z}_t$ with the past values of the structural shocks. However, under the invertibility condition, such correlation can be controlled with the past values of $\mathbf{y}_t$ as assumed in (1.5) and possible autocorrelation of $\eta_t$ can be further controlled by lagged values of $\mathbf{z}_t$. We assume (1.5) and (1.4) even without the invertibility condition. If this nontestable assumption does not hold, it is impossible to rule out the possibility that the change of $\mathbf{z}_t$ is due to the past structural shocks even under control of the past values of itself and the variables in the VAR. As discussed in Stock and Watson (2018), then (in case of $k = 1$) the shape of the IRF is estimated inconsistently even if $\mathbf{y}_{t+h}$ is directly projected on $\mathbf{z}_t$ under control of lagged $\mathbf{z}_t$ and $\mathbf{y}_t$ as in local-projection with IV (LP-IV) approach.

[4]For example, given a proxy variable of a monetary policy shock ($k = 1$), the most reasonable choice for the first variable in the VAR is the interest rate.

$\varepsilon_{i,t} = \mathbf{H}_{i1}\mathbf{u}_{1,t} + \mathbf{H}_{i2}\mathbf{u}_{2,t}$ from the invertibility condition (1.7), followed by

$$\begin{aligned} E(\mathbf{z}_t\varepsilon_{i,t}^{'}) &= E(\mathbf{z}_t\mathbf{u}_{1,t}^{'})\mathbf{H}_{i1}^{'} + E(\mathbf{z}_t\mathbf{u}_{2,t}^{'})\mathbf{H}_{i,2} \\ &= \Gamma\mathbf{H}_{i1}^{'} \end{aligned}$$

for $i = 1,2$, since $E(\mathbf{z}_t\mathbf{u}_{1,t}^{'}) = \Gamma$ from (1.10) and $E(\mathbf{z}_t\mathbf{u}_{2,t}^{'}) = \mathbf{0}_{k\times(n-k)}$ from (1.11). By pre-multiplying $E(\mathbf{z}_t\varepsilon_{1,t}^{'})^{-1} = \mathbf{H}_{11}^{'-1}\Gamma^{-1}$ to $E(\mathbf{z}_t\varepsilon_{2,t}^{'})^{-1} = \Gamma\mathbf{H}_{21}^{'}$, we have

$$\mathbf{H}_{11}^{'-1}\mathbf{H}_{21}^{'} = E(\mathbf{z}_t\varepsilon_{1,t}^{'})^{-1}E(\mathbf{z}_t\varepsilon_{2,t}^{'}). \tag{1.12}$$

$E(\mathbf{z}_t\varepsilon_{1,t}^{'})$ and $E(\mathbf{z}_t\varepsilon_{2,t}^{'})$ can be estimated with the observable instrument or proxy variable and the estimated reduced-form error from (1.6), and thus (1.12) works as the additional constraints on the elements in the variance-covariance matrix of $\varepsilon_t$. Combined with (1.7) and (1.8), (1.12) enables the identification of $\mathbf{H}_1$.[5]

### 1.2.3 Sufficient and necessary conditions for the validity of the Proxy-SVAR

In this subsection, we show that the sufficient and necessary conditions for the validity of the Proxy-SVAR approach is the absence of the direct predictive power of the $\mathbf{z}_t$ in the VAR. Given a vector of proxy variables $\mathbf{z}_t$ satisfying (1.3) and (1.4) we can write $\mathbf{u}_{1,t}$ as

$$\mathbf{u}_{1,t} = \Pi\mathbf{z}_t + \mathbf{w}_t \tag{1.13}$$

where $\Pi = E(\mathbf{u}_{1,t}\mathbf{z}_t^{'})E(\mathbf{z}_t\mathbf{z}_t^{'})^{-1} = \Gamma^{'}\Sigma_{zz}^{-1}$ with $\Sigma_{zz} = E(\mathbf{z}_t\mathbf{z}_t^{'})$. Note that $E(\mathbf{z}_t\mathbf{w}_s^{'}) = \mathbf{0}_{k\times k}$ for all $s$: since $\mathbf{w}_t$ is the regression error from projecting $\mathbf{z}_t$ on $\mathbf{u}_{1,t}$, $\mathbf{z}_t$ is uncorrelated with $\mathbf{w}_t$. For $t \neq s$, $E(\mathbf{z}_t\mathbf{w}_s^{'}) = E(\mathbf{z}_t\mathbf{u}_{1,s}^{'}) - E(\mathbf{z}_t\mathbf{z}_s^{'})\Pi = \mathbf{0}_{k\times k}$, since $\mathbf{z}_t$ is serially uncorrelated.

In order to prove the main proposition, we use following lemma:

[5]In Appendix, we present further detail in case of $k = 1$. In can be shown that the vector of regression coefficients obtained by projecting $z_t$ on each elements in $\varepsilon_t$ is proportional to the first column of $\mathbf{H}$.

**Lemma 1.** *Let* $\mathbf{H}_1^* = \mathbf{H}_1\mathbf{D}_\Gamma$ *and* $\Gamma^* = \mathbf{D}_\Gamma^{-1}\Gamma$, *where* $\mathbf{D}_\Gamma = diag(\gamma_{11}, \gamma_{22}, \cdots, \gamma_{kk})$ *and* $\gamma_{ii}$ *denotes the ith diagonal element of* $\Gamma$. *With* $k(k-1)$ *restrictions on* $\Gamma^*$ *or* $\mathbf{H}_1^*$, *The IRF to* $\mathbf{u}_{1,t}$ *computed with the Proxy-SVAR approach is consistent up to scale if and only if the reduced-form error* $\varepsilon_t = \mathbf{y}_t - E(\mathbf{y}_t|\mathbf{Y}_{t-1})$ *satisfies*

$$E(\varepsilon_t \mathbf{u}_{1,s}^{'}) = \begin{cases} \mathbf{H}_1 & if\, t = s \\ \mathbf{0}_{n\times k} & otherwise \end{cases} \tag{1.14}$$

*Proof.* (1.14) can be rewritten as $\varepsilon_t = \mathbf{H}_1\mathbf{u}_{1,t} + \mathbf{v}_t$ with $E(\mathbf{u}_{1,t}\mathbf{v}_s^{'}) = \mathbf{0}_{k\times n}$ for all $s$, which is equivalent to $\varepsilon_t = \mathbf{H}_1\Pi\mathbf{z}_t + \mathbf{H}_1\mathbf{w}_t + \mathbf{v}_t$. Since $\mathbf{z}_t$ is uncorrelated with $\mathbf{w}_t$ and $\mathbf{v}_t$, projecting $\hat{\varepsilon}_t$ on $\mathbf{z}_t$ yields consistent estimator of $\mathbf{H}_1\Pi$. Since $\Pi = \Gamma^{'}\Sigma_{zz}^{-1}$, $\mathbf{H}_1\Pi = \mathbf{H}_1^*\Gamma^{*'}\Sigma_{zz}^{-1}$. By construction, all diagonal elements of $\Gamma^*$ are one and $\hat{\Sigma}_{zz} = T^{-1}\sum_{t=1}^{T}\mathbf{z}_t\mathbf{z}_t^{'}$ , $\mathbf{H}_1^*$ and $\Gamma^*$ can be identified with $k(k-1)$ additional restrictions on $\Gamma$ or $\mathbf{H}_1$ given $\widehat{\mathbf{H}_1\Pi}$. Let $\mathbf{h}_{1,i}$ and $\mathbf{h}_{1,i}^*$ denote the $i$th column of $\mathbf{H}_1$ and $\mathbf{H}_1^*$, respectively. Since $\mathbf{h}_{1,i} = \gamma_{ii}\mathbf{h}_{1,i}^*$ for $i = 1, 2, \cdots k$, the IRF to $u_{1,it}$ is proportional to the IRF computed with $\mathbf{h}_{1,i}^*$ in the place of $\mathbf{h}_1$.

Now suppose (1.14) does not hold, i.e, $E(\varepsilon_{t+h}\mathbf{u}_{1,t}^{'}) \neq \mathbf{0}_{n\times k}$. Then we can write $\varepsilon_t = \sum_{j=0}^{\infty}\Xi_j\mathbf{u}_{1,t-j} + \mathbf{v}_{t,}$, where the $n\times k$ matrix $\Xi_j$ is nonzero for some $j$ and $E(\mathbf{u}_{1,t}\mathbf{v}_s^{'}) = 0_{k\times n}$ for all $s$. Without loss of generality, focus on the effect of $u_{1,t}$ and assume the first column of $\Xi_j$ is a nonzero vector for some $j$. Consider the Wold representation of $\mathbf{y}_t$, where $\mathbf{y}_t$ is written as the linear combination of current and past value of $\varepsilon_t = \mathbf{y}_t - E(\mathbf{y}_t|\mathbf{Y}_{t-1})$ as $\mathbf{y}_t = \mu_y + \sum_{j=1}^{\infty}\Psi_j\varepsilon_{t-j}$. From (1.3) and (1.4), we have $E(\varepsilon_t\mathbf{z}_{t-j}^{'}) = \Xi_j\Gamma^{'}$for $j \geq 0$, and thus the contemporaneous effect of $u_{1i,t}$ on $\mathbf{y}_t$ and $\varepsilon_t$ is $i$th column of $\Xi_0 = E(\varepsilon_t\mathbf{z}_t^{'})\Gamma^{'-1}$. Let $\xi_{i,j}$ denote $i$th column of $\Xi_j$.Under the invertibility condition, the IRF of interest in the Proxy-SVAR framework is

$$\begin{aligned}
&E(\mathbf{y}_{t+h}|u_{1i,t}=1,\mathbf{Y}_{t-1})-E(\mathbf{y}_{t+h}|u_{1i,t}=0,\mathbf{Y}_{t-1}) \\
=&\sum_{j=0}^{\infty}\Psi_j\{E(\varepsilon_{t+h-j}|u_{1i,t}=1)-E(\varepsilon_{t+h-j}|u_{1i,t}=0)\} \\
=&\Psi_h\{E(\varepsilon_t|u_{1i,t}=1)-E(\varepsilon_t|u_{1i,t}=0)\} \\
=&\Psi_h\xi_{i,0},
\end{aligned} \tag{1.15}$$

where the second equality is from the invertibility. However, the true IRF is

$$E(\mathbf{y}_{t+h}|u_{1i,t}=1,\mathbf{u}_{t-1},\mathbf{u}_{t-2},\cdots)-E(\mathbf{y}_{t+h}|u_{1i,t}=0,\mathbf{u}_{t-1},\mathbf{u}_{t-2},\cdots)$$

which is not equal to the IRF in (1.15) because $\xi_{i,h-j}\neq\mathbf{0}_n$ in general. □

Note that (1.14) requires partial invertibility, where the correlation of $\varepsilon_t$ and lagged values of $\mathbf{u}_{2,t}$ is not restricted to be zero. Under the full invertibility condition (1.7) and (1.8), we need $\frac{k(k-1)}{2}$ restrictions on the model parameters[6] instead of $k(k-1)$. The gap between the number of required restriction is from the the additional assumption in the full invertibility condition that $\dim(\varepsilon_t)=\dim(\mathbf{u}_t)$.

Now we have following proposition for the validity of the Proxy-SVAR approach:

**Proposition 1.1.** *Consider following system of regression equations:*

$$\mathbf{y}_t=\mu+\sum_{j=1}^{p}\Phi_j\mathbf{y}_{t-j}+\sum_{j=0}^{q}\mathbf{B}_j\mathbf{z}_{t-j}+\mathbf{e}_t, \tag{1.16}$$

*where $\mathbf{z}_t$ satisfies (1.3), (1.4), and $E(\mathbf{e}_t|\mathbf{Y}_{t-1},\mathbf{Z}_{t-1})=\mathbf{0}_n$. With $k(k-1)$ additional restrictions on $\mathbf{H}_1$ or $\Gamma$, the Proxy-SVAR provides consistent estimator of the IRF to $\mathbf{u}_{1,t}$ up to scale if and only if $\mathbf{B}_j=\mathbf{0}_{n\times k}$ for all $j>0$.*

[6]See Mertens and Ravn (2013) for further detail for the Proxy-SVAR with multiple IV. They identify two different types of tax shock with two proxies for them.

*Proof.* Suppose $\mathbf{B}_j = \mathbf{0}_n$ for all $j > 0$. Since $\mathbf{z}_t$ is uncorrelated with $\mathbf{y}_{t-1}, \mathbf{y}_{t-2}, \cdots, \mathbf{y}_{t-p}$, we have $\varepsilon_t = \mathbf{B}_0\mathbf{z}_{t-1} + \mathbf{e}_t$. From Lemma 1, the IRF computed with the Proxy-SVAR approach is consistent up to scale with $k(k-1)$ restrictions on $\Gamma$ or $\mathbf{H}_1$. Now Suppose $\mathbf{B}_j \neq \mathbf{0}_n$ for some $j > 0$. Then there exists a $n \times k$ nonzero matrix $\mathbf{C}_j$ that $E(\varepsilon_t \mathbf{z}'_{t-j}) = \mathbf{C}_j$. Note that $\varepsilon_t$ is a linear combination of current and past structural shocks, and (1.3) and (1.4) imply that $E((\mathbf{z}_t - \Gamma\mathbf{u}_{1,t})\mathbf{u}'_{i,s})$ is zero matrix for all $i$ and $s$. Then $E(\varepsilon_s(\mathbf{u}'_{1,t}\Gamma' - \mathbf{z}'_t)) = \mathbf{0}_{n\times k}$ for all $s$, followed by $E(\varepsilon_t \mathbf{u}'_{1,t-j}) = E(\varepsilon_t(\mathbf{u}'_{1,t-j}\Gamma' - \mathbf{z}'_{t-j} + \mathbf{z}'_{t-j}))\Gamma'^{-1} = \mathbf{C}_j\Gamma'^{-1}$, which is nonzero. Therefore, if $\varepsilon_t$ satisfies (1.14), then $\mathbf{B}_j = \mathbf{0}_{n\times k}$ for all $j > 0$. □

Forni and Gambetti (2014) demonstrate that the invertibility condition holds if there is no state variable that has extra forecasting ability if it is added in the VAR. Proposition 1.1 implies that given $\mathbf{z}_t$ defined as in (1.3) and (1.4), we do not need any other state variable to test if the IRF from the Proxy-SVAR approach is trustable or not.

The proposition also implies that given any scalar variable $z_t$ satisfying $E(z_t|\mathbf{Y}_{t-1}) = 0$, estimated IRF with the Proxy-SVAR approach is asymptotically equal to the IRF up to scale from following reduced-rank VAR:

$$\mathbf{y}_t = \mathbf{c} + \sum_{j=1}^{p} \Phi_j \mathbf{y}_{t-j} + \mathbf{b}_0 z_t + \mathbf{e}_t, \tag{1.17}$$

In other words, the Proxy-SVAR can be understood as a special case of the unrestricted regression (1.16) restricting lagged value of $z_t$ from directly forecast $\mathbf{y}_t$. Paul (2018) also proves that the IRF computed with (1.17) is asymptotically proportional to the IRF implied by the multi-step approach of the proxy-SVAR approach. As we show in 1.2 below, the proxy-SVAR is a special case of the unrestricted VAR allowing direct effect of lagged structural shocks of interest on $\mathbf{y}_t$.

### 1.2.4 Shorter sample period of $\mathbf{z}_t$ than $\mathbf{y}_t$

As in the case of Gertler and Karadi (2015), it is possible that the data of $\mathbf{y}_t$ covers longer period than $\mathbf{z}_t$. In the Proxy-SVAR approach, one can use longer length of the data of $\mathbf{y}_t$ by estimating the reduced-form error $\hat{\varepsilon}_t$ and then projecting on $\mathbf{z}_t$ only for the period when the proxy is available. Even without using this two-step approach, it can be shown that if the timing of available proxy is independent to the structural shocks and the measurement errors, we can estimate the unrestricted model (1.16) and test the validity of the Proxy-SVAR with the longer data of $\mathbf{y}_t$ by treating $\mathbf{z}_t = \mathbf{0}_k$ when the proxy is not available. In appendix, we demonstrate that $\mathbf{z}_t$ extended with zero vectors still can be written as (1.13) satisfying $E(\mathbf{z}_t \mathbf{w}_s^{'}) = \mathbf{0}_{k\times k}$ for all $s$, which is the key property to prove Proposition 1.1 and Proposition 1.2 discussed below.

## 1.3 Illustrations with monetary policy news

In this section, we illustrate that an existing Proxy-SVAR mis-estimates the monetary shock effect in two respects. First, we analytically solve a simple New Keynesian model with forward guidance monetary policy and show that a VAR consisting of the endogenous variables of the model violates the invertibility condition. Second, we revisit GK who estimates the effect of the monetary policy shock with its proxy variable constructed with high-frequency observations from the fed funds future market. We test the direct predictive power of the proxy of the monetary news in the VAR of GK and find significant evidence against the validity of the Proxy-SVAR approach.

### 1.3.1 A New Keynesian model with forward guidance monetary policy

#### The model setup

Consider a basic New Keynesian model with Calvo pricing in which $(i)$ the utility function of household is a log function of consumption and quadratic function of labor[7] $(ii)$ the output function of each firm is a Cobb-Douglas function of labor with fixed level of technology, and $(iii)$ the nominal interest rate is set up by the central bank following the Taylor rule. The model can be summarized

[7] This utility function implies that the intertemporal elasticity of substitution and Frisch labor supply elasticity are one. Further details of the model setup is described in Appendix

with three equations below:

$$\pi_t = \beta E_t \pi_{t+1} + \kappa^* x_t, \tag{1.18}$$

$$x_t = -(i_t - E_t \pi_{t+1}) + E_t x_{t+1} \tag{1.19}$$

$$i_t = \phi_\pi \pi_t + \phi_x x_t + v_t \tag{1.20}$$

(1.18) is New Keynesian Philips curve[8], (1.19) is IS curve, and (1.20) is monetary policy equation. The only difference of this model from the basic New Keynesian model is the specification on the monetary disturbance $v_t$. Following existing studies of theoretical models with forward guidance including Lasen and Svensson (2011), Del Negro et al. (2015) and Keen et al. (2017), let

$$\psi(L) v_t = w_t, \tag{1.21}$$

where the lag operator $\psi(L)$ governs the degree of persistence of the monetary policy, and $w_t$ is the composite term of the two monetary policy shocks $u_{11,t}$ and $u_{12,t}$ as below:

$$w_t = \sum_{j=1}^{q} \alpha_j u_{2,t-j} + \alpha_0 u_{1,t} \tag{1.22}$$

Here, $u_{2,t}$ is the forward guidance news and $u_{1,t}$ is the monetary policy shock that increases the interest rate instantaneously. The two shocks satisfy $E(u_{i,t}|\Omega_{t-1}) = 0$ , $E(u_{i,t}^2|\Omega_{t-1}) = 1$ and $E(u_{1,t}u_{2,t}|\Omega_{t-1}) = \rho_{12}$ for $i = 1,2$, where $\Omega_{t-1}$ denote the agent's information set up to $t-1$ period.

Note that in (1.22), $w_t$ is correlated with the forward guidance monetary policy news if $\alpha_j \neq 0$ for some $j > 0$: a news shock $u_{2,t-j}$ announced at period $t-j$ affects the interest rate in period $t$ by $\alpha_j$ with delay by $j$ periods, while the agent acknowledges the news when at the announcement. For simplicity, we assume that there is no miscommunication or information asymmetry between the

[8] $\kappa^*$ is a function of labor supply elasticity and the Calvo parameter, which determines the probability that each firm can change its price in a period.

central bank and the economic agents. That is, once the monetary policy news is announced, it is perfectly known by the household and the firm (i.e., $E_t u_{2,t+j} = 0$ for $j > 0$, and $E_t u_{2,t-j} = u_{2,t-j}$ for $j \geq 0$). In this model, there is no other shock than $u_{1,t}$ and $u_{2,t}$ and thus the equilibrium dynamics of the variables observed by the econometrician depends only on the current and past values of the monetary policy news.

### Forecasting errors forecasted by monetary news

For illustration, consider a simple case of $\phi_x = 0$ and $\psi(L) = 1$, in which the Taylor rule only depends on inflation and the monetary policy shock has no persistence.[9] Defining $\mathbf{y}_t = (\pi_t, x_t)'$ , we can rewrite the system of the three equations with following matrix notations:

$$\mathbf{A}_0 \mathbf{y}_t = \mathbf{A}_1 E_t \mathbf{y}_{t+1} + \mathbf{R} v_t, \tag{1.23}$$

where

$$\mathbf{A}_0 = \begin{bmatrix} 1 & -\kappa^* \\ \phi_\pi & 1 \end{bmatrix}, \quad \mathbf{A}_1 = \begin{bmatrix} \beta & 0 \\ 1 & 1 \end{bmatrix}, \quad \text{and } \mathbf{R} = \begin{bmatrix} 0 \\ -1 \end{bmatrix}$$

From (1.23), we can solve $\mathbf{y}_t$ as a function of expected discounted present value of monetary disturbances:

$$\mathbf{y}_t = \sum_{s=1}^{\infty} \mathbf{A}^{*s-1} \mathbf{R}^* E_t v_{t+s-1} \tag{1.24}$$

where $\mathbf{A}^* = \mathbf{A}_0^{-1} \mathbf{A}_1$ and $\mathbf{R}^* = \mathbf{A}_0^{-1} \mathbf{R}$. Since

$$E_t v_{t+j} = \begin{cases} \alpha_j u_{2,t} + \alpha_{j+1} u_{2,t-1} + \cdots + \alpha_q u_{2,t-q+j} & \text{for } j = 1, \cdots, q \\ 0 & \text{for } j > q \end{cases}$$

[9] Generalizing this assumption does not affect the implication of the model.

$\mathbf{y}_t$ is a linear combination of $u_{1,t}$ and $u_{2,t}, u_{2,t-1}, \cdots, u_{2,t-q}$ as below:

$$\mathbf{y}_t = \mathbf{R}^* \alpha_0 u_{1,t} + \sum_{j=1}^{q} \mathbf{R}^* \alpha_j u_{2,t-j} + \sum_{s=1}^{q} \mathbf{A}^{*s} \mathbf{R}^* \sum_{j=s}^{q} \alpha_j u_{2,t+s-j} \tag{1.25}$$

In case of $(\alpha_0, \alpha_1, \cdots, \alpha_q) = (1, 0, 0, \cdots, 0)$, there is no forward guidance and the news changes the interest rate with no lag. The solution of output gap is simply white noise as below:

$$x_t = \left( \frac{-1}{1 + \kappa^* \phi_\pi} \right) u_{1,t} \tag{1.26}$$

If the true data generating process of the output gap is as in (1.26), the econometrician can estimate the IRF of interest up to its scale with the observation of $\{x_t\}_{t=1}^{T}$. On the other hand, if $(\alpha_0, \alpha_1, \cdots, \alpha_q) = (0, 1, 0, \cdots, 0)$, the news changes the interest rate with one-period lag and the output gap follows MA(1) process:

$$x_t = \left( \frac{-1}{1 + \kappa^* \phi_\pi} \right) (\theta u_{2,t} + u_{2,t-1}), \tag{1.27}$$

where

$$\theta = \frac{1 + \kappa^* (\phi_\pi \beta - 1)}{1 + \kappa^* \phi_\pi}$$

If $\theta > 1$ , the MA(1) process is invertible and $u_{2,t}$ can be recovered from the statistical MA(1) model or AR with log enough lags. However, if $\theta \leq 1$, $x_t$ follows a noninvertible MA process and thus $u_{2,t}$ cannot be recovered by an econometrician without further information. With conventional values of the model parameters, we can show that (1.27) is not invertible: in order to show $\theta \leq 1$ , it is enough to show that $\kappa^*(\phi_\pi \beta - 1) \leq \kappa^* \phi_\pi$. The NKPC with reasonable values of the Calvo parameter and labor elasticity implies $\kappa^* \geq 0$. In case of $\kappa^* > 0$, we only need to compare $\phi_\pi \beta - 1$ and $\phi_\pi$. As long as $\phi_\pi > 0$, we have $\phi_\pi(\beta - 1) < 1$ or $\phi_\pi \beta - 1 < \phi_\pi$ and thus $\theta \leq 1$ ($\theta = 1$ when $\kappa^* = 0$).

Note that $\theta > 1$ implies that the more recent news is discounted more than the old news. As Leeper et al. (2013) asserts, this is because the older news $u_{2,t-1}$ contains information of closer (current) periods than $u_{2,t}$ (one-period ahead). In DSGE models without the news shocks of the delayed effects, $u_{2,t}$ is less discounted than $u_{2,t-j}$ for $j > 0$, and this is how the econometrician's model treats the shocks if he estimates the model parameters simply by a VAR of $\mathbf{y}_t$. However, since the underlying MA process (1.27) is not invertible, even if the econometrician knows the true dynamics of the output - in this case MA(1) - the residual estimated from the statistical model is not consistent with $u_{2,t}$. Specifically, it depends on $u_{2,t-j}$ for $j > 0$ as

$$\varepsilon_t = \theta u_{2,t} + (1-\theta^2)u_{2,t-1} - \theta(1-\theta^2)u_{2,t-2} + \theta^2(1-\theta^2)u_{2,t-3} - \cdots, \tag{1.28}$$

where $\varepsilon_t$ is the reduced-form error from $x_t = \delta_0\varepsilon_t + \delta_1\varepsilon_{t-1}$ or $x_t = \sum_{j=1}^{p} \phi x_{t-1} + \varepsilon_t$ for large enough $p$ to approximate the MA(1).

Now consider general cases with more structural shocks or additional channels[10] in which the monetary policy shock affects economy. Let $\mathbf{u}_{1,t}$ denote the vector of the monetary policy news $(u_{1,t}, u_{2,t})'$, and $\mathbf{u}_{2,t}$ denote other structural shocks independent to $\mathbf{u}_{1,t}$. The log-linearized equilibrium dynamics of $\mathbf{y}_t$ in a DSGE model follows vector ARMA process

$$\Phi(L)\mathbf{y}_t = \Theta(L)\mathbf{u}_t, \tag{1.29}$$

where $\mathbf{u}_t = (\mathbf{u}_{1,t}, \mathbf{u}_{2,t})'$ is the vector of all structural shocks. As discussed above, if monetary policy disturbance contains anticipated news $u_{2,t}$ as in (1.22), we cannot guarantee that the MA component $\Theta(L)\mathbf{u}_t$ is invertible. If it is not invertible, the reduced-form error of a VAR or vector ARMA of $\mathbf{y}_t$ can depend on the past values of $\mathbf{u}_t$ as in (1.28). Then the "shocks" computed with a linear combination of the reduced form errors are predicted by the true structural shocks and the IRF is inconsistent. In our example, even with large size of observable information set, the econometrician cannot estimate the effect of the unanticipated monetary policy shock $u_{1,t}$ as well as the forward guidance news $u_{2,t}$ with conventional VAR of $\mathbf{y}_t$, unless $\mathbf{u}_{1,t}$ is contained in his information set.

[10] For instance, the DSGE model in Gertler and Karadi (2011) allows financial friction, and the model in Nakamura and Steinsson (2018) relaxes the assumption of symmetric information between the central bank and the private sector.

### 1.3.2 Testing the invertibility condition in a Monetary VAR

The theoretical model in the previous subsection implies that the proxy variable for monetary policy shock also predicts the reduced-form errors, and thus $z_{t-j}$ for some $j > 0$ predicts $\mathbf{y}_t$ even under control of the lagged values of it. We empirically investigate this property by testing the zero-predictive power restriction in the monetary VAR in GK. They estimate the effect of a monetary policy shock to macroeconomic variables with monthly VAR implied by the DSGE model in Gertler and Karadi (2011)[11] with $p = 12$ and $\mathbf{y}_t = (GS1_t, \ln IP_t, \ln CPI_t, EBP_t)'$ or $(FFR_t, \ln IP_t, \ln CPI_t, EBP_t)'$, where $FFR$ is the Federal funds rate, $GS1$ is one-year treasury rate, $\ln IP_t$ is log of industrial production, $\ln CPI_t$ is log of consumer product index, and $EBP_t$ is excess bond premium of Gilchrist and Zakrajek (2012) as a measure of credit cost. The monthly series of the VAR variables cover from 1979M7 to 2012M6, and the data of the proxy variable for the monetary policy shock starts from 1991M1 due to its limited availability.

**The proxy variable of monetary policy news**

In GK, the monetary policy shock and its effect is identified with the Proxy-SVAR approach illustrated in Section 1.2. In line with the studies[12] estimating effect of the monetary surprise with high frequency identification (HFI), GK approximate the monetary shocks with daily observations of the changes in the fed funds futures on FOMC meeting dates and use the proxy variable as the IV in the Proxy-SVAR framework. The settlement price of the interest rate futures expiring $j$ month later reflects the economic agent's expectation on the interest rate in the next $j$ months, and the change of this price is the direct measure of revision in the agent's expectation on the future interest rates. The change is computed within 30 minute window of each announcement to ensure that the innovation in the future price is only due to the Fed's decision in the meeting. Following Kuttner (2001), the

[11] Due to the financial friction allowed in their model, the credit spread measured by the gap between the borrowing cost in the private sector and the government bond rate arises as an additional channel that amplifies the effect of the monetary policy shock to the real economy.

[12] See Kuttner (2001), Hamilton (2008), Campbell et al. (2012).

daily series is transformed into the monthly series in order to combine with the monthly VAR.[13]

Formally, let $\Omega_t^*$ denote the information set of economic agents up to period $t$ before the monetary policy news is announced. It contains current and past values of economic variables and structural shocks except the monetary news $u_{11,t}$ and $u_{12,t}$. Let $f_{t+j,\tau}^{post}$ denote the settlement price on day $\tau$ in month $t$ of the interest rate futures expiring in month $t+j$ right after (say 30 minute) the FOMC meeting, and $f_{t+j,\tau}^{pre}$ denote the settlement price on the same day in the same month of the same future right before the announcement. Assuming that there is no change in the risk premium in the short window (Piazzesi and Swanson, 2008 and Kuttner, 2001), the gap between $f_{t+j,\tau}^{post}$ and $f_{t+j,\tau}^{pre}$ on each FOMC meeting day approximates the revision of the agent's expectation on the interest rate after $j$ month only due to the monetary policy news:

$$f_{t+j,\tau}^{post} - f_{t+j,\tau}^{pre} \approx E(i_{t+j}|\Omega_t^*, u_{11,t}, u_{12,t}) - E(i_{t+j}|\Omega_t^*) \tag{1.30}$$

GK employ two proxy variables with $j=0$ and $j=3$, denoted by $FF1$ and $FF4$, for two different VARs.[14] Since $FF4$ reflects the market's revision on the future path of the interest rate, it is correlated with the forward guidance news. On the other hand, $FF1$ is only correlated with the innovation in the current month's Federal funds rate, so it only represents the monetary shock that changes the interest rate instantaneously.

**Testing the validity of the Proxy-SVAR approach**

Figure 1.1 presents IRFs of GK's baseline model with $GS1$ and $FF4$ as the proxy variable estimated with the Proxy-SVAR approach (black line with 90% confidence band) and the one-step approach with the reduced-rank VAR (red line). For comparison, we control the scale of the shock so that the contemporaneous change in the interest rate to be one basis point. As shown in the figure, the two IRFs are extremely close, supporting the discussion on the reduced rank VAR (1.17).

[13]Specifically, for each day $\tau$ of the month, surprises on any FOMC days between $\tau$ and $\tau-31$ are accumulated. Then the monthly series is computed by taking average of these monthly surprises across each day of the month.

[14]In other words, only one proxy ($k=1$) is used in each Proxy-SVAR.

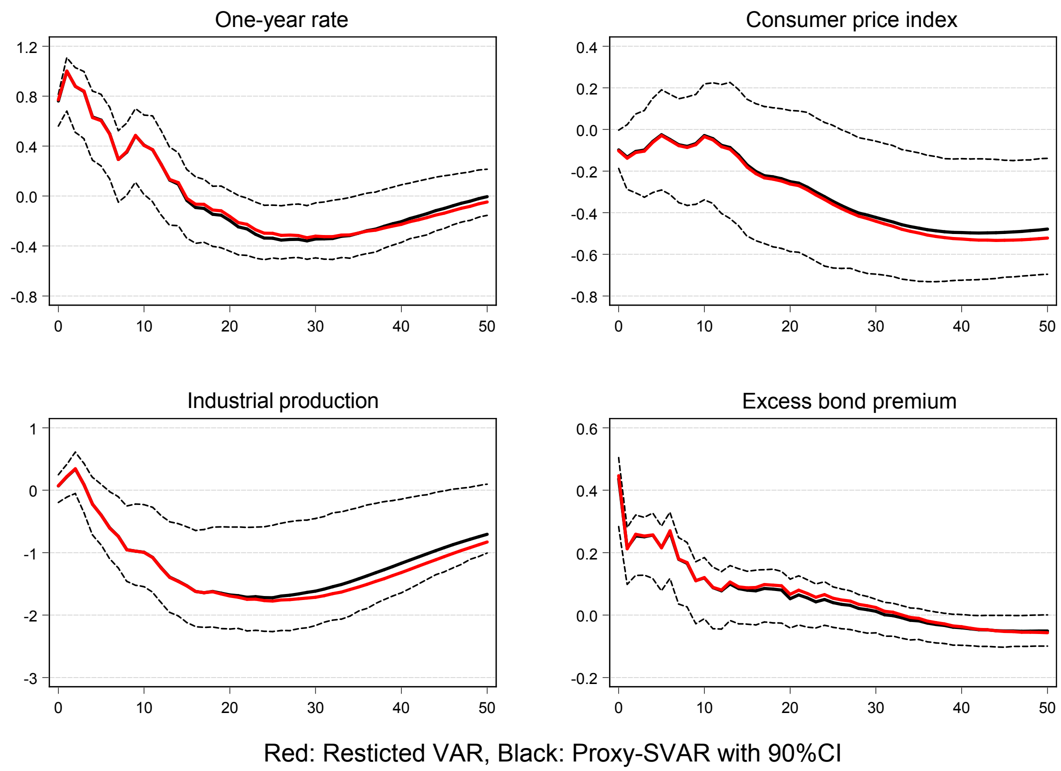


**Figure 1.1**: IRF from the Proxy-SVAR and the reduced rank VAR

Note: this figure presents IRF of the GK's baseline model with $\mathbf{y}_t = (GS1_t, \log CPI_t, \log IP_t, EBP_t)'$ and $z_t = FF4_t$. The black line with 90% confidence interval is the IRF estimated with the Proxy-SVAR approach in GK, and the red line is the IRF from the reduced rank VAR (1.17). We control the scale of shock to fix the initial response of the interest rate to be 1 basis point.

**Table 1.1**: Test of the zero-predictive power of $FF1$ and $FF4$

| Policy indicator | Proxy variable | $q$ | $\chi^2(df)$ | p-value |
|---|---|---|---|---|
| Panel A: Forecasting ability of $FF4$ and $FF1$ ($p = 12$) | | | | |
| $GS1$ | $FF4$ | 12 | 69.6835 (48) | 0.0221 |
| | | 24 | 119.7539 (96) | 0.0508 |
| $FFR$ | $FF1$ | 12 | 92.5623 (48) | 0.0001 |
| | | 24 | 141.0272 (96) | 0.0019 |
| Panel B: Forecasting ability of $FF4$ and $FF1$ ($p = 24$) | | | | |
| $GS1$ | $FF4$ | 12 | 78.4431 (48) | 0.0036 |
| | | 24 | 134.8787 (96) | 0.0055 |
| $FFR$ | $FF1$ | 12 | 180.0419 (48) | 0.0000 |
| | | 24 | 120.1276 (96) | 0.0000 |

The table presents the results of the likelihood ratio test for the zero-predictive power restriction of the proxy variables of monetary policy shock $FF1$ and $FF4$ in the VAR models with the policy indicator as $GS1$ and $FFR$. $p$ and $q$ denote the number of lags of $\mathbf{y}_t$ and $z_t$ in (1.16), respectively. $\chi^2$ denotes the test statistics computed as described in the text, and $df$ is the degree of freedom of $\chi^2$.

Then we test the zero-predictive power restriction under the null hypothesis that $\mathbf{b}_j$ in (1.16) are zero for $j \geq 1$. Table 1.1 shows the test results, in which the likelihood ratios $LR$ on the forth column are computed by

$$LR = T(\ln|\Sigma_{\mathbf{ee},0}| - \ln|\Sigma_{\mathbf{ee},1}|),$$

where $\Sigma_{\mathbf{ee},0}$ and $\Sigma_{\mathbf{ee},1}$ denote the variance-covariance matrix of $\mathbf{e}_t$ in the restricted model (1.17) and the unrestricted model (1.16), respectively. The asymptotic distribution of the $LR$ is $\chi^2$ distribution with degree of freedom $nq$. The p-value is calculated under the null hypothesis that the proxy variable has no direct predictive power on $\mathbf{y}_t$. We also test the direct predictive power of the surprise in the current month's short-term future rate denoted by $FF1$ and the model with the federal funds rate ($FFR$) as the policy indicator.

According to the test results, the null hypothesis of the (partial) invertibility is significantly rejected in both specifications with $GS1$ and $FFR$. It is also shown that the direct predictive power of $FF4$ and $FF1$ does not disappear even with the increased number of lags from $p = 12$ to 24. Rather, the p-value of the test statistics decreases substantially as we control more lags. The significant forecasting ability of $FF4$ and $FF1$ reject the validity of the Proxy-SVAR approach in GK's formulation.

By construction, $FF4$ reflects the revision of the agent's expectation on the future interest rates, and thus it is correlated with the forward guidance monetary policy news. As discussed in the previous subsection, the reduced-form error in the conventional VAR of $\mathbf{y}_t$ is predicted by the lagged values of the news shock and the unanticipated shock, which implies that the proxy variables of the monetary policy surprises have additional predictive power on $\mathbf{y}$ beyond its history.

## 1.4 Impulse-response analysis without the invertibility condition

The theoretical model and the empirical evidence presented in Section 1.3 call for a generalized approach to estimate IRF allowing the direct predictive power of the proxy variable. We prove that given a serially uncorrelated proxy variable independent to the uninterested shocks, the IRF computed from the unrestricted model (1.16) is the consistent estimator of the true IRF up to its scale even with nonzero measurement error. In other words, although an IV or a proxy variable for the shock is defined outside of the VAR, we can directly control the current and past values of it in the VAR for the consistent impulse-response function under control of scale. With a proper number of additional restrictions on the model parameters, we can identify multiple structural shocks with multiple proxy variables. Since the regression with the past values of the proxy employs all available information, our approach is more efficient than other regression-based methods including LP-IV. As an application, we re-estimate the models in GK.

### 1.4.1 Unrestricted IRF with the proxy variable

The aim of an impulse-response analysis is to estimate the $n\times H$ matrix $[\theta_{1i,1}\,\theta_{1i,2}\cdots\theta_{1i,H}]$ where $\theta_{1i,h}$ denotes the response of $\mathbf{y}_{t+h}$ to $u_{i,t}$. Due to the measurement error in the proxy variable, the scale of the shock (e.g, the magnitude of effect of $u_{i,t}$ on $y_{i,t}$ for each $i$) cannot be estimated if the invertibility fails to hold. Instead of estimating the absolute magnitude of the IRF, we focus on estimating the IRF up to scale for each $i=1,2,\cdots,k$. The scale can be controlled by the researcher before the estimation , for example, he can define a unit monetary policy shock as the shock that increases the Federal funds rate by one basis point on the shock period.

The Proposition 1.2 stated below implies that given IVs or proxy variables satisfying (1.3) and (1.4), we can compute the IRF of interest simply by controlling its current and past values in the VAR:

**Proposition 1.2.** *Consider the structural MA representation of observation $\{\mathbf{y}_t\}_{t=1}^{T}$ in (1.1). And consider a vector of $k$ proxy variables $\mathbf{z}_t$ satisfying (1.3) and (1.4). Let $\Sigma_{zz}=E(\mathbf{z}_t\mathbf{z}_t^{'})$, $\mathbf{D}_\Gamma=diag(\gamma_{11},\gamma_{22},\cdots,\gamma_{kk})$ with $\gamma_{ii}$ the ith diagonal element of $\Gamma$, and $\Gamma^*=\mathbf{D}_\Gamma^{-1}\Gamma$. For each $h=1,2,\cdots,H$, let $\Theta_{z,h}=[\ \theta_{z1,h}\quad\theta_{z2,h}\quad\cdots\quad\theta_{zk,h}\ ]$, where*

$$\theta_{zi,h}=E(\mathbf{y}_{t+h}|z_{i,t}=1,\mathbf{Y}_{t-1},\mathbf{Z}_{t-1})-E(\mathbf{y}_{t+h}|z_{i,t}=0,\mathbf{Y}_{t-1},\mathbf{Z}_{t-1})$$

*Then for each $i=1,2,\cdots,k$,*

$$\begin{bmatrix}\theta_{1i,0} & \theta_{1i,1} & \cdots & \theta_{1i,H}\end{bmatrix}=\gamma_{ii}\begin{bmatrix}\theta_{zi,0} & \theta_{zi,1} & \cdots & \theta_{zi,H}\end{bmatrix}\Sigma_{zz}\left(\Gamma^{*'}\right)^{-1} \tag{1.31}$$

*Proof.* As in (1.13), we can write $\mathbf{u}_{1,t}=\Pi\mathbf{z}_t+\mathbf{w}_t$ with $\Pi=\Gamma^{'}\Sigma_{zz}^{-1}$. Then (1.1) can be rewritten as

$$\mathbf{y}_{t+h}=\sum_{j=0}^{\infty}\Theta_{1,j}\Gamma^{'}\Sigma_{zz}^{-1}\mathbf{z}_{t+h-j}+\sum_{j=0}^{\infty}\Theta_{1,j}\mathbf{w}_{t+h-j}+\sum_{j=0}^{\infty}\Theta_{2,j}\mathbf{u}_{2,t+h-j} \tag{1.32}$$

Note that $E(\mathbf{w}_s|\mathbf{z}_t,\mathbf{Y}_{t-1},\mathbf{Z}_{t-1})=E(\mathbf{w}_s|\mathbf{Y}_{t-1},\mathbf{Z}_{t-1})$ for all $s$, since $\mathbf{z}_t$ is orthogonal to $\mathbf{w}_s$, $\mathbf{Y}_{t-1}$, and

$\mathbf{Z}_{t-1}$. Since $\mathbf{z}_t$ is only correlated with $\mathbf{u}_{1,t}$ and $\eta_t$, both of which are uncorrelated with $\mathbf{u}_{2,s}$ for all $s$, we have $E(\mathbf{u}_{2,s}|\mathbf{z}_t, \mathbf{Y}_{t-1}, \mathbf{Z}_{t-1}) = E(\mathbf{u}_{2,s}|\mathbf{Y}_{t-1}, \mathbf{Z}_{t-1})$ for all $s$. Then

$$\begin{aligned} \Theta_{z,h} &= [\ \theta_{11,h} \quad \theta_{12,h} \quad \cdots \quad \theta_{1k,h}\ ]\Gamma^{'}\Sigma_{zz}^{-1}. \\ &= [\ \theta_{11,h} \quad \theta_{12,h} \quad \cdots \quad \theta_{1k,h}\ ]\mathbf{D}_{\Gamma}\mathbf{D}_{\Gamma}^{-1}\Gamma^{'}\Sigma_{zz}^{-1} \\ &= [\ \gamma_{11}\theta_{11,h} \quad \gamma_{22}\theta_{12,h} \quad \cdots \quad \gamma_{kk}\theta_{1k,h}\ ]\Gamma^{*'}\Sigma_{zz}^{-1}, \end{aligned} \tag{1.33}$$

followed by (1.31). □

In contrast to Proxy-SVAR, each element in $\mathbf{u}_{1,t}$ is allowed to directly forecast future $\mathbf{y}_{t+j}$, and the number of structural shocks can be larger than the number of variables in the VAR. Proposition 1.2 implies that once we know the values of $\{\Theta_{z,h}\}_{h=1}^{H}$, $\Sigma_{zz}$ and $\Gamma^{*}$, we can estimate the IRF to each structural shock in $\mathbf{u}_{1,t}$ up to its scale. With the estimated values of $\{\hat{\Phi}_j\}_{j=1}^{p}$ and $\{\hat{\mathbf{B}}_j\}_{j=0}^{q}$ in the unrestricted forecasting model (1.16), $\{\hat{\Theta}_{z,h}\}_{h=1}^{H}$ can be computed using following companion form

$$\mathscr{Y}_t = \mathcal{F}\mathscr{Y}_{t-1} + \mathcal{H}_z\mathbf{z}_t + \mathcal{H}_v\mathbf{v}_t, \tag{1.34}$$

where $\mathscr{Y}_t = (\mathbf{Y}_t, \mathbf{Z}_t)$ with $\mathbf{Y}_t = (\mathbf{y}_t^{'}, \mathbf{y}_{t-1}^{'}, \cdots, \mathbf{y}_{t-p+1}^{'})^{'}$ and $\mathbf{Z}_t = (\mathbf{z}_t^{'}, \mathbf{z}_{t-1}^{'}, \cdots, \mathbf{z}_{t-q+1}^{'})^{'}$. $\mathcal{F}$, $\mathcal{H}_z$, and $\mathcal{H}_v$ consists of the model parameters in (1.16), identity matrices and zero vectors.[15] $\Theta_{z,h}$ can be written as a function of $\mathcal{F}$ and $\mathcal{H}_m$ as

$$\Theta_{z,h} = \begin{bmatrix} \mathbf{I}_n & \mathbf{0}_{n\times\{(p-1)n+qk\}} \end{bmatrix} \mathcal{F}^h\mathcal{H}_z,$$

where $[\ \mathbf{I}_n \quad \mathbf{0}_{n\times\{(p-1)n+qk\}}\ ]$ selects the first $n$ elements of $\mathcal{F}^h\mathcal{H}_z$.

Since $\hat{\Sigma}_{zz} = T^{-1}\sum_{t=1}^{T}\mathbf{z}_t\mathbf{z}_t^{'}$ and all diagonal elements of $\Gamma^{*}$ are one, the remaining unknown parameters are the off-diagonal elements of $\Gamma^{*}$. (1.16) implies $\Theta_{z,0} = \mathbf{B}_0$, followed by $\mathbf{B}_0\Sigma_{zz} = \Theta_{1,0}^{*}\Gamma^{*'}$ from (1.33) with $\Theta_{1,0}^{*} = \Theta_{1,0}\mathbf{D}_{\Gamma}$. Given $k(k-1)$ additional restrictions on $\Theta_{1,0}^{*}$ or $\Gamma^{*}$, we

[15] See appendix for further detail on the definition of $\mathscr{F}$, $\mathcal{H}_m$, and $\mathcal{H}_v$

can identify $nk+k(k-1)$ parameters in $\Theta^*_{1,0}$ and $\Gamma^*$ with $n \cdot k$ parameters of $\mathbf{B}_0\Sigma_{zz}$.

### 1.4.2 Monte Carlo experiments

We generate following vector ARMA (1,1) series of $\mathbf{y}_t = (y_{1,t}, y_{2,t}, y_{3,t})'$ with $z_t$ 1,000 times, and estimate the IRF for $h = 1, 2, \cdots, 12$ with $(i)$ the unrestricted regression $(ii)$ Proxy-SVAR and $(iii)$ LP-IV:

$$\begin{bmatrix} y_{1,t} \\ y_{2,t} \\ y_{3,t} \end{bmatrix} = \begin{bmatrix} 0.6 & 0 & 0 \\ 0 & 0.6 & 0 \\ 0 & 0 & 0.6 \end{bmatrix} \begin{bmatrix} y_{1,t-1} \\ y_{2,t-1} \\ y_{3,t-1} \end{bmatrix} + \begin{bmatrix} 1 & 0 & 0 \\ 0 & 1 & 0 \\ 0 & 0 & 1 \end{bmatrix} \begin{bmatrix} u_{1,t} \\ u_{2,t} \\ u_{3,t} \end{bmatrix} + \begin{bmatrix} 1.5 & 0 & 0 \\ 1 & 0.5 & 0 \\ -0.5 & 0 & 0.5 \end{bmatrix} \begin{bmatrix} u_{1,t-1} \\ u_{2,t-1} \\ u_{3,t-1} \end{bmatrix} \tag{1.35}$$

$$z_t = d_t(\gamma u_{1,t} + \sigma_\eta \eta_t) \tag{1.36}$$

$$d_t = 1(d_t^* > 0.5) \tag{1.37}$$

where $(u_{1,t}, u_{2,t}, u_{3,t}, \eta_t)' \sim \mathbf{N}(\mathbf{0}_4, \mathbf{I}_4)$ and $d_t^* \in (0,1)$ follows uniform distribution. The sample size of the simulated data is $T = 300$. We set $\gamma = \sigma_\eta = 0.5$. The specification of $z_t$ implies that the proxy variable is available with the probability of 0.5 and it contains 50% of measurement error if $d_t = 1$.

Figure 1.2 shows the true IRF and the IRFs from the aforementioned three approaches. For the unrestricted regression we allow $p = q = 4$. The IRF with LP-IV is the estimated value of $\beta_{l,h}$ in

$$y_{l,t+h} = z_t\beta_{l,h} + \lambda_{l,h}\mathbf{x}_{t-1} + u_{l,t+h},$$

where $\mathbf{x}_{t-1}$ includes lagged values of $z_t$ and $\mathbf{y}_t$ up to 4 lags. For the Proxy-SVAR, we estimate with

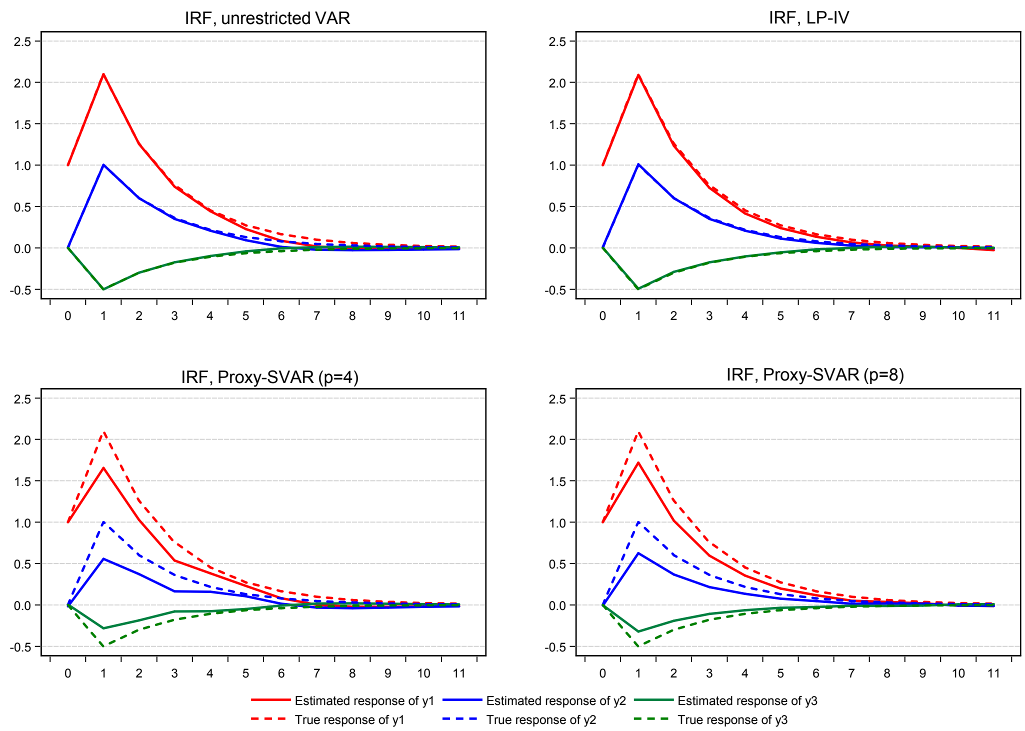


**Figure 1.2**: Monte Carlo experiment: average the of simulated IRFs

Note: the figure presents average values of the IRFs computed from 1,000 simulated data with the data generating process (1.35), (1.36), and (1.37). The sample size of the simulated data is 200. We set $\gamma = \sigma_\eta = 0.5$.

$p = 4$ and 8. We control the scale of the shock so that the average of 1,000 simulated the responses of $y_{1,t}$ to $u_{1,t}$ at the impact period equals one. As the figure presents, our method and the LP-IV estimates the IRF consistently, while the IRF of the Proxy-SVAR is inconsistent even with large $p$. The reason of the bias is the failure of the invertibility condition: in the data generating process, the direct effect of $u_{1,t-1}$ on $\mathbf{y}_t$ is larger than that of $u_{1,t}$, and thus the reduced-form VAR of $\mathbf{y}_t$ cannot recover $u_{1,t}$ even with $z_t$. On the other hand, adding the current and past values of $z_t$ in the VAR eliminates the omitted variable bias under control of the scale even with $\eta_t$, $d_t$, and $\gamma$ which prevent $z_t$ from being the perfect measure of $u_{1,t}$.

In Figure 1.3, we compare the mean-squared errors (MSE) of our approach, Proxy-SVAR, and LP-IV. The MSE of the LP-IV is larger than our approach and the ratio of the MSE of the two

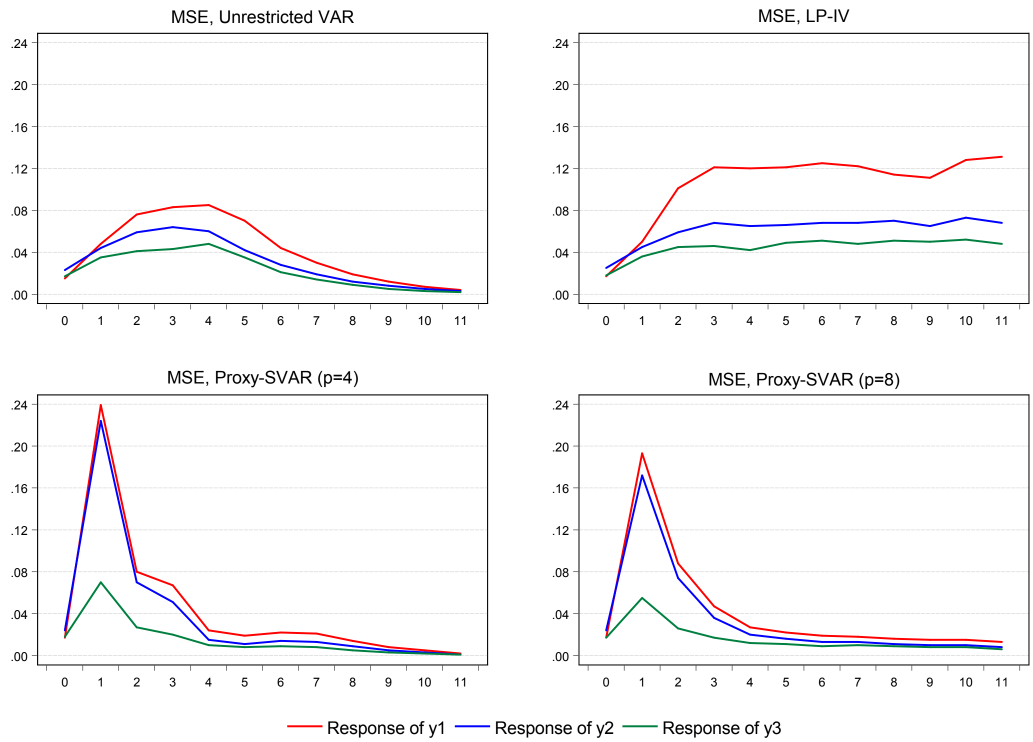


**Figure 1.3**: Monte Carlo experiment: MSE of the simulated IRFs

Note: the figure presents MSE of the IRFs computed from 1,000 simulated data with the data generating process (1.35), (1.36), and (1.37). The sample size of the simulated data is $T = 300$. We set $\gamma = \sigma_\eta = 0.5$.

approaches increases exponentially as $h$ increases. Since both approaches yields consistent estimator of the IRF up to scale, the gap between the MSE represents the gap of the efficiency. The LP-IV directly regress $\mathbf{y}_{t+h}$ on $z_t$, and the estimator of IRF suffers from the loss of the efficiency due to the dropped information between the period $t$ and $t+h$. Due to the inconsistency, the MSE of the Proxy-SVAR is higher in the periods shortly after the shock.

### 1.4.3 Re-estimating GK's model without the invertibility condition

Based on Proposition 1.2, we re-estimate IRF with the same $\mathbf{y}_t$ and $z_t$ as in GK allowing the direct effect of past monetary shocks through the lagged values of the proxy variable. We estimate two models in GK, one with the one-year Treasure rate $(GS1)$ and the IV to be $FF4$ and the other with the Federal funds rate ($FFR$) and $FF1$. Since there is only one structural shock of interest with one corresponding proxy variable in each model, we do not need any additional restrictions on the parameters for the impulse-response analysis.

As pointed out by Ramey (2016) and Stock and Watson (2018), each proxy variable for monetary shock has autocorrelation induced by transforming the daily series of the future prices into the monthly ones. By construction, it is reasonable to assume that $z_t$ is uncorrelated with the past or future values of other structural shocks than the monetary news. To transform each proxy variable into white noise as in (1.3), we project $FF1$ and $FF4$ on its lagged values up to three periods and obtain the residuals to use them as $z_t$ for each model.

Figure 1.4 and 1.5 compare the estimated IRFs from the Proxy-SVAR approach and the unrestricted regression with 90% confidence band. In each case, we control the scale of the shock so the response of the interest rate in each specification increases by one basis point (0.25%). With fixed $p = 12$ as in GK, we choose $q$ based on the sequential log likelihood test.[16] The figures imply that applying our unrestricted approach to the GK's model results in substantially different IRFs from their estimates. The response of Federal funds rate and one-year Treasury rate to the monetary news is estimated to be significantly larger and more persistent. The monetary shock effects on the output and consumer price index become larger in short-term, but insignificant shortly after as opposed to the significant mid-term effects under the invertibility assumption. Relaxing the restriction also changes the sign and the magnitude of the effects on the credit market significantly: the response of the credit cost estimated without the restriction is higher in the short run around $h = 6$, but becomes lower in after 6 months with negative sign.

## 1.5 Conclusion

This paper suggests a simple and efficient approach for impulse-response analysis with proxy variables of structural shocks. Instead of using the proxy variable as IV for the shock identification

[16] For the model with $GS1$ and $FF4$, we use $q = 16$. With $FFR$ and $FF1$, we use $q = 15$ .

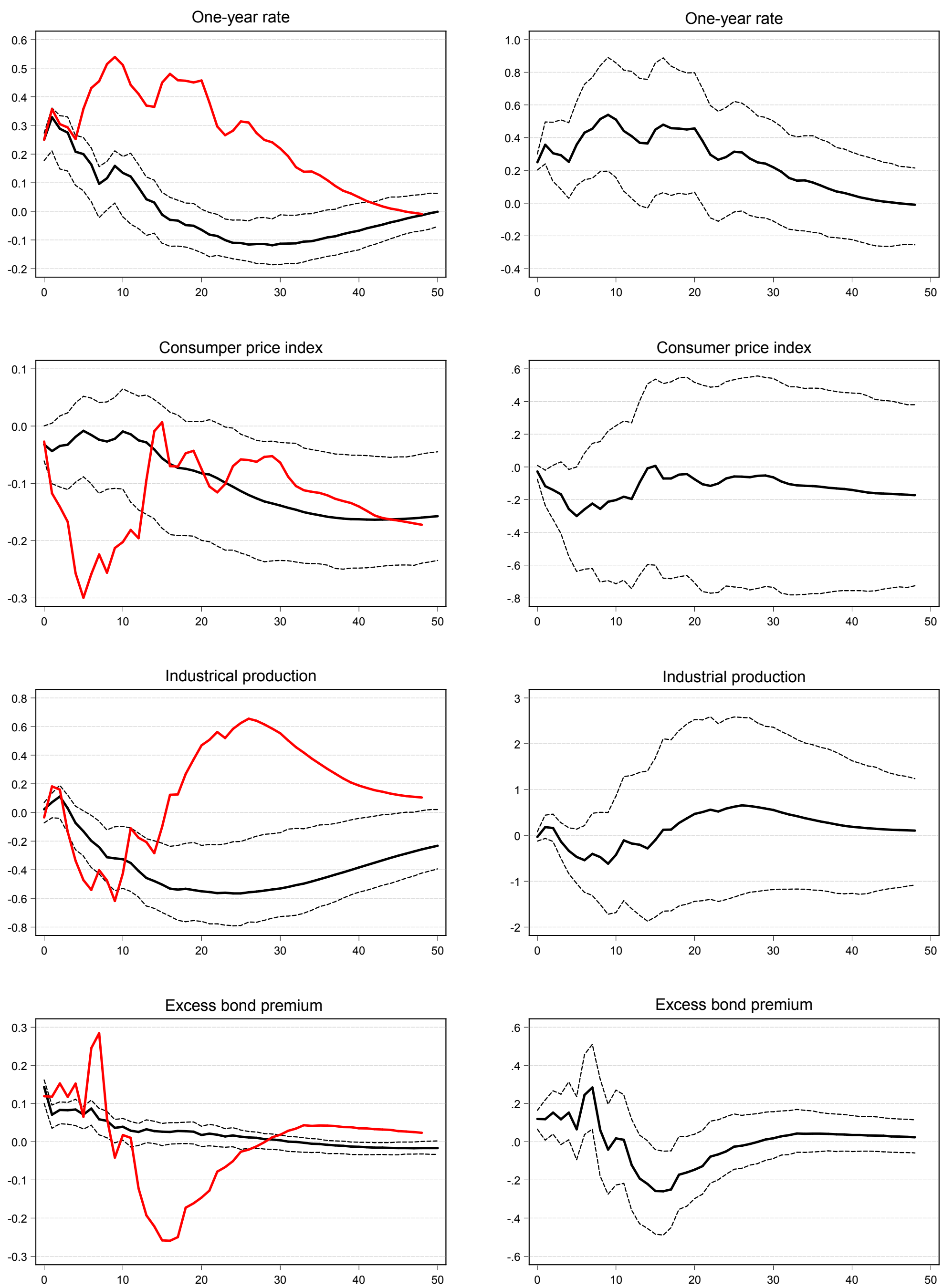


**Figure 1.4**: Comparison of the IRF from the unrestricted model and proxy SVAR: $\mathbf{y}_t = (GS1_t, \log CPI_t, \log IP_t, EBP_t)'$ and $z_t = FF4_t$

Note: In the left column, the black line with 90% confidence interval is the IRF estimated with the Proxy-SVAR approach, and the red line is the IRF from the unrestricted model ( 1.16). The right column presents the IRF of the unrestricted model with 90% confidence interval. We control the scale of shock to fix the initial response of the interest rate to be 1 basis point. For both IRFs, we fix $p = 12$ as in GK. For the unrestricted regression, we choose $q$ with sequential log-likelihood test.

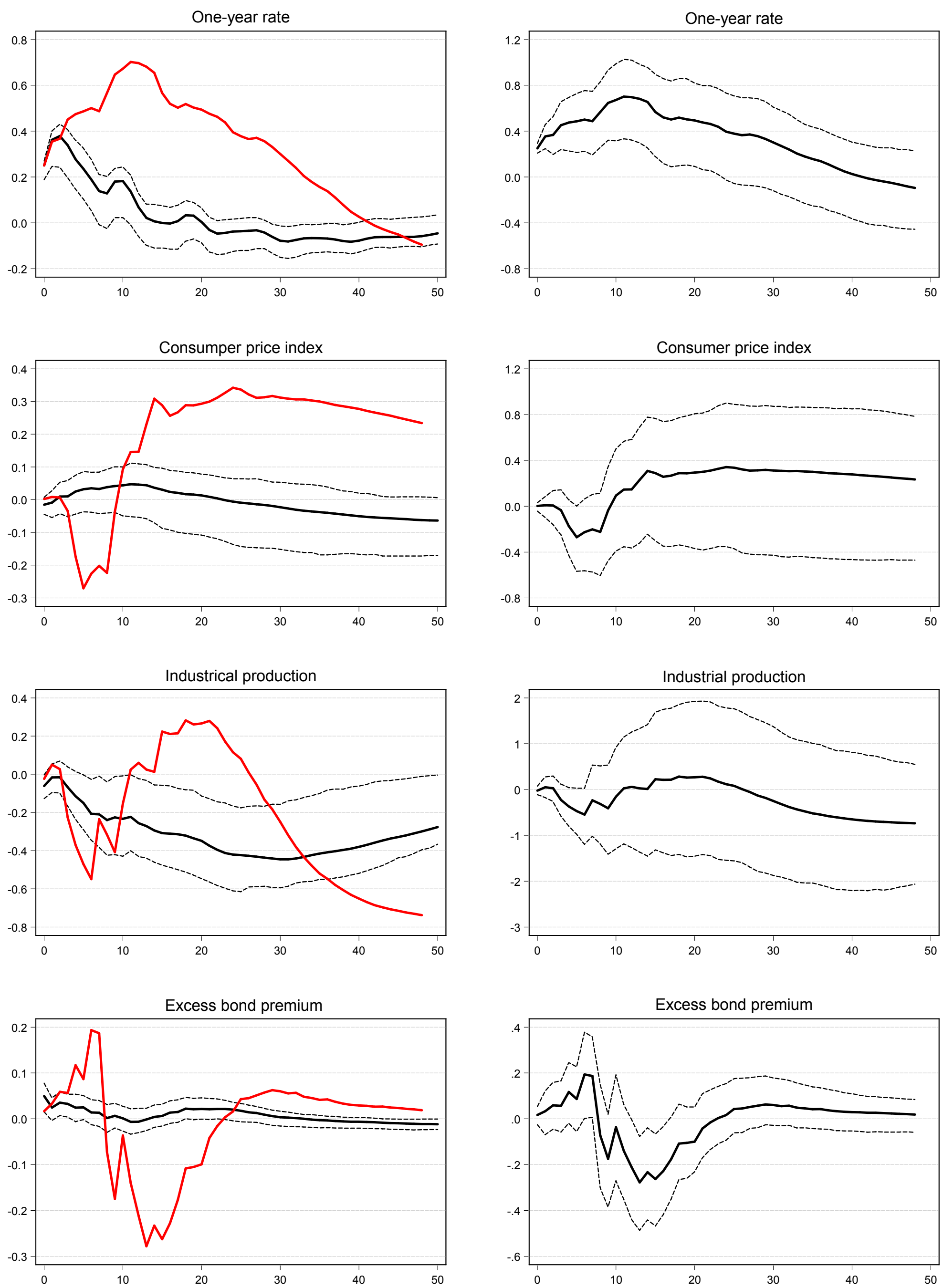


**Figure 1.5**: Comparison of the IRF from the unrestricted model and proxy SVAR: $\mathbf{y}_t = (FFR_t, \log CPI_t, \log IP_t, EBP_t)'$ and $z_t = FF1_t$

Note: In the left column, the black line with 90% confidence interval is the IRF estimated with the Proxy-SVAR approach, and the red line is the IRF from the unrestricted model ( 1.16). The right column presents the IRF of the unrestricted model with 90% confidence interval. We control the scale of shock to fix the initial response of the interest rate to be 1 basis point. For both IRFs, we fix $p = 12$ as in GK. For the unrestricted regression, we choose $q$ with sequential log-likelihood test.

assuming the invertibility condition, we use the proxy variables as additional regressors in the VAR. We prove that after removing its autocorrelation, controlling the current and past values of the proxy variable in the VAR yields consistent estimator of IRF under control of its scale. We also show that the Proxy-SVAR, which is the most efficient approach under the invertibility and linearity, is valid if and only if the pre-whitened proxy variable has no direct forecasting ability if it is added in the VAR. Our unrestricted regression is an efficient alternative to the LP-IV approach, which also does not rely on the invertibility condition but suffers from loss of efficiency. With the simple theoretical macroeconomic model and empirical evidence, we show that the existing Proxy-SVAR mis-estimates the monetary policy shock effect. We show that the proxy variable of the monetary news has significant extra predictive power to the variables in the VAR, and our unrestricted approach results in substantially different IRFs from the Proxy-SVAR approach.

## 1.6 Acknowledgement

Chapter 1, in full, is currently being prepared for submission for publication of the material. The dissertation author, Eul Noh, was the primary author of this chapter.

## Appendix 1.A Identification strategy in Proxy-SVAR (Mertens and Ravn, 2013)

Consider the case of $k = 1$. As in the main text, our interest is to identify the first structural shock $u_{1,t}$. As mentioned in the main text, consider the order of the variables in the VAR such that the correlation of $u_{1,t}$ and the reduced-form error in the first equation $\varepsilon_{1,t}$ is nonzero. Under the invertibility condition, we can consider following partition of $\mathbf{u}_t$, $\varepsilon_t$ and $\mathbf{H}$:

$$\underbrace{\begin{bmatrix} \varepsilon_{1,t} \\ \varepsilon_{2,t} \end{bmatrix}}_{\varepsilon_t} = \underbrace{\begin{bmatrix} h_{11} & \mathbf{h}_{12} \\ \mathbf{h}_{21} & \mathbf{h}_{22} \end{bmatrix}}_{\mathbf{H}} \underbrace{\begin{bmatrix} u_{1.t} \\ \mathbf{u}_{2,t} \end{bmatrix}}_{\mathbf{u}_t}, \tag{1.38}$$

where $h_{11} \neq 0$. And consider a partition of variance-covariance matrix of $\varepsilon_t$ as below:

$$\Sigma_{\varepsilon\varepsilon} = \begin{bmatrix} \Sigma_{11} & \Sigma_{12} \\ \Sigma_{21} & \Sigma_{22} \end{bmatrix}$$

After estimating the reduced-form VAR (1.6), one can estimate $\hat{\varepsilon}_t$ and $\hat{\Sigma}_{\varepsilon\varepsilon}$. As shown in (1.12), $h_{11}^{-1}\mathbf{h}_{21}$ can be estimated by regressing $\hat{\varepsilon}_{1,t}$ on $\hat{\varepsilon}_{2,t}$ with $z_t$ as an instrument variable:

$$\hat{h}_{11}^{-1}\hat{\mathbf{h}}_{21} = \left( \sum_{t=1}^{T} z_t \hat{\varepsilon}_{1,t} \right)^{-1} \sum_{t=1}^{T} z_t \hat{\varepsilon}_{2,t} \tag{1.39}$$

Once $\mathbf{h}_{12}\mathbf{h}_{12}^{'}$ is given, one can identify $h_{11}$ assuming its sign from

$$h_{11}^2 = \Sigma_{11}^2 - \mathbf{h}_{12}\mathbf{h}_{12}^{'}, \tag{1.40}$$

which is derived from $\Sigma_{\varepsilon\varepsilon} = \mathbf{H}\mathbf{H}^{'}$. Mertens and Ravn (2013) show that

$$\mathbf{h}_{12}\mathbf{h}_{12}^{'} = \left( \Sigma_{21} - h_{11}^{-1}\mathbf{h}_{21}\Sigma_{11} \right)^{'} \mathbf{Q} \left( \Sigma_{21} - h_{11}^{-1}\mathbf{h}_{21}\Sigma_{11} \right), \tag{1.41}$$

where

$$\mathbf{Q} = h_{11}^{-1}\mathbf{h}_{21}\Sigma_{11}\mathbf{h}_{21}^{'}h_{11}^{-1} - \left( \Sigma_{21}\mathbf{h}_{21}^{'}h_{11}^{-1} + h_{11}^{-1}\mathbf{h}_{21}\Sigma_{21}^{'} \right) + \Sigma_{22} \tag{1.42}$$

With $\hat{\Sigma}_{\varepsilon\varepsilon}$ and $\hat{h}_{11}^{-1}\hat{\mathbf{h}}_{21}$ from (1.39), one can calculate $\hat{\mathbf{Q}}$ with (1.42) and $\hat{h}_{11}$ with (1.40) and (1.42). Then finally, $\hat{\mathbf{h}}_{12}$ is recovered from (1.39) to obtain $\hat{\mathbf{h}}_1 = (\hat{h}_{11}, \hat{\mathbf{h}}_{21}^{'})^{'}$.

# Appendix 1.B Setup for the New Keynesian model

## 1.B.1 Household

The representative consumer maximizes her utility function

$$E_0 \sum_{t=0}^{\infty} \beta^t (\log(C_t) - L_t^2/2)$$

where $C_t = \left(\int_0^1 C_t(i)^{\frac{\sigma-1}{\sigma}} di\right)^{\frac{\sigma}{\sigma-1}}$ is consumption index, $L_t$ is labor, and $\beta \in (0,1)$ is time preference. $C_t(i)$ for $i \in [0,1]$ is the variety of continuum of consumption goods. The household faces budget constraint

$$\int_0^1 P_t(i) C_t(i) di + Q_t B_t = B_{t-1} + W_t L_t + \Pi_t,$$

where $P_t(i)$ is price of consumption good $C_t(i)$, $Q_t$ is the price of zero coupon bond $B_t$, $W_t$ is labor wage, and $\Pi_t$ is profit from the firm.

## 1.B.2 Firm

Each firm $i$ hires labor $L_t(i)$ and produces output goods with technology

$$Y_t(i) = L_t(i)^{1-\alpha}$$

to maximizes its profit. As in the standard New Keynesian models in the literature, we assume that (i) firms face same labor wage $W_t$, (ii) the labor market is perfectly competitive and (iii) each firm face same demand curve

$$Y_t(i) = Y_t(P_t(i)/P_t)^{-\sigma}$$

where $Y_t = \left(\int_0^1 Y_t(i)^{\frac{\sigma-1}{\sigma}} di\right)^{\frac{\sigma}{\sigma-1}}$ is aggregate output. We assume Calvo pricing model - in each period, each firm resets its price only with probability $1-\theta$, where the chance of the price reset is realized independently across periods.

## Appendix 1.C Shorter sample period of $\mathbf{z}_t$ than $\mathbf{y}_t$

Treating $\mathbf{z}_t = \mathbf{0}_k$ when the proxy is not available, we can write $\mathbf{z}_t = d_t \mathbf{z}_t^*$, where $d_t$ is the scalar indicator variable and $\mathbf{z}_t^* = \Gamma \mathbf{u}_{1,t} + \eta_t$ with $\eta_t$ as in (1.4). If $d_t$ is independent to $\mathbf{u}_t$ and $\eta_t$,

$$E(\mathbf{z}_t^* \mathbf{u}_{i,s}^{'} | d_t) = E(\mathbf{z}_t^* \mathbf{u}_{i,s}^{'}) = \begin{cases} \Gamma & \text{if } i = 1 \text{ and } t = s \\ \mathbf{0}_{k \times k} & \text{otherwise} \end{cases} \tag{1.43}$$

$$E(\mathbf{z}_t^* \mathbf{z}_s^{*'} | d_t, d_s) = E(\mathbf{z}_t^* \mathbf{z}_s^{*'}) = \begin{cases} \Sigma_{zz} & \text{if } t = s \\ \mathbf{0}_{k \times k} & \text{otherwise} \end{cases}, \tag{1.44}$$

Let $\Pi = E(\mathbf{u}_{1,t} \mathbf{z}_t^{*'}) E(\mathbf{z}_t^* \mathbf{z}_t^{*'})^{-1} = \Gamma' \Sigma_{zz}^{-1}$ and $\mathbf{w}_t = \mathbf{u}_{1,t} - \Pi \mathbf{z}_t$. Conditional on $d_t = 1$, $\mathbf{w}_t$ is the regression error from projecting $\mathbf{z}_t = \mathbf{z}_t^*$ on $\mathbf{u}_{1,t}$, and $E(\mathbf{z}_t \mathbf{w}_t^{'} | d_t = 1) = \mathbf{0}_{k \times k}$. Since $\mathbf{z}_t = \mathbf{0}_k$ if $d_t = 0$, $E(\mathbf{z}_t \mathbf{w}_t^{'} | d_t = 0) = \mathbf{0}_{k \times k}$. For $t \neq s$, $E(\mathbf{u}_{1,t} \mathbf{z}_s^{'} | d_s = 1) = E(\mathbf{u}_{1,t} \mathbf{z}_s^{*'})$, which is equal to $\Pi \mathbf{E}(\mathbf{z}_t \mathbf{z}_s^{*'} | d_s = 1) + E(\mathbf{z}_t^* \mathbf{w}_s^{'} | d_s = 1)$. From (1.43) and (1.44), $E(\mathbf{u}_{1,t} \mathbf{z}_s^{*'}) = E(\mathbf{z}_t \mathbf{z}_s^{*'} | d_s = 1) = \mathbf{0}_{k \times k}$, and we have $E(\mathbf{z}_t \mathbf{w}_s^{'} | d_s = 1) = \mathbf{0}_{k \times k}$. Since $\mathbf{z}_s = \mathbf{0}_k$ if $d_s = 0$, $E(\mathbf{u}_{1,t} \mathbf{z}_s^{'} | d_s = 0) = E(\mathbf{z}_t \mathbf{z}_s^{'} | d_s = 0) = \mathbf{0}_{k \times k}$ and $E(\mathbf{z}_t \mathbf{w}_s^{'} | d_s = 0) = \mathbf{0}_{k \times k}$. Therefore, $E(\mathbf{z}_t \mathbf{w}_s^{'}) = \mathbf{0}_{k \times k}$ for all $s$.

## Appendix 1.D Companion form of the unrestricted forecasting model

Using the companion form, we can simplify the unrestricted forecasting model (1.16) in the main text as below:

$$\underbrace{\begin{bmatrix} \mathbf{Y}_t \\ \mathbf{M}_t \end{bmatrix}}_{\mathscr{Y}_t} = \underbrace{\begin{bmatrix} \mathcal{F} & \mathcal{B} \\ \mathbf{0}_{kq\times np} & \mathcal{S} \end{bmatrix}}_{\mathscr{F}} \underbrace{\begin{bmatrix} \mathbf{Y}_{t-1} \\ \mathbf{M}_{t-1} \end{bmatrix}}_{\mathscr{Y}_{t-1}} + \mathcal{H}_z \mathbf{z}_t + \mathcal{H}_v \mathbf{v}_t, \tag{1.45}$$

where

$$\mathcal{F} = \begin{bmatrix} \tilde{\Phi} \\ \tilde{\mathbf{S}} \end{bmatrix}, \quad \tilde{\mathbf{S}} = \begin{bmatrix} \mathbf{I}_{n(p-1)} & \mathbf{0}_{n(p-1)\times n} \end{bmatrix}, \quad \tilde{\Phi} = \begin{bmatrix} \Phi_1 & \Phi_2 & \cdots & \Phi_p \end{bmatrix}$$

$$\mathcal{B} = \begin{bmatrix} \tilde{\mathbf{B}} \\ \mathbf{0}_{n(p-1)\times kq} \end{bmatrix}, \quad \tilde{\mathbf{B}} = \begin{bmatrix} \mathbf{B}_1 & \mathbf{B}_2 & \cdots & \mathbf{B}_q \end{bmatrix}$$

$$\mathcal{S} = \begin{bmatrix} \mathbf{0}_{k\times k(q-1)} & \mathbf{0}_{k\times k} \\ \mathbf{I}_{k(q-1)} & \mathbf{0}_{k(q-1)\times k} \end{bmatrix} \quad \mathcal{H}_z = \begin{bmatrix} \mathbf{B}_0 \\ \mathbf{0}_{n(p-1)\times k} \\ \mathbf{I}_k \\ \mathbf{0}_{k(q-1)\times k} \end{bmatrix} \quad \mathcal{H}_v = \begin{bmatrix} \mathbf{I}_n \\ \mathbf{0}_{\{n(p-1)+kq\}\times n} \end{bmatrix}$$

# Reference

Bloom, N. (2009). The Impact of Uncertainty Shocks. *Econometrica*, 77(3):623685.

Campbell, J., Evans, C. L., Fisher, J., and Justiniano, A. (2012). Macroeconomic Effects of Federal Reserve Forward Guidance. *Brookings Papers on Economic Activity*, 43(1 (Spring)):1–80.

Carriero, A., Mumtaz, H., Theodoridis, K., and Theophilopoulou, A. (2015). The Impact of Uncertainty Shocks under Measurement Error: A Proxy SVAR Approach. *Journal of Money, Credit and Banking*, 47(6):1223–1238.

Christiano, L. J., Eichenbaum, M., and Evans, C. (1996). The Effects of Monetary Policy Shocks: Evidence from the Flow of Funds. *The Review of Economics and Statistics*, 78(1):16–34.

Del Negro, M., Giannoni, M., and Patterson, C. (2015). The Forward Guidance Puzzle. Staff Reports 574, Federal Reserve Bank of New York.

Forni, M. and Gambetti, L. (2014). Sufficient Information in Structural VARs. *Journal of Monetary Economics*, 66(C):124–136.

Gertler, M. and Karadi, P. (2011). A Model of Unconventional Monetary Policy. *Journal of Monetary Economics*, 58(1):17 – 34. Carnegie-Rochester Conference Series on Public Policy: The Future of Central Banking April 16-17, 2010.

Gertler, M. and Karadi, P. (2015). Monetary Policy Surprises, Credit Costs, and Economic Activity. *American Economic Journal: Macroeconomics*, 7(1):44–76.

Gilchrist, S. and Zakrajek, E. (2012). Credit Spreads and Business Cycle Fluctuations. *American Economic Review*, 102(4):1692–1720.

Grkaynak, R. S., Sack, B., and Swanson, E. (2005). Do Actions Speak Louder Than Words? The Response of Asset Prices to Monetary Policy Actions and Statements. *International Journal of Central Banking*, 1(1).

Hamilton, J. D. (1994). *Time Series Analysis*. Princeton Univ. Press, Princeton, NJ.

Hamilton, J. D. (2008). Daily Monetary Policy Shocks and New Home Sales. *Journal of Monetary Economics*, 55(7):1171 – 1190.

Hamilton, J. D. (2009). Daily Changes in Fed Funds Futures Prices. *Journal of Money, Credit and Banking*, 41(4):567–582.

Hansen, L. P. and Sargent, T. J. (1991). *Rational Expectations Econometrics*. Westview Press.

Hausman, J. A. (1978). Specification Tests in Econometrics. *Econometrica*, 46(6):1251–1271.

Jord, . (2005). Estimation and Inference of Impulse Responses by Local Projections. *American Economic Review*, 95(1):161–182.

Keen, B. D., Richter, A. W., and Throckmorton, N. A. (2017). Forward Guidance and the State of the Economy. *Economic Inquiry*, 55(4):1593–1624.

Kliem, M. and Kriwoluzky, A. (2013). Reconciling Narrative Monetary Policy disturbances with Structural VAR model Shocks? *Economics Letters*, 121(2):247–251.

Kuttner, K. N. (2001). Monetary policy surprises and interest rates: Evidence from the Fed funds futures market. *Journal of Monetary Economics*, 47(3):523 – 544.

Lasen, S. and Svensson, L. E. O. (2011). Anticipated Alternative Instrument-Rate Paths in Policy Simulations. *The International Journal of Central Banking*, 7(3):1–35.

Leeper, E. M., Walker, T. B., and Yang, S.-C. S. (2013). Fiscal Foresight and Information Flows. *Econometrica*, 81(3):1115–1145.

Lunsford, K. G. (2015). Identifying Structural VARs with a Proxy Variable and a Test for a Weak Proxy. *Working Paper*, (1528).

Lunsford, K. G. and Jentsch, C. (2016). Proxy SVARs: Asymptotic Theory, Bootstrap Inference, and the Effects of Income Tax Changes in the United States. *Working Paper*, (1619).

Mertens, K. and Ravn, M. O. (2013). The Dynamic Effects of Personal and Corporate Income Tax Changes in the United States. *American Economic Review*, 103(4):1212–47.

Mertens, K. and Ravn, M. O. (2014). A Reconciliation of SVAR and Narrative Estimates of Tax Multipliers. *Journal of Monetary Economics*, 68(S):1–19.

Mumtaz, H., Pinter, G., and Theodoridis, K. (2018). What Do VARs Tell Us about the Impact of a Credit Supply Shock? *International Economic Review*, 59(2):625–646.

Nakamura, E. and Steinsson, J. (2018). High-Frequency Identification of Monetary Non-Neutrality: The Information Effect. *The Quarterly Journal of Economics*, 133(3):1283–1330.

Olea, J. L. M., Stock, J. H., and Watson, M. W. (2015). Uniform inference in SVARs Identified with External Instruments. *Working Paper*.

Olea, J. L. M., Stock, J. H., and Watson, M. W. (2016). Inference in SVARs Identified with an External Instrument. *Working Paper*.

Paul, P. (2018). The Time-Varying Effect of Monetary Policy on Asset Prices. *Working paper*.

Piazzesi, M. and Swanson, E. T. (2008). Futures Prices as Risk-adjusted Forecasts of Monetary Policy. *Journal of Monetary Economics*, 55(4):677 – 691.

Plagborg-Mller, M. and Wolf, C. K. (2018). Instrumental Variable Identification of Dynamic Variance Decompositions. *Working paper*.

Ramey, V. (2016). Macroeconomic Shocks and Their Propagation. *Handbook of Macroeconomics*, 2:71–162.

Romer, C. D. and Romer, D. H. (2004). A New Measure of Monetary Shocks: Derivation and Implications. *American Economic Review*, 94(4):1055–1084.

Romer, C. D. and Romer, D. H. (2010). The Macroeconomic Effects of Tax Changes: Estimates Based on a New Measure of Fiscal Shocks. *American Economic Review*, 100(3):763–801.

Sims, C. A. (1980). Macroeconomics and Reality. *Econometrica*, 48(1):1–48.

Sims, C. A., Stock, J. H., and Watson, M. W. (1990). Inference in Linear Time Series Models with Some Unit Roots. *Econometrica*, 58(1):113–144.

Stock, J. H. and Watson, M. W. (2008). Recent Developments in Structural VAR Modeling. *NBER Summer Institute Whats New in Econometrics: Time Series*.

Stock, J. H. and Watson, M. W. (2012). Disentangling the Channels of the 2007-09 Recession. *Brookings Papers on Economic Activity*, 43(1 (Spring)):81–156.

Stock, J. H. and Watson, M. W. (2018). Identification and Estimation of Dynamic Causal Effects in Macroeconomics Using External Instruments. *Working Paper*.

# Chapter 2

# Cyclical Variation in the Government Spending Multipliers: A Markov-switching SVAR approach

## 2.1 Introduction

Measuring the size of fiscal multipliers is a central issue in macroeconomics. Given that many countries used discretionary fiscal policy to combat weak economic growth during the Great Recession, a key question is how effective government purchases are in bad times. Some studies, including Barro and Redlick (2011) and Ramey (2011), find government spending multipliers smaller than 1 on average over a long history, suggesting that increases in government spending crowd out private demand to some extent.

However, there is a long-lasting belief in economics that the effects of fiscal policy are stronger in bad times relative to normal times so that the multipliers for recession could potentially be much larger than 1. The textbook Keynesian theory tells us that the effects of government spending are stronger when there is more slack in the economy because private consumption and investment are less likely to be crowded out when resources are underutilized. Canzoneri et al. (2016) show that a standard business cycle model equipped with costly financial intermediation is capable of generating large fiscal multipliers in recessions and small multipliers in expansions. Michaillat (2014) develops a New Keynesian model in which the effects of government-led stimulation policy differ across the business cycle phases even in the absence of the zero lower bound. He shows that an additional hire in the public sector would crowd out less private employment when unemployment is high.

Recently there has been a growing number of empirical studies investigating the state dependence of the government spending multipliers. However, these studies reach very different conclusions. Auerbach and Gorodnichenko (2012a,b), Bachmann and Sims (2012), Mittnik and Semmler (2012) and Caggiano et al. (2015) find much larger government spending multipliers for recession than for expansion. Fazzari et al. (2015) also find that the government spending multipliers become larger and more persistent during times of slack. By contrast, Owyang et al. (2013) and Ramey and Zubairy (2018) do not observe larger multipliers when there is substantial economic slack in the United States. Bognanni (2013) and Alloza (2016) present evidence that the multipliers could even be smaller in economic downturns than in booms.

Using a U.S. dataset from 1890Q1-2015Q4 developed by Ramey and Zubairy (2018), we document the countercyclical behavior of the government spending multipliers: the multiplier values are larger in recessions than in expansions, and the gaps are statistically significant. More specifically, the mutipliers are around 0.5 in expansions and around 0.9 in recessions. To estimate the state-dependent responses of aggregate output to an unanticipated and exogenous change in government spending, we build a Markov-switching structural VAR that includes government spending, tax revenue, and output as endogenous variables and the military spending news as an exogenous variable. We assume that there are two unobserved regimes of the economy, and the dynamics of the economy vary with the regime. The regime can always shift from one to the other with constant transition probabilities. Then we estimate the model, and as suggested by Ramey and Zubairy (2018), we calculate the government spending multiplier at some specific horizon as the cumulative change in output normalized by the cumulative change in government spending in response to a military spending news shock. We calculate the multipliers for various horizons conditional on the regime when the shock occurs, and compare the multiplier values for different regimes to check if the effects of government purchases depend on the regime of the economy.

Our model has two distinctive features. First, following Jefferson (1998), we use both quantitative and qualitative information to infer the regime of the economy. Whereas economists know that the size of fiscal multipliers may vary with the regime, a consensus on how to measure the regime does not exist. In a typical Markov regime-switching model where the regime is assumed unobserved, one can use quantitative data to estimate the probability of occurrence of each regime for any historical period. We can then relate these inferred probabilities to other indicators. For example, if the probability is substantially different during periods that the Dating Committee of the NBER designates as recession, we can confidently claim that the unobserved regimes correspond to different business cycle phases. Since this inference is a byproduct of the estimation of the model, it is determined entirely by the data used. In addition to this purely data-driven approach, the regime could also be determined directly by some simple qualitative indicators. For example, Alloza (2016) uses the NBER business cycle dates to measure expansion and recession. Owyang et al. (2013) and Ramey and Zubairy (2018) assume that the economy is in the non-slack regime if the unemployment rate is below 6.5% and in the slack regime if otherwise. The conclusions of various studies could depend critically on how they measure the regime of the economy.[1] Different inference methods mean different data observations are used to inform the model parameters for each regime, which in turn give different estimates of state-dependent fiscal multipliers.

Both the inference based on quantitative data and the inference based on qualitative indicators have drawbacks. The former method is not accurate if the data quality is not good enough, and the

[1]Bognanni (2013) and Alloza (2016) argue that differences in the methods used to calculate the time-varying probability of occurrence of recession is the main reason why their results differ from Auerbach and Gorodnichenko (2012a).

latter is arbitrary to a large extent. Jefferson (1998) proposes a method of combining quantitative and qualitative information to improve inference in a simple Markov regime-switching model. Instead of taking the qualitative indicator as a perfect measure for the regime, he assumes it to be a proxy for the regime with measurement error and we are able to assess how reliable the indicator variable is. We generalize Jefferson's approach to the VAR framework and consider multiple qualitative indicators at the same time. In particular, we use the NBER business cycle dates as well as the indicator variable based on the unemployment rate as in Owyang et al. (2013) and Ramey and Zubairy (2018). Our result shows that the effects of government spending are much more likely to change with the official business cycle phases than with the labor market condition. This explains why Owyang et al. (2013) and Ramey and Zubairy (2018) do not find significant differences in the government spending multipliers between times of high unemployment and times of low unemployment, although their results are robust to using the NBER dates as an alternative measure for the regime of the economy.

The other important feature of our model is that by taking advantage of the Markov-switching framework, we develop a simple recursive method for estimating the dynamic effects of a government spending shock, allowing the regime of the economy to change naturally after the shock. In contrast, it is hard to construct impulse response functions in existing nonlinear VAR models. For simplicity, many papers, including Auerbach and Gorodnichenko (2012a), Bachmann and Sims (2012), Bognanni (2013), and Alloza (2016), compute impulse response functions under the assumption that the regime is fixed permanently. Given the fact that the average duration of U.S. recessions is about only six quarters, their results tend to obscure the true effects of government purchases that happen in recessions. We show that if we prohibit the regime from changing, we will obtain much larger multipliers in recessions. In particular, we estimate the 5-year multiplier for recession to be 1.9. This explains why the leading study by Auerbach and Gorodnichenko (2012a) obtains multipliers as high as 2.24 in recessions. As an alternative to the nonlinear VAR approach, Auerbach and Gorodnichenko (2012b), Owyang et al. (2013), and Ramey and Zubairy (2018) compute impulse response functions using Jordà (2005)'s local projection method, which amounts to a direct forecast of future output in response to a government spending shock conditional on the regime when the shock hits. Although the local projection method implicitly allows the regime to switch after the shock, it suffers a notable efficiency loss due to its nonparametric property. We show that differences in the methods used to estimate impulse response functions can explain why Ramey and Zubairy (2018) fail to find statistically significant variations in the government spending multipliers even if they use the NBER business cycle dates to measure the regime.

At the end of this paper, we consider a generalization of our model that allows the regime transition probabilities to be dependent on government spending shocks. The rationale is that an expansionary fiscal policy may help the economy escape from recession while a contractionary fiscal policy may end an expansion early. Our result shows that the influence of fiscal policy shocks on the regime is insignificant, which justifies our assumption of constant transition probabilities. We also show that allowing for time-varying transition probabilities does not change our conclusion about the state-dependent effects of government spending shocks.

Our paper is related to three strands of literature. First, we contribute to the literature gauging the size of fiscal multipliers, such as Blanchard and Perotti (2002), Ramey (2011), and Auerbach and Gorodnichenko (2012a) among others. Our results corroborate the existing evidence that fiscal policy is more effective in economic downturns than in booms. However, the multipliers in recessions are smaller than 1, which is consistent with the finding in Ramey and Zubairy (2018). The second strand of literature investigates the asymmetric effects of aggregate shocks over the business cycle; see Auerbach and Gorodnichenko (2012a), Caggiano et al. (2014), and Tenreyro and Thwaites (2016). We contribute by providing a useful framework that could be easily applied to study the cyclical effects of monetary policy shocks or uncertainty shocks. Our model is more convenient than the existing nonlinear VAR models for impulse response analysis and more efficient than the local projection method. The last strand of literature applies Markov-switching models in macroeconomics. Hamilton (2016) serves as an excellent survey. We extend this literature by exploring a Markov-switching structural VAR, which takes advantage of both quantitative and qualitative information for inference about the regime and generates impulse response functions that allow for regime changes.

The remainder of this paper proceeds as follows. We present our econometric model in section 2. In section 3, we briefly describe the data we use and show our main results. Section 4 shows how we reconcile the main findings in previous studies. Section 5 discusses the extension of our model that allows for time-varying regime transition probabilities. Section 6 concludes.

sectionEconometric Model

In this section, we present a Markov regime-switching structural VAR used to estimate the state-dependent effects of government spending shocks on aggregate output. We assume there are two regimes of the economy, and the dynamics of the economy change according to the regime. Then we show how to estimate the model and draw probabilistic inferences about the regime. Finally we illustrate how we compute the government spending multipliers for different regimes.

### 2.1.1 A Markov-switching Structural VAR

As is conventional in the literature beginning with Auerbach and Gorodnichenko (2012a), we build a structural VAR to describe the behavior of $\mathbf{y}_t$ that includes government spending ($G_t$), tax revenue ($T_t$), and output ($Y_t$).[2] The model is recursively identified so that shocks to tax revenue and output can not affect government spending contemporaneously. Blanchard and Perotti (2002) justify this identification assumption by the observation that it usually takes policymakers more than a quarter to decide how government should change its spending in response to those shocks, pass the decisions through the legislature, and send them to implementation. However, Ramey (2011) argues that the shocks to government spending identified in this way can be predicted by war dates or professional forecasts because there is usually a lag between the announcement of a fiscal policy and its actual implementation, an issue known as fiscal foresight. From the standpoint of the neoclassical models, an increase in government spending creates a negative wealth effect for the representative

[2]The detailed definition of these variables will be discussed in the next section.

household. From this perspective, it is the change in the present discounted value of government purchases that really matters, and households react immediately once they learn the news about future government purchases. Because the conventional VAR as just described captures shocks only when they occur, it misses the initial response of the economy to the news.

To control for the timing of government spending shocks, we follow Ramey (2011) to incorporate the military spending news ($N_t$), an estimate of changes in the expected present value of military spending, in the VAR. The government spending multiplier then measures the change in output relative to the change in government spending in response to a military spending news shock. Because military spending is very likely to be orthogonal to the U.S. macroeconomic condition and there is no statistical evidence that the value of the news depends on its past values or the other variables, we assume it to be totally exogenous. The fiscal and macroeconomic variables are allowed to react to both current and lagged values of the news.

To allow the effects of government spending shocks to be state-dependent, we model the dynamics of $\mathbf{y}_t$ using a Markov-switching structural VAR (MS-SVAR hereafter)[3]:

$$\mathbf{A}_0(s_t)\mathbf{y}_t = \mathbf{c}(s_t) + \sum_{j=1}^{4} \mathbf{A}_j(s_t)\mathbf{y}_{t-j} + \sum_{j=0}^{4} \Gamma_j(s_t)N_{t-j} + \varepsilon_t, \quad \varepsilon_t \sim \mathbf{N}(\mathbf{0}, \Sigma) \tag{2.1}$$

The values of the intercept and slope parameters in equation (2.1) vary with the unobserved state variable $s_t$, which is assumed to be exogenous and follow a two-state Markov chain with transition probabilities:

$$p_{ij} \equiv p(s_t = j | s_{t-1} = i) \qquad i, j = 1, 2 \tag{2.2}$$

$\mathbf{A}_0(s_t)$ is a lower-triangular matrix with ones on the diagonal. $\mathbf{c}(s_t)$ is the intercept. $\Sigma$ is a diagonal matrix collecting the variances of structural shocks $\varepsilon_t$. There is no restriction on $\mathbf{A}_j(s_t)$ or $\Gamma_j(s_t)$. Since we use quarterly data, we set the lag order to four to capture any important dynamics.

It is worth mentioning that we follow Gordon and Krenn (2010) and Ramey and Zubairy

[3]The specification of equation (2.1) is sometimes referred to as VARX in the literature. We show in Appendix 2.B that it can be rewritten as a restricted VAR.

(2018) to divide all the variables by trend (potential) output instead of taking logarithms of the variables as in many previous studies including Auerbach and Gorodnichenko (2012a). With the logged variables in the VAR, it is necessary to convert the estimated impulse responses of government spending and output to multipliers based on the sample average of $Y_t/G_t$. Ramey and Zubairy (2018) notice that in the post-WWII sample, the value of $Y_t/G_t$ varies modestly with a mean of 5. However, in the full sample from 1890-2015 that is used in our paper, the value of $Y_t/G_t$ fluctuates widely with a mean close to 8. This means that we may overestimate the multipliers relative to the existing results which are mostly based on the post-WWII sample, if we follow the common practice in the literature. In contrast, our approach makes all the variables in the same dollar unit, which allows us to calculate the government spending multiplier directly as the ratio of the response of output to the response of government spending without using the sample average of $Y_t/G_t$.

### 2.1.2 Estimation and Inference

Let $\mathbf{x}_t$ denote the vector including all the regressors on the right-hand side of equation (2.1), and $\beta(s_t)$ denote the matrix of the corresponding coefficients. For simplicity, we can write equation (2.1) as:

$$\mathbf{A}_0(s_t)\mathbf{y}_t = \beta(s_t)\mathbf{x}_t + \varepsilon_t \tag{2.3}$$

Let $\mathcal{Y}_t \equiv \{\mathbf{y}_t, \mathbf{y}_{t-1}, \ldots, \mathbf{y}_1\}$ denote observations up to time $t$. The parameters to be estimated are collected in a column vector $\theta$, including $p_{11}$, $p_{22}$, and all the unknown elements in $\mathbf{A}_0(j)$, $\beta(j)$ $(j = 1, 2)$ and $\Sigma$. The log likelihood function of the observed sample data is:

$$\begin{aligned} \mathcal{L}(\mathcal{Y}_T;\theta) &= \sum_{t=1}^{T} \ln f(\mathbf{y}_t|N_t, \mathcal{Y}_{t-1};\theta) \\ &= \sum_{t=1}^{T} \ln\{\sum_{j=1}^{2} p(s_t = j|\mathcal{Y}_{t-1};\theta) f(\mathbf{y}_t|\mathbf{x}_t, s_t = j;\theta)\} \end{aligned} \tag{2.4}$$

where the conditional likelihood function is:

$$f(\mathbf{y}_t|\mathbf{x}_t, s_t = j;\theta) = \frac{1}{\sqrt{(2\pi)^3|\Sigma|}} \exp\{-\frac{1}{2}[\mathbf{A}_0(j)\mathbf{y}_t - \beta(j)\mathbf{x}_t]'\Sigma^{-1}[\mathbf{A}_0(j)\mathbf{y}_t - \beta(j)\mathbf{x}_t]\} \tag{2.5}$$

The maximum likelihood estimate $\hat{\theta}$ can be obtained through the expectation maximization (EM) algorithm proposed by Hamilton (1990). With the model estimated, we can draw a probabilistic inference about the unobserved state variable $s_t$ using full-sample information, denoted by $p(s_t = j|\mathcal{Y}_T;\hat{\theta})$ $(j = 1,2)$. This probability is called the smoothed inference, and an easy way to implement it has also been introduced in Hamilton (1990).

This inference relies solely on the observed sample data. One valid concern is that the available data is not rich or clean enough to shed much light on $s_t$. Instead of estimating time-varying probabilities of occurrence of each regime, some studies simply use qualitative indicators to determine the regime of the economy for each period. For example, Alloza (2016) uses the NBER business cycle dates as an indicator of the regime, and Owyang et al. (2013) determine the regime by whether the unemployment rate exceeds 6.5%. While extremely simple, the inference method based solely on qualitative information is not innocuous as the choice of qualitative information is somewhat arbitrary. As we will show later in section 4, the conclusion about the state-dependent effects of fiscal policy may depend on the qualitative indicator one chooses. If the indicator variable is not a reliable measure for the true regime, one may fail to observe the state dependence of the government spending multipliers.

Jefferson (1998) proposes a method of combining quantitative data and qualitative information for inference in a simple Markov-switching model. We generalize his method to the VAR framework, and consider various indicators that might be useful at the same time. Suppose there are $n$ different indicator variables $z_t^{(1)}, \ldots, z_t^{(n)}$ that can be regarded as independent proxies with measurement error for the unobserved state variable $s_t$. We define:

$$g_1^{(i)} \equiv p(z_t^{(i)} = 1 | s_t = 1), \quad 1 - g_1^{(i)} \equiv p(z_t^{(i)} = 2 | s_t = 1) \tag{2.6}$$

$$g_2^{(i)} \equiv p(z_t^{(i)} = 2 | s_t = 2), \quad 1 - g_2^{(i)} \equiv p(z_t^{(i)} = 1 | s_t = 2) \tag{2.7}$$

for $i = 1, 2, \ldots, n$. The parameters $g_1^{(i)}$ and $g_2^{(i)}$ indicate how reliable $z_t^{(i)}$ is on average. If $g_1^{(i)}$ and $g_2^{(i)}$ are close to 1, it suggests that the $i$th indicator contains much useful information about $s_t$. We treat $g_1^{(i)}$ and $g_2^{(i)}$ as free parameters, and estimate them along with the other parameters in the model. Let $\mathbf{w}_t \equiv \{\mathbf{y}_t, z_t^{(1)}, z_t^{(2)}, \ldots, z_t^{(n)}\}$ denote observations of both quantitative and qualitative variables at time $t$, and define $\mathcal{W}_t \equiv \{\mathbf{w}_t, \mathbf{w}_{t-1}, \ldots, \mathbf{w}_1\}$. The log likelihood function associated with our model then changes into:

$$\begin{aligned} \mathcal{L}(\mathcal{W}_T; \lambda) &= \sum_{t=1}^{T} \ln f(\mathbf{w}_t | N_t, \mathcal{W}_{t-1}; \lambda) \\ &= \sum_{t=1}^{T} \ln\{\sum_{j=1}^{2} p(s_t = j | \mathcal{W}_{t-1}; \lambda) f(\mathbf{w}_t | \mathbf{x}_t, s_t = j; \lambda)\} \end{aligned} \tag{2.8}$$

where $\lambda$ contains $g_1^{(i)}$ and $g_2^{(i)}$ $(i = 1, 2, \ldots, n)$ in addition to $\theta$. The conditional likelihood function becomes:

$$f(\mathbf{w}_t | \mathbf{x}_t, s_t = j; \lambda) = f(\mathbf{y}_t | \mathbf{x}_t, s_t = j; \theta) \prod_{i=1}^{n} p(z_t^{(i)} | s_t = j; g_1^{(i)}, g_2^{(i)}) \tag{2.9}$$

The first term on the right-hand side of equation (2.9) is exactly the same as equation (2.5), and the following product term is obtained directly from equation (2.6) or (2.7). We adapt the EM algorithm to obtain the maximum likelihood estimate $\hat{\lambda}$, and calculate the smoothed inference $p(s_t = j | \mathcal{W}_T; \hat{\lambda})$ $(j = 1, 2)$ that makes use of qualitative information as well. The implementation of the modified EM algorithm and the smoothed inference is described in Appendix 2.A. With the information set augmented, the inference about $s_t$ should be more accurate than that based only on quantitative information $\mathcal{Y}_T$.

Within this general framework, two special cases are worth noting. First, we consider

$g_1^{(i)} = g_2^{(i)} = 1$. The inference about $s_t$ becomes:

$$p(s_t = j|\mathcal{W}_T;\hat{\lambda}) = \begin{cases} 1 & \text{if } z_t^{(i)} = j \\ 0 & \text{if } z_t^{(i)} \neq j \end{cases} \qquad j = 1,2 \tag{2.10}$$

or equivalently:

$$s_t = z_t^{(i)} \tag{2.11}$$

This is the case in Alloza (2016), Owyang et al. (2013) and Ramey and Zubairy (2018) which assume that some indicator variable measures the regime of the economy perfectly. The second special case requires $g_1^{(i)} + g_2^{(i)} = 1$ for any $i = 1,2,\ldots,n$. This implies $p(z_t^{(i)} = 1|s_t = 1) = p(z_t^{(i)} = 1|s_t = 2)$ and $p(z_t^{(i)} = 2|s_t = 1) = p(z_t^{(i)} = 2|s_t = 2)$. Because the value of $z_t^{(i)}$ is totally independent of $s_t$, the qualitative information is of no use for inference about $s_t$. It is not hard to prove that in this case, the smoothed inference collapses to the one that uses only quantitative information:

$$p(s_t = j|\mathcal{W}_T;\hat{\lambda}) = p(s_t = j|\mathcal{Y}_T;\hat{\theta}) \quad j = 1,2 \tag{2.12}$$

In our application, we label regime 1 “good regime” and label regime 2 “bad regime” without loss of generality. Based on the literature and data availability, we consider two indicator variables that could be informative about $s_t$ and almost uncorrelated:

$$z_t^{NBER} = \begin{cases} 1 & \text{if period } t \text{ is an NBER dated expansion} \\ 2 & \text{if period } t \text{ is an NBER dated recession} \end{cases}$$

$$z_t^{UNEMP} = \begin{cases} 1 & \text{if the unemployment rate in period } t \leq 6.5\% \\ 2 & \text{if the unemployment rate in period } t > 6.5\% \end{cases}$$

The parameters measuring the reliability of these indicators are denoted by $g_1^{NBER}$, $g_2^{NBER}$, $g_1^{UNEMP}$, and $g_2^{UNEMP}$. Another indicator variable worth considering is based on the interest rate:

$$z_t^{ZLB} = \begin{cases} 1 & \text{if the interest rate is away from the zero lower bound} \\ 2 & \text{if the interest rate is near the zero lower bound} \end{cases}$$

Some studies argue that the effects of fiscal policy are different in the zero lower bound periods and in the normal periods. For example, Christiano et al. (2011) and Miyamoto et al. (2018), among others, argue that the government spending multipliers are larger than 1 when the nominal interest rate is zero. We obtain very similar results if we replace $z_t^{UNEMP}$ with $z_t^{ZLB}$ for inference.[4]

### 2.1.3 Computing State-dependent Multipliers

Before calculating the government spending multipliers, we need to calculate impulse response functions based on the estimated model parameters. Since the economic dynamics differs across regimes, the effects of a government spending shock should depend on the regime when the shock hits. Conditional on the regime prevailing at the time of the shock, the impulse response function is proportional to the size and symmetric in the sign of the shock because future regimes of the economy are not affected by it. Due to the assumption that the regime-switching process is governed by an exogenous Markov chain, it is easy to allow for regime transition throughout the duration of the responses.

For simplicity, we rewrite our model (2.1) as a typical four-variable VAR that augments $\mathbf{y}_t$ with $N_t$, with additional restrictions on the autoregressive coefficients, and represent it in companion form:

$$\tilde{\mathbf{y}}_t = \tilde{\mathbf{c}}(s_t) + \Phi(s_t)\tilde{\mathbf{y}}_{t-1} + \mathbf{B}(s_t)\tilde{\varepsilon}_t \tag{2.13}$$

where $\tilde{\mathbf{y}}_t = (N_t, G_t, T_t, Y_t, \ldots, N_{t-3}, G_{t-3}, T_{t-3}, Y_{t-3})'$ and $\tilde{\varepsilon}_t$ collects structural shocks. The expressions for $\tilde{\mathbf{c}}(s_t)$, $\Phi(s_t)$ and $\mathbf{B}(s_t)$ are provided in Appendix 2.B. Let $\tilde{y}_{l,t}$ denote the $l$th element of $\tilde{\mathbf{y}}_t$

[4] We can not put $z_t^{UNEMP}$ and $z_t^{ZLB}$ in the model at the same time because they are highly correlated.

for $l = 1, \ldots, 4$. We follow Koop et al. (1996) to define the generalized impulse response (*GIR*) of the $l$th variable at date $t+h$ to a shock to the $k$th variable ($k = 1, \cdots, 4$) at date $t$ conditional on the regime prevailing as:

$$GIR^{k,l}_{t+h|s_t} \equiv E(\tilde{y}_{l,t+h}|\tilde{\varepsilon}_t = \mathbf{e}_k, s_t) - E(\tilde{y}_{l,t+h}|\tilde{\varepsilon}_t = \mathbf{0}, s_t) \tag{2.14}$$

where $\mathbf{e}_k$ is the $k$th column of the identity matrix $\mathbf{I}_{16}$. Note that in this definition, the response function does not depend on future regimes of the economy $\{s_{t+1}, \ldots, s_{t+h}\}$. It implies that the *GIR* measures on average what will happen in the future given the shock and current regime. Because the *GIR* is hard to estimate in existing nonlinear VAR models such as the smooth-transition VAR and the threshold VAR, many studies instead estimate the regime-dependent impulse response function (*RDIR*):

$$RDIR^{k,l}_{t+h|s_t} \equiv E(\tilde{y}_{l,t+h}|\tilde{\varepsilon}_t = \mathbf{e}_k, s_t = \cdots = s_{t+h}) - E(\tilde{y}_{l,t+h}|\tilde{\varepsilon}_t = \mathbf{0}, s_t = \cdots = s_{t+h}) \tag{2.15}$$

The *RDIR* measures the impact of a structural shock when the economy remains in its current regime for the horizon over which we calculate the impulse response function. The *RDIR* should be close to the *GIR* when the economy is currently in expansion since expansions usually last for long time. However, if the economy is currently in recession, the *RDIR* could be very different from the *GIR* at long horizons since recessions usually last for a short period. While the *RDIR* is much easier to estimate, it is not a good measure for the realistic effects of government spending shocks that happen in recessions at horizons beyond two years. As formalized in the following proposition, the *GIR* can be expressed as a function of $\Phi(s_t)$ and $\mathbf{B}(s_t)$:

**Proposition 2.1.** $GIR^{k,l}_{t+h|s_t} = \mathbf{e}_l' E(\prod_{i=1}^{h} \Phi(s_{t+i})|s_t)\mathbf{B}(s_t)\mathbf{e}_k$ *for* $h \geq 1$.

*Proof.* See Appendix 2.C. □

This formula is very similar to the one for computing conventional impulse response functions except the expectation operator, which averages out all possible changes in future regimes. The remaining issue is how to compute the expectation. It is not impossible to write out the expectation term analytically, but it would become very complicated when $h$ is large. One may also simulate the path of $\{s_{t+1},\dots,s_{t+h}\}$ and then take an average of the simulated values of the endogenous variable to obtain the impulse response function. Although this approach should yield a good estimate with a large number of simulations, it is computationally intensive especially when $h$ is large. Therefore we propose a recursive method to solve out the expectation term and estimate the *GIR*.

**Proposition 2.2.** *Let* $\Phi_{s_t}^{(h)} \equiv E(\prod_{i=1}^{h}\Phi(s_{t+i})|s_t)$ *for* $h = 1,2,\dots.$ *We can calculate it recursively as:*

$$\Phi_j^{(h)} = p_{j1}\Phi(1)\Phi_1^{(h-1)} + p_{j2}\Phi(2)\Phi_2^{(h-1)}$$

*where* $\Phi_1^{(0)} = \Phi_2^{(0)} = \mathbf{I}_{16}$ *and* $p_{ji} = p(s_t = i|s_{t-1} = j)$ *for* $i, j = 1,2.$ *The generalized impulse response function can thus be calculated as:*

$$GIR_{t+h|s_t}^{k,l} = \mathbf{e}_l^{\prime}\Phi_{s_t}^{(h)}\mathbf{B}(s_t)\mathbf{e}_k$$

*Proof.* See Appendix 2.D. □

Following Mountford and Uhlig (2009), Fisher and Peters (2010), Uhlig (2010), and Ramey and Zubairy (2018), we calculate the $H-$quarter cumulative government spending multiplier conditional on $s_t$ as:

$$M_{s_t}^{H} = \frac{\sum_{h=0}^{H} GIR_{t+h|s_t}^{N,Y}}{\sum_{h=0}^{H} GIR_{t+h|s_t}^{N,G}} \tag{2.16}$$

The multiplier measures the cumulative change in output relative to the cumulative change in government spending in response to a military news shock in the first $H$ quarters if the shock happens in regime $s_t$. Ramey and Zubairy (2018) argue that this cumulative method produces multipliers that are more relevant for policy purposes than the widely used measure originated by Blanchard and

Perotti (2002) that defines the multiplier as the ratio of the peak response of output to the initial change in government spending. Ramey and Zubairy (2018) also show that the cumulative method tends to result in lower estimates of the multipliers as compared to the Blanchard-Perotti method.

## 2.2 Data and Results

We use U.S. quarterly data from 1890Q1-2015Q4 developed by Ramey and Zubairy (2018). Here we briefly describe the construction of this century-long dataset.[5] The military spending news series ($N_t$) is initially constructed by Ramey (2011) and then extended by Ramey and Zubairy (2018). In order to guarantee that the news series is unanticipated and exogenous, the authors use narrative methods to estimate changes in the expected present discounted value of government spending that are related to military and political events, which are by nature very likely to be independent of the state of the economy. Government spending ($G_t$) is defined as nominal government purchases including all federal, state, and local purchases, but net of transfer payments. Tax revenue ($T_t$) is the nominal value of federal government receipts. Output ($Y_t$) is nominal U.S. GDP. Quarterly data since 1947 is from BEA NIPA. For 1890-1946, historical annual series are interpolated to obtain quarterly series. The other variables, such as the GDP deflator and the unemployment rate, are constructed in similar ways. To construct the real trend GDP, Ramey and Zubairy (2018) use a sixth degree polynomial for the logarithm of GDP, from 1890 to 2015 excluding 1930 through 1946. We multiply real trend GDP by the GDP deflator to get nominal trend GDP and use it to scale variables in our model.

We estimate our model as described in section 2. Table 2.1 shows the estimated reliability of $z_t^{NBER}$ and $z_t^{UNEMP}$. The value of $g_1^{NBER}$ is greater than $g_1^{UNEMP}$ and the value of $g_2^{NBER}$ is greater than $g_2^{UNEMP}$, suggesting that the official business cycle dates are overall a better proxy for $s_t$ than the labor market condition. In Figure 2.1, we plot the time-varying probabilities of occurrence of regime 2. We declare that the economy is in regime 2 if the probability is above some threshold,

[5]Full details can be found in the data appendix of Ramey and Zubairy (2018).

**Table 2.1**: Estimated reliability of the NBER dates and the unemployment rate as proxies for the unobserved regime of the economy

| Parameter | Point estimate | Standard error |
|---|---|---|
| $g_1^{NBER}$ | 0.81 | 0.03 |
| $g_2^{NBER}$ | 0.73 | 0.07 |
| $g_1^{UNEMP}$ | 0.68 | 0.03 |
| $g_2^{UNEMP}$ | 0.54 | 0.07 |

say 0.5, and in regime 1 if otherwise.[6] It is clear that the historical periods when the economy is estimated to be in regime 2 correspond closely to the periods designated as recessions by the NBER. The correlation between our estimated regimes and $z_t^{NBER}$, $z_t^{UNEMP}$ and the real GDP growth rate is 0.48, 0.16 and -0.54, respectively. As a consequence, we interpret regime 1 as expansion and regime 2 as recession without loss of generality. Our estimate of regime 2 misses some NBER recessions such as the one in 1953, 1960, 1970, 1980 and 1990, though. The main reason is that the declines in GDP are mild during these recessions. It is worth noting that our estimated regime 2 includes some periods when the unemployment rate was very high but the real economy was growing, such as a few periods during the Great Depression. Our estimated regime 2 also picks up some war periods such as 1918Q1-1918Q2 and 1943Q1-1943Q3 when the economy was in good shape for sure. This is probably because government purchases are much more stimulative during war years than during tranquil times.

The left panel of Figure 2.2 shows the effects of a military news shock that is normalized to be 1% of potential GDP. We display the generalized impulse responses of government spending, tax, and GDP together with 90% asymptotic confidence intervals. The responses of government spending and GDP are both more muted in expansions than in recessions. The right panel of the same figure depicts the gaps in impulse responses between the recession regime and the expansion regime. As can be seen, the gaps are statistically significant.

Table 2.2 shows our estimates of the government spending multipliers at various horizons

[6]Any reasonable choice of the threshold does not affect our result.

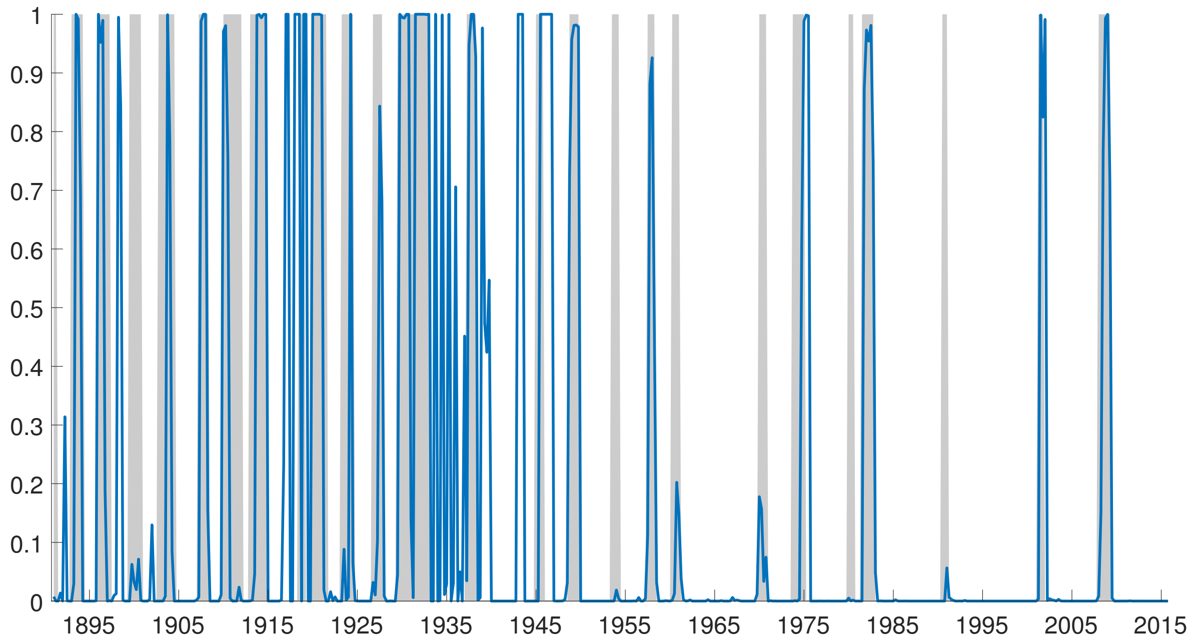


**Figure 2.1**: Smoothed probability of occurrence of regime 2

Note: The shaded region represents the NBER recession dates. The solid line shows the estimated time-varying probabilities of regime 2.

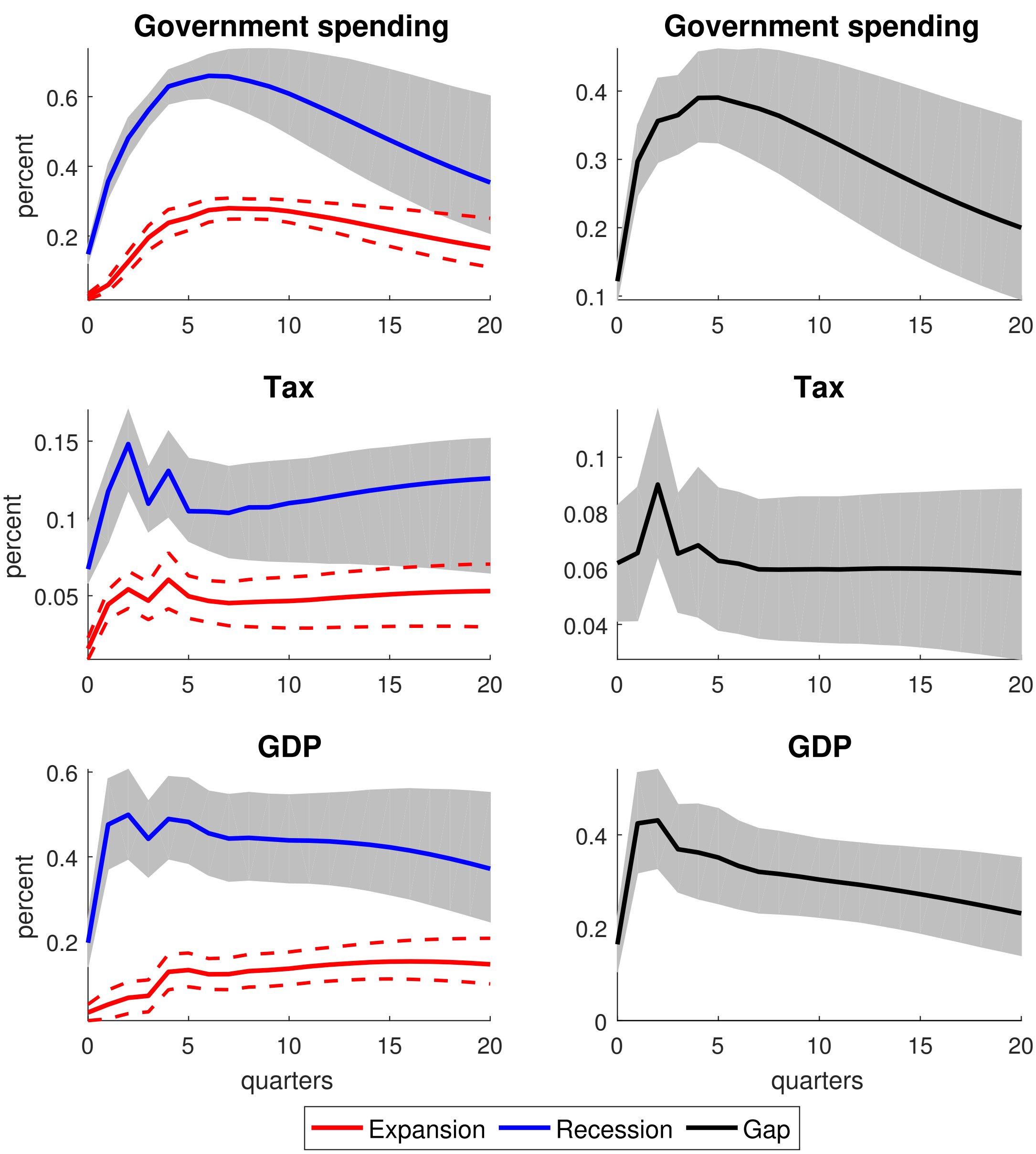


**Figure 2.2**: Generalized impulse responses (% of potential GDP) of government spending, tax, and GDP to a military news shock for expansion and recession, and differences in impulse responses between the two regimes

Note: The left panel shows the generalized impulse responses of government spending, tax, and GDP to a military news shock that is normalized to be 1% of potential GDP. The red solid lines are impulse responses for regime 1 (expansion) and the blue solid lines are impulse responses for regime 2 (recession). The right panel shows differences in impulse responses between the two regimes. The shaded area and dashed lines represent the 90% asymptotic confidence intervals.

**Table 2.2**: Estimates of $H-$quarter cumulative government spending multipliers for expansion and recession

| Horizon | Expansion | Recession | Gap |
|---|---|---|---|
| H = 4 | 0.55 | 0.97 | 0.42 |
| | [0.35, 0.74] | [0.78, 1.18] | [0.16, 0.71] |
| H = 8 | 0.50 | 0.82 | 0.32 |
| | [0.36, 0.63] | [0.66, 1.01] | [0.14, 0.53] |
| H = 12 | 0.51 | 0.79 | 0.29 |
| | [0.38, 0.63] | [0.63, 0.98] | [0.14, 0.46] |
| H = 16 | 0.55 | 0.81 | 0.26 |
| | [0.42, 0.68] | [0.64, 1.01] | [0.13, 0.43] |
| H = 20 | 0.59 | 0.84 | 0.24 |
| | [0.45, 0.74] | [0.65, 1.06] | [0.12, 0.40] |

Note: The results are obtained from the MS-SVAR model where both the NBER dates and the unemployment rate are used for inference about the unobserved regime of the economy. The values in brackets give the 90% confidence intervals.

for different regimes. The first thing we should note is that the multipliers are always smaller than 1, suggesting that government purchases always crowd out private demand to some extent. The multiplier values are stable over horizons. If the shock happens during an expansion, the multiplier values are around 0.55. If the shock happens during a recession, the multiplier values are around 0.9. The 1-year multiplier for recession is very close to 1 and is almost twice as large as the 1-year multiplier for expansion. The second thing to note is that the gaps in the multipliers between recessions and expansions are statistically significant, although it is not obvious whether the gaps are economically significant. Our results indicate that the differences in the effects of government spending between good and bad times may not be as large as some previous studies estimate.

## 2.3 Reconciling Estimates of the Government Spending Multipliers

Our estimates of the government spending multipliers challenge some existing results in the literature. For example, Auerbach and Gorodnichenko (2012a) obtain multipliers as high as 2.24 for recession, whereas we do not observe any multiplier value that is greater than 1. Moreover, our finding of the state dependence of the government spending multipliers is at odds with the extensive evidence provided by Ramey and Zubairy (2018) that the multipliers do not vary with the amount of slack in the economy, although we use the same dataset. In this section, we relate our empirical strategy to these two studies, and explain why we reach different conclusions.

### 2.3.1 Auerbach and Gorodnichenko (2012a)

The pioneering paper by Auerbach and Gorodnichenko (2012a) (AG hereafter) estimates a smooth-transition structural VAR to measure the asymmetric effects of government spending shocks over the business cycle. Although their paper is different from ours in many aspects, such as model specification, identification, and data used, we reach the same conclusion that government spending is more effective in recessions as compared to in expansions.

Nevertheless, AG differ from us in the magnitudes of the multipliers. At the 5-year horizon, AG's estimate of the multiplier for recession is 2.24 whereas our estimate is 0.84. To explain the difference, we focus on the different methods for constructing impulse response functions employed by AG and us. In AG, the authors assume that the economy remains in its current regime throughout the duration of the responses to a government spending shock. In other words, AG estimate the regime-dependent impulse response functions instead of the generalized impulse response functions. Ramey and Zubairy (2018) point out that AG's estimate of the multiplier for recession grows as the horizon grows because their constructed impulse response for GDP keeps increasing while government spending does not. The reason why the response of GDP is so unusual is that AG assumes the economy to remain in recession, and people should forecast future output growth to be higher than current growth during recessions. To verify this explanation, Ramey and Zubairy (2018) estimate AG's model, but allow the regime of the economy to switch endogenously with respect to the history of both the government spending shocks and the nongovernment spending shocks when converting the model parameters into impulse response functions. They find that the multipliers in recessions are around 1, which are very close to our results.

We add to the evidence provided by Ramey and Zubairy (2018) by calculating what the multipliers would be for our maximum likelihood estimates if we were to prohibit the regime from switching. Figure 2.3 shows the result. Under the assumption of non-changing regimes, we obtain much larger multipliers for recession than our benchmark result. The multiplier grows with the horizon and becomes 1.9 after five years, which is in line with AG's finding. We conclude that previous studies constructing the government spending multipliers based on the regime-dependent impulse response functions tend to overestimate the average effects of government purchases in recessions.

### 2.3.2 Ramey and Zubairy (2018)

Ramey and Zubairy (2018) (RZ hereafter) argue that there are no differences in the effects of government spending shocks between slack regimes and non-slack regimes. Instead of using a structural VAR, they adapt the local projection method proposed by Jordà (2005) to estimate state-dependent impulse responses of government spending and GDP to military news shocks. They estimate the 2-year and 4-year cumulative multipliers to be around 0.6 in both slack periods and non-slack periods.

In line with RZ, we find that the government spending multipliers are always smaller than 1 regardless of the regime of the economy. However, in contrast to RZ, we find statistically significant evidence that the effects of government spending are stronger in recessions than in expansions. We believe the main reason why RZ fail to observe statistically significant variations in the multipliers is

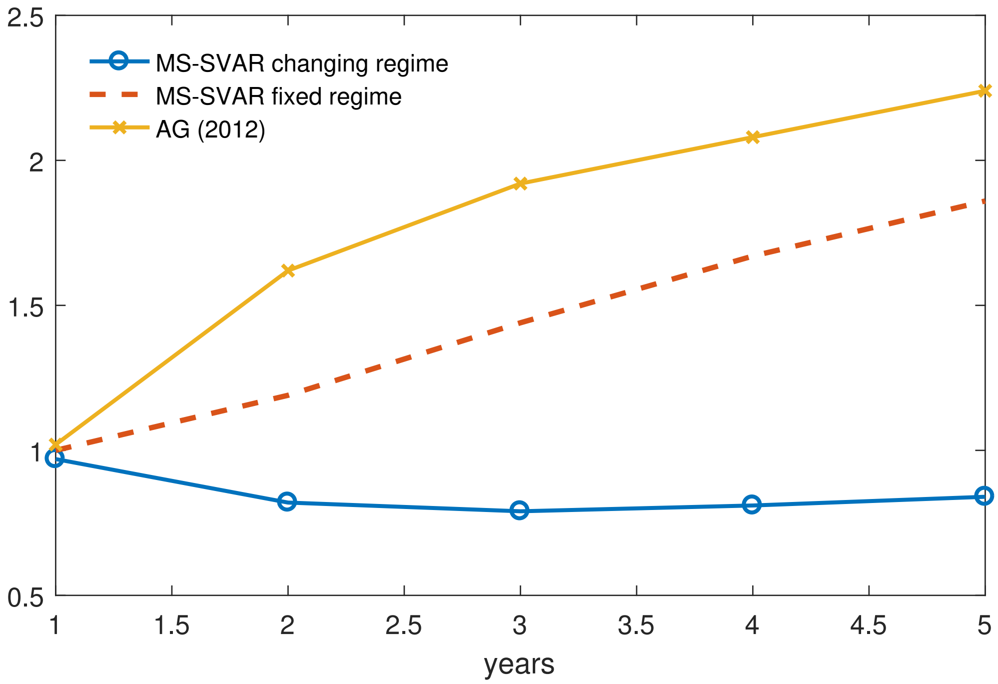


**Figure 2.3**: Cumulative government spending multipliers for the recession regime for alternative specifications

Note: This figure shows cumulative government spending multipliers for the recession regime at various horizons. The blue line with circles shows the multipliers obtained from our MS-SVAR model that allows the regime to change. The dashed line shows the multipliers obtained from our MS-SVAR model assuming a fixed regime of the economy after the shock. The yellow line with x-marks shows the multipliers obtained by Auerbach and Gorodnichenko (2012a).

that the local projection method they use to construct impulse response functions (and the government spending multipliers) is too inefficient to generate precise estimates. Essentially this is a multiperiod direct forecasting method that is subject to efficiency loss relative to the VAR method based on one-period-ahead forecasting. As a result, one may not be able to reject the null hypothesis of constant government spending multipliers using the local projection method, even if the multipliers are truly state-dependent.

RZ show that their conclusion is robust no matter whether we use the NBER business cycle dates or the unemployment rate to measure slack and non-slack regimes. We are not surprised that RZ do not find variations in the government spending multipliers across times of high unemployment and times of low unemployment because we have shown that the labor market condition is not a reliable indicator of the regime of the economy across which the multipliers differ. Nevertheless, we do expect to see some variations in the multipliers across official business cycle phases because our estimated regimes have a high correlation with the NBER dates. To verify that, we estimate our MS-SVAR model using only $z_t^{NBER}$ or $z_t^{UNEMP}$ for inference.[7] First, we assume that the NBER dates are a perfect measure for the regime of the economy, namely $s_t = z_t^{NBER}$. Table 2.3 shows the result. While the multipliers in expansions are always smaller than 1, the multipliers in recessions are always larger than 1. The differences in the multipliers between recessions and expansions are statistically significant at the 1-year horizon and the 2-year horizon. Next we assume that the unemployment rate-based indicator variable is a perfect measure for the regime, namely $s_t = z_t^{UNEMP}$. In this case, the multiplier is always smaller than 1 irrespective of the regime or horizon. The multipliers in slack periods and non-slack periods are very similar. The gaps in the multipliers are negligible and statistically insignificant. The magnitudes of the multipliers are also close to RZ's estimates.

To illustrate that the inefficiency of the local projection method explains why RZ fail to observe differences in the government spending multipliers when they use the NBER dates to measure the regime of the economy, we show what the multipliers would be if we employ the local

[7] In this experiment, the Markov transition probabilities are not identified in the model. We calibrate the values of the transition probabilities using the series of $z_t^{NBER}$ or $z_t^{UNEMP}$.

**Table 2.3**: Estimates of state-dependent $H-$quarter cumulative government spending multipliers: regime measured by the NBER dates or the unemployment rate

| | NBER dates | | |
|---|---|---|---|
| Horizon | Expansion | Recession | Gap |
| H = 4 | 0.38 | 1.20 | 0.82 |
| | [0.05, 0.71] | [0.33, 2.37] | [0.07, 1.84] |
| H = 8 | 0.34 | 1.17 | 0.83 |
| | [-0.07, 0.74] | [0.09, 2.60] | [0.01, 2.05] |
| H = 12 | 0.30 | 1.12 | 0.83 |
| | [-0.17, 0.74] | [-0.11, 2.75] | [-0.09, 2.20] |
| H = 16 | 0.25 | 1.08 | 0.83 |
| | [-0.25, 0.74] | [-0.25, 2.86] | [-0.15, 2.34] |
| H = 20 | 0.20 | 1.04 | 0.83 |
| | [-0.34, 0.72] | [-0.37, 2.94] | [-0.18, 2.42] |
| | Unemployment rate | | |
| Horizon | Low | High | Gap |
| H = 4 | 0.50 | 0.50 | 0.00 |
| | [-0.15, 1.13] | [0.23, 0.77] | [-0.66, 0.72] |
| H = 8 | 0.47 | 0.47 | 0.00 |
| | [-0.14, 1.11] | [0.24, 0.72] | [-0.58, 0.57] |
| H = 12 | 0.40 | 0.45 | 0.05 |
| | [-0.33, 1.17] | [0.21, 0.70] | [-0.61, 0.66] |
| H = 16 | 0.36 | 0.43 | 0.06 |
| | [-0.36, 1.20] | [0.18, 0.70] | [-0.60, 0.65] |
| H = 20 | 0.35 | 0.41 | 0.06 |
| | [-0.35, 1.17] | [0.14, 0.71] | [-0.57, 0.62] |

Note: The results are obtained from the MS-SVAR model where the regime is measured precisely by the NBER dates or the unemployment rate. The values in brackets give the 90% confidence intervals.

**Table 2.4**: Local projection estimates of $H-$quarter cumulative government spending multipliers for expansion and recession

| Horizon | Expansion | Recession | Gap |
|---|---|---|---|
| H = 4 | 0.50 | 0.52 | 0.03 |
| | [0.24, 0.75] | [-0.28, 1.33] | [-0.83, 0.88] |
| H = 8 | 0.58 | 0.11 | -0.47 |
| | [0.43, 0.72] | [-1.15, 1.37] | [-1.79, 0.85] |
| H = 12 | 0.66 | -0.46 | -1.12 |
| | [0.56, 0.76] | [-2.39, 1.46] | [-3.08, 0.84] |
| H = 16 | 0.66 | -0.74 | -1.40 |
| | [0.55, 0.76] | [-3.07, 1.59] | [-3.76, 0.97] |
| H = 20 | 0.69 | -0.74 | -1.43 |
| | [0.57, 0.82] | [-3.00, 1.53] | [-3.74, 0.88] |

Note: The results are obtained from the local projection estimation. The NBER dates are used as the indicator of the regime of the economy. The values in brackets give the HAC-robust 90% confidence intervals.

projection method for estimation. Following RZ, we estimate the $h-$period cumulative government spending multiplier by regressing the sum of GDP from $t$ to $t+h$ on the sum of government spending from $t$ to $t+h$ and control variables $N_{t-j}, G_{t-j}, T_{t-j}, Y_{t-j}$ $(j = 1, \cdots, 4)$, using the military spending news variable $N_t$ as an instrument for the sum of government spending. To allow for state-dependence, the values of the regression coefficients are postulated to depend on the NBER dates. Table 2.4 shows the result. Two things are worth noting. First, the multipliers in recessions are estimated to be negative, which is hard to interpret. The second thing to note is that the estimates are very imprecise. According to the confidence intervals reported, we are unable to reject the null hypothesis that the multipliers in expansions and recessions are equal. However, due to the large standard errors associated with the point estimates, we are also unable to reject the null that the gaps in the multipliers between expansions and recessions equal our estimates based on the MS-SVAR, as shown in Table 2.3.

## 2.4 Time-varying Transition Probabilities

A key feature of our MS-SVAR model is that the regime transition probabilities are constant. This assumption makes the model more tractable. However, one may think that the regime of the economy is likely to be affected by fiscal policy shocks. Presumably a positive government spending shock can help the economy escape from recession, while a negative government spending shock may end an expansion early. Therefore a natural generalization of our model is to allow the transition probabilities to depend on the values of fiscal shocks. In this section, we first test if the assumption of constant transition probabilities is reasonable, and then check the robustness of our result with the assumption relaxed.

There are many ways to specify time-varying transition probabilities, and we consider a simple one. Motivated by Diebold et al. (1994) and Kim and Nelson (1999), we assume that the state variable $s_t$ has a logit specification:

$$s_t = \begin{cases} 1 & \text{if } s_t^* < 0 \\ 2 & \text{if } s_t^* \geq 0 \end{cases} \tag{2.17}$$

where $s_t^*$ is a latent variable defined by:

$$s_t^* = a_0 + a_1 \delta_{2,t-1} + \gamma N_{t-1} + \eta_t \tag{2.18}$$

$\delta_{2,t-1} = 1$ if $s_{t-1} = 2$ and 0 otherwise. $N_{t-1}$ is the lagged military spending news shock (divided by potential GDP), and $\eta_t$ follows a standard logistic distribution.[8] The transition probabilities are then

[8] We assume that the regime of the economy is realized at the beginning of each period, so the military news shock affects the regime of the next period.

**Table 2.5**: Estimates of parameters in the logit specification of $s_t$

| Parameter | Point estimate | Standard error |
|---|---|---|
| $a_0$ | -2.51 | 0.20 |
| $a_1$ | 3.16 | 0.32 |
| $\gamma$ | 0.11 | 2.57 |

time-varying:

$$p_{11,t} \equiv p(s_{t+1} = 1 | s_t = 1, N_t) = \frac{1}{1 + \exp(a_0 + \gamma N_t)} \tag{2.19}$$

$$p_{22,t} \equiv p(s_{t+1} = 2 | s_t = 2, N_t) = \frac{\exp(a_0 + a_1 + \gamma N_t)}{1 + \exp(a_0 + a_1 + \gamma N_t)} \tag{2.20}$$

If $\gamma = 0$, the transition probabilities become time-invariant and the model collapses to our benchmark model in section 2. Therefore we can test the assumption of constant transition probabilities by the significance of $\gamma$.

We estimate our MS-SVAR adapted for time-varying transition probabilities as specified above. Table 2.5 shows the estimates of the parameters in equation (2.19) and (2.20) along with their standard errors. The null hypothesis $\gamma = 0$ can not be rejected, which implies that the regime of the economy is not likely to be affected by fiscal policy shocks and thus justifies our assumption of constant regime transition probabilities.

Next we show that time variation in the transition probabilities has a negligible effect on our estimates of the fiscal multipliers. If the military spending news shock affects the regime of the following period, the government spending multipliers are supposed to be dependent on the value of the shock. To illustrate, we consider two extreme cases. In the first case, the military spending news shock takes the maximum value over our sample period, which was seen in 1941Q4 due to the direct involvement of the U.S. in WWII. In the second case, the military spending news shock takes the minimum value over our sample period, which was seen in 1945Q3 due to the ending of WWII.

**Table 2.6**: Estimates of state-dependent $H-$quarter cumulative multipliers for two extreme cases: allowing for time-varying transition probabilities

| | Shock=$max_t N_t$ | | | Shock=$min_t N_t$ | | |
|---|---|---|---|---|---|---|
| Horizon | Expansion | Recession | Gap | Expansion | Recession | Gap |
| H = 4 | 0.63 | 1.04 | 0.41 | 0.68 | 1.13 | 0.48 |
| H = 8 | 0.86 | 1.33 | $0.47^{*}$ | 0.89 | 1.39 | $0.53^{*}$ |
| H = 12 | 0.90 | 1.44 | $0.54^{*}$ | 0.92 | 1.48 | $0.59^{*}$ |
| H = 16 | 0.90 | 1.49 | $0.58^{**}$ | 0.92 | 1.52 | $0.63^{**}$ |
| H = 20 | 0.89 | 1.51 | $0.62^{**}$ | 0.91 | 1.54 | $0.67^{**}$ |

Note: We estimate our MS-SVAR model allowing for time-varying transition probabilities, and estimate the state-dependent government spending multipliers for two extreme cases. In the first case, the military news shock takes the maximum value over our sample period. Column 2 through 4 show the results. In the second case, the military news shock takes the minimum value over our sample period. Column 5 through 7 show the results. The asterisks on the fourth and last entry in each row represent significance level: $^{*}p < 0.10$, $^{**}p < 0.05$.

Table 2.6 reports the estimated government spending multipliers for both cases. As can be seen, the multipliers are very similar to our benchmark results in Table 2.2. Therefore our conclusion is robust to time variation in the regime transition probabilities.

## 2.5 Conclusion

This paper proposes a Markov regime-switching structural VAR to study the state-dependent effects of government spending shocks on aggregate output. Using a U.S. dataset, we find that the government spending multipliers are statistically larger in recessions than in expansions, which confirms the existing evidence that fiscal policy is more effective during bad times. However, the multipliers are always smaller than 1, and the differences between recessions and expansions are not as large as some previous studies have claimed.

Our model has two distinctive features that enable us to understand why previous studies reach different conclusions. First, we combine quantitative data and qualitative indicators to infer the regimes across which the government spending multipliers differ. Second, we propose a recursive method to estimate the dynamic effects of a government spending shock that allows the regime of the economy to change after the shock. We show that if we prohibit the regime from switching, we will obtain much larger multipliers for recession. This explains why Auerbach and Gorodnichenko (2012a), who assume the economy to remain in its current regime indefinitely, obtain very large multipliers in recessions. Moreover, we argue that the main reason why Ramey and Zubairy (2018) do not find statistically significant differences in the government spending multipliers between good and bad times is that their estimation strategy is inefficient. We show that their method produces much less precise estimates than ours.

## 2.6 Acknowledgement

Chapter 2 is coauthored with Yifei Lyu. The dissertation author was the primary author of this chapter. And this chapter, in full is currently begin prepared for submission for publication of the material.

## Appendix 2.A Algorithm for Estimation and inference

The expectation maximization (EM) algorithm is introduced in Hamilton (1990) for obtaining maximum likelihood estimates of parameters in Markov regime-switching models. To implement the EM algorithm in our application, we only need to evaluate the smoothed probability $p(s_t|\mathcal{W}_T;\lambda)$ for $t = 1,2\ldots,T$. Given old estimates of parameters $\lambda$, we calculate smoothed probabilities and use them to reweight the observed data. Then some simple calculations based on the weighted data generate new estimates of $\lambda$. The likelihood value increases after each such iteration, and iteration continues until a fixed point for $\lambda$ is obtained.

**Calculating smoothed probabilities** Given estimates $\hat{\lambda}$, we first calculate the filtered probability $p(s_t|\mathcal{W}_t;\hat{\lambda})$ for $t = 1,2,\ldots,T$ using the Hamilton filter introduced in Hamilton (1989) as the basis for calculating smoothed probabilities. The Hamilton filter accepts as input $p(s_{t-1}|\mathcal{W}_{t-1};\hat{\lambda})$ and produces $p(s_t|\mathcal{W}_t;\hat{\lambda})$. It runs as follows. First, we calculate

$$p(s_t, s_{t-1}|\mathcal{W}_{t-1};\hat{\lambda}) = p(s_t|s_{t-1})p(s_{t-1}|\mathcal{W}_{t-1};\hat{\lambda}) \tag{2.21}$$

Then we calculate

$$f(\mathbf{w}_t|\mathcal{W}_{t-1};\hat{\lambda}) = \sum_{s_t=1}^{2}\sum_{s_{t-1}=1}^{2} f(\mathbf{w}_t|\mathbf{x}_t, s_t;\hat{\lambda})p(s_t, s_{t-1}|\mathcal{W}_{t-1};\hat{\lambda}) \tag{2.22}$$

where $f(\mathbf{w}_t|\mathbf{x}_t, s_t;\hat{\lambda})$ can be found in equation (2.9). Finally, the output is obtained from

$$p(s_t|\mathcal{W}_t;\hat{\lambda}) = \sum_{s_{t-1}=1}^{2} \frac{f(\mathbf{w}_t|\mathbf{x}_t, s_t;\hat{\lambda})p(s_t, s_{t-1}|\mathcal{W}_{t-1};\hat{\lambda})}{f(\mathbf{w}_t|\mathcal{W}_{t-1};\hat{\lambda})} \tag{2.23}$$

After calculating filtered probabilities, we can calculate smoothed probabilities according to Hamilton (1990). Note that for any $\tau > t$, we have

$$p(s_\tau, s_t|\mathcal{W}_\tau;\hat{\lambda}) = \sum_{s_{\tau-1}=1}^{2} \frac{p(s_\tau|s_{\tau-1})f(\mathbf{w}_\tau|\mathbf{x}_\tau, s_\tau;\hat{\lambda})p(s_{\tau-1}, s_t|\mathcal{W}_{\tau-1};\hat{\lambda})}{f(\mathbf{w}_\tau|\mathcal{W}_{\tau-1};\hat{\lambda})} \tag{2.24}$$

When $\tau = t$, $p(s_\tau, s_t|\mathcal{W}_\tau;\hat{\lambda})$ equals the filtered probability $p(s_t|\mathcal{W}_t;\hat{\lambda})$. For any $t$, we can start from the filtered probability and then iterate on the above expression for $\tau = t+1, t+2, \ldots, T$. The smoothed probability is finally obtained from

$$p(s_t|\mathcal{W}_T;\hat{\lambda}) = \sum_{s_T=1}^{2} p(s_T, s_t|\mathcal{W}_T;\hat{\lambda}) \tag{2.25}$$

**Updating parameters** The EM algorithm solves a sequence of optimization problems (indexed by $l = 0, 1, \ldots$). Let $Q(\lambda_{l+1}; \lambda_l, \mathcal{W}_T)$ denote the expected log-likelihood:

$$Q(\lambda_{l+1}; \lambda_l, \mathcal{W}_T) = \int_{\mathscr{S}} \log p(\mathcal{W}_T, \mathscr{S}; \lambda_{l+1}) \cdot p(\mathcal{W}_T, \mathscr{S}; \lambda_l) \tag{2.26}$$

where $\mathscr{S} = \{s_T, s_{T-1}, \ldots, s_1\}$. Let $\hat{\lambda}_l$ denote estimates of parameters from our previous iteration. We choose as $\hat{\lambda}_{l+1}$ the value of $\lambda_{l+1}$ that maximizes $Q(\lambda_{l+1}; \hat{\lambda}_l, \mathcal{W}_T)$. Hamilton (1990) proves that $\hat{\lambda}_{l+1}$ is associated with a higher likelihood value than is $\hat{\lambda}_l$, so the sequence of $\{\hat{\lambda}_l\}_{l=0}^{\infty}$ converges to a local MLE. In our application, given $\hat{\lambda}_l$, we run the iteration by first scaling the observed sample data by the square root of smoothed probabilities

$$\mathbf{y}_t^{(l+1,j)} = \mathbf{y}_t \cdot \sqrt{p(s_t = j | \mathcal{W}_T; \hat{\lambda}_l)} \qquad j = 1, 2$$
$$\mathbf{x}_t^{(l+1,j)} = \mathbf{x}_t \cdot \sqrt{p(s_t = j | \mathcal{W}_T; \hat{\lambda}_l)} \qquad j = 1, 2$$

Then simple OLS regressions based on $\mathbf{y}_t^{(l+1,j)}$ and $\mathbf{x}_t^{(l+1,j)}$ yield new estimates $\hat{\mathbf{A}}_0^{(l+1)}(j)$, $\hat{\beta}^{(l+1)}(j)$ and $\hat{\Sigma}^{(l+1)}$. The remaining new parameters are calculated as

$$p_{jj}^{(l+1)} = \frac{\sum_{t=2}^{T} p(s_t = j, s_{t-1} = j | \mathcal{W}_T; \hat{\lambda}_l)}{\sum_{t=2}^{T} p(s_{t-1} = j | \mathcal{W}_T; \hat{\lambda}_l)} \qquad j = 1, 2 \tag{2.27}$$

$$g_1^{NBER}(l+1) = \frac{\sum_{t=1}^{T} (2 - z_t^{NBER}) p(s_t = 1 | \mathcal{W}_T; \hat{\lambda}_l)}{\sum_{t=1}^{T} p(s_t = 1 | \mathcal{W}_T; \hat{\lambda}_l)} \tag{2.28}$$

$$g_2^{NBER}(l+1) = \frac{\sum_{t=1}^{T} (z_t^{NBER} - 1) p(s_t = 2 | \mathcal{W}_T; \hat{\lambda}_l)}{\sum_{t=1}^{T} p(s_t = 2 | \mathcal{W}_T; \hat{\lambda}_l)} \tag{2.29}$$

$$g_1^{UNEMP}(l+1) = \frac{\sum_{t=1}^{T} (2 - z_t^{UNEMP}) p(s_t = 1 | \mathcal{W}_T; \hat{\lambda}_l)}{\sum_{t=1}^{T} p(s_t = 1 | \mathcal{W}_T; \hat{\lambda}_l)} \tag{2.30}$$

$$g_2^{UNEMP}(l+1) = \frac{\sum_{t=1}^{T} (z_t^{UNEMP} - 1) p(s_t = 2 | \mathcal{W}_T; \hat{\lambda}_l)}{\sum_{t=1}^{T} p(s_t = 2 | \mathcal{W}_T; \hat{\lambda}_l)} \tag{2.31}$$

It is worth mentioning that the EM algorithm enables us to find a local maximum of the likelihood function, however, the global maximum is not guaranteed. We explore many different starting values $\hat{\lambda}_0$, and select the local maximum that delivers the highest likelihood value. To confirm that we have reached the global maximum, we use the simulated annealing (SA) algorithm, a widely used global optimization algorithm, to search for the MLE as an alternative. We obtain the same result using the EM algorithm and the SA algorithm. To construct asymptotic standard errors, we numerically calculate second derivatives of the log likelihood.

## Appendix 2.B Companion form of the MS-SVAR

Because the news variable $N_t$ by construction is unforecastable, we can augment the vector of endogenous variables $\mathbf{y}_t$ with $N_t$ and rewrite equation (2.1) as:

$$\mathbf{A}_0^*(s_t)\mathbf{y}_t^* = \mathbf{c}^*(s_t) + \sum_{j=1}^{4} \mathbf{A}_j^*(s_t)\mathbf{y}_{t-j}^* + \varepsilon_t^* \tag{2.32}$$

where $\mathbf{y}_t^* = (N_t, G_t, T_t, Y_t)'$, $\mathbf{c}^*(s_t) = (c^N, \mathbf{c}(s_t)')'$, $\varepsilon_t^* = (\varepsilon_t^N, \varepsilon_t')'$, and

$$\mathbf{A}_0^*(s_t) = \begin{pmatrix} 1 & \mathbf{0} \\ -\Gamma_0(s_t) & \mathbf{A}_0(s_t) \end{pmatrix}$$

$$\mathbf{A}_j^*(s_t) = \begin{pmatrix} 0 & \mathbf{0} \\ \Gamma_j(s_t) & \mathbf{A}_j(s_t) \end{pmatrix}$$

for $j = 1, \ldots, 4$. $c^N$ is the mean of the military spending news that is assumed to be constant over time. $\varepsilon_t^N$ is the shock to the military spending news. With both sides premultiplied by $\mathbf{A}_0^*(s_t)^{-1}$, equation (2.32) becomes:

$$\mathbf{y}_t^* = \mathbf{A}_0^*(s_t)^{-1}\mathbf{c}^*(s_t) + \sum_{j=1}^{4} \mathbf{A}_0^*(s_t)^{-1}\mathbf{A}_j^*(s_t)\mathbf{y}_{t-j}^* + \mathbf{A}_0^*(s_t)^{-1}\varepsilon_t^* \tag{2.33}$$

Then we can express this VAR(4) system in companion form:

$$\tilde{\mathbf{y}}_t = \tilde{\mathbf{c}}(s_t) + \Phi(s_t)\tilde{\mathbf{y}}_{t-1} + \mathbf{B}(s_t)\tilde{\varepsilon}_t \tag{2.34}$$

where $\tilde{\mathbf{y}}_t = (\mathbf{y}_t^{*'}, \mathbf{y}_{t-1}^{*'}, \mathbf{y}_{t-2}^{*'}, \mathbf{y}_{t-3}^{*'})'$, $\tilde{\mathbf{c}}(s_t) = ((\mathbf{A}_0^*(s_t)^{-1}\mathbf{c}^*(s_t))', \mathbf{0}_{1\times 12})'$, $\tilde{\varepsilon}_t = (\varepsilon_t^{*'}, \mathbf{0}_{1\times 12})'$, and

$$\Phi(s_t) = \begin{pmatrix} \mathbf{A}_0^*(s_t)^{-1}\mathbf{A}_1^*(s_t) & \mathbf{A}_0^*(s_t)^{-1}\mathbf{A}_2^*(s_t) & \mathbf{A}_0^*(s_t)^{-1}\mathbf{A}_3^*(s_t) & \mathbf{A}_0^*(s_t)^{-1}\mathbf{A}_4^*(s_t) \\ \mathbf{I} & \mathbf{0} & \mathbf{0} & \mathbf{0} \\ \mathbf{0} & \mathbf{I} & \mathbf{0} & \mathbf{0} \\ \mathbf{0} & \mathbf{0} & \mathbf{I} & \mathbf{0} \end{pmatrix}$$

$$\mathbf{B}(s_t) = \begin{pmatrix} \mathbf{A}_0^*(s_t)^{-1} & \mathbf{0} \\ \mathbf{0} & \mathbf{0} \end{pmatrix}$$

# Appendix 2.C Proof of Proposition 1

Starting from the companion form

$$\tilde{\mathbf{y}}_t = \tilde{\mathbf{c}}(s_t) + \Phi(s_t)\tilde{\mathbf{y}}_{t-1} + \mathbf{B}(s_t)\tilde{\varepsilon}_t \tag{2.35}$$

we obtain the following expression for $h \geq 1$ using recursive method:

$$\tilde{\mathbf{y}}_{t+h} = \tilde{\mathbf{c}}_{t+1,t+h} + \left(\prod_{i=1}^{h}\Phi(s_{t+i})\right)(\tilde{\mathbf{c}}(s_t) + \Phi(s_t)\tilde{\mathbf{y}}_{t-1} + \mathbf{B}(s_t)\tilde{\varepsilon}_t) + \mathbf{u}_{t+1,t+h} \tag{2.36}$$

where

$$\tilde{\mathbf{c}}_{t+1,t+h} = \begin{cases} \tilde{\mathbf{c}}(s_{t+h}) + \sum_{m=1}^{h-1}\left(\prod_{j=0}^{m-1}\Phi(s_{t+h-j})\right)\tilde{\mathbf{c}}(s_{t+h-m}) & \text{if } h \geq 2 \\ \tilde{\mathbf{c}}(s_{t+h}) & \text{if } h = 1 \end{cases}$$

and

$$\mathbf{u}_{t+1,t+h} = \begin{cases} \mathbf{B}(s_{t+h})\tilde{\varepsilon}_{t+h} + \sum_{m=1}^{h-1} \left(\prod_{j=0}^{m-1} \Phi(s_{t+h-j})\right) \mathbf{B}(s_{t+h-m})\tilde{\varepsilon}_{t+h-m} & \text{if } h \geq 2 \\ \mathbf{B}(s_{t+h})\tilde{\varepsilon}_{t+h} & \text{if } h = 1 \end{cases}$$

Note that given a path of future regimes $\{s_{t+1}, s_{t+2}, \cdots, s_{t+h}\}$, $\mathbf{u}_{t+1,t+h}$ has zero mean:

$$E\left(\mathbf{u}_{t+1,t+h} \middle| s_{t+1}, s_{t+2}, \cdots, s_{t+h}\right) = \mathbf{0} \tag{2.37}$$

The conditional expectation of $\tilde{y}_{l,t+h}$ is:

$$E\left(\tilde{y}_{l,t+h} \middle| \tilde{\varepsilon}_t, s_t\right) = \mathbf{e}_l' E\left(E\left(\tilde{\mathbf{y}}_{t+h} \middle| \tilde{\varepsilon}_t, s_t, s_{t+1}, \cdots, s_{t+h}\right) \middle| \tilde{\varepsilon}_t, s_t\right) \tag{2.38}$$

where $\mathbf{e}_l$ is the $l$th column of $\mathbf{I}_{16}$. Combining 2.36-2.38, we have

$$E\left(\tilde{y}_{l,t+h} \middle| \tilde{\varepsilon}_t, s_t\right) = \mathbf{e}_l' E\left(\tilde{\mathbf{c}}_{t+1,t+h} \middle| s_t\right) + \mathbf{e}_l' E\left(\prod_{i=1}^{h} \Phi(s_{t+i}) \middle| s_t\right) \left(\tilde{\mathbf{c}}(s_t) + \Phi(s_t)\tilde{\mathbf{y}}_{t-1} + \mathbf{B}(s_t)\tilde{\varepsilon}_t\right)$$

which leads to proposition 1 in the main text:

$$\begin{aligned} GIR_{t+h|s_t}^{k,l} &\equiv E\left(\tilde{y}_{l,t+h} \middle| \tilde{\varepsilon}_t = \mathbf{e}_k, s_t\right) - E\left(\tilde{y}_{l,t+h} \middle| \tilde{\varepsilon}_t = \mathbf{0}, s_t\right) \\ &= \mathbf{e}_l' E\left(\prod_{i=1}^{h} \Phi(s_{t+i}) \middle| s_t\right) \mathbf{B}(s_t)\mathbf{e}_k. \end{aligned}$$

# Appendix 2.D Proof of Proposition 2

We define $\Phi_j^{(h)} \equiv E\left(\prod_{i=1}^{h} \Phi(s_{t+i}) \middle| s_t = j\right)$, so

$$\begin{aligned}
\Phi_j^{(h)} &= E\left\{ E\left( \prod_{i=1}^{h} \Phi(s_{t+i}) \middle| s_{t+1} \right) \middle| s_t = j \right\} \\
&= E\left\{ \Phi(s_{t+1})\, E\left( \prod_{i=1}^{h-1} \Phi(s_{t+1+i}) \middle| s_{t+1} \right) \middle| s_t = j \right\} \\
&= E\left( \Phi(s_{t+1}) \Phi_{s_{t+1}}^{(h-1)} | s_t = j \right) \\
&= p(s_{t+1} = 1 | s_t = j) \Phi(1) \Phi_1^{(h-1)} + p(s_{t+1} = 2 | s_t = j) \Phi(2) \Phi_2^{(h-1)} \\
&= p_{j1} \Phi(1) \Phi_1^{(h-1)} + p_{j2} \Phi(2) \Phi_2^{(h-1)}
\end{aligned}$$

# Reference


Aastveit, K. A., Bjørnland, H. C., and Thorsrud, L. A. (2015). What drives oil prices? emerging versus developed economies. *Journal of Applied Econometrics*, 30(7):1013–1028.

Alloza, M. (2016). Is fiscal policy more effective in uncertain times or during recessions? Working Paper.

Auerbach, A. J. and Gorodnichenko, Y. (2012a). 20120930measuring the output responses to fiscal policy. *American Economic Journal: Economic Policy*, 4(2):1–27.

Auerbach, A. J. and Gorodnichenko, Y. (2012b). 20121230fiscal multipliers in recession and expansion. In *Fiscal Policy after the Financial crisis*, pages 63–98. University of Chicago press.

Bachmann, R. and Sims, E. R. (2012). Confidence and the transmission of government spending shocks. *Journal of Monetary Economics*, 59(3):235–249.

Balke, N. S., Brown, S. P., and Yücel, M. K. (2002). Oil price shocks and the u.s. economy: Where does the asymmetry originate? *Energy Journal*, 27(3):27–52.

Barro, R. J. and Redlick, C. J. (2011). Macroeconomic effects from government purchases and taxes. *Quarterly Journal of Economics*, 126(1):51–102.

Barsky, R. B. and Kilian, L. (2001). Do we really know that oil caused the great stagflation? a monetary alternative. *NBER Macroeconomics Annual*, 16:137–183.

Barsky, R. B. and Kilian, L. (2004). Oil and the macroeconomy since the 1970s. *Journal of Economic Perspectives*, 18(4):115–134.

Baumeister, C. and Hamilton, J. D. (2015). Structural interpretation of vector autoregressions with incomplete identification: Revisiting the role of oil supply and demand shocks. *Manuscript, University of Notre Dame and UCSD*.

Baumeister, C. and Kilian, L. (2016). Lower oil prices and the u.s. economy: Is this time different? *Brookings Papers on Economic Activity*, 2016(2):287–357.

Baumeister, C. and Peersman, G. (2013). Time-varying effects of oil supply shocks on the us economy. *American Economic Journal: Macroeconomics*, 5(4):1–28.

Bernanke, B., Gertler, M., and Watson, M. W. (2004). Oil shocks and aggregate macroeconomic behavior: The role of monetary policy: A reply. *Journal of Money, Credit, and Banking*, 36(2):287–291.

Bernanke, B. S. (1983). Irreversibility, uncertainty, and cyclical investment. *Quarterly Journal of Economics*, 98(1):85–106.

Blanchard, O. and Perotti, R. (2002). An empirical characterization of the dynamic effects of changes in government spending and taxes on output. *Quarterly Journal of Economics*, 117(4):1329–1368.

Blanchard, O. J. and Galí, J. (2007). The macroeconomic effects of oil price shocks: Why are the 2000s so different from the 1970s? In *International Dimensions of Monetary Policy*, pages 373–421. University of Chicago Press.

Blanchard, O. J. and Riggi, M. (2013). Why are the 2000s so different from the 1970s? a structural interpretation of changes in the macroeconomic effects of oil prices. *Journal of the European Economic Association*, 11(5):1032–1052.

Bognanni, M. (2013). An empirical analysis of time-varying fiscal multipliers. *Recuperado de http://markbognanni. com.(Federal Reserve Bank of Cleveland).*

Caggiano, G., Castelnuovo, E., Colombo, V., and Nodari, G. (2015). Estimating fiscal multipliers: News from a non-linear world. *Economic Journal*, 125(584):746–776.

Caggiano, G., Castelnuovo, E., and Groshenny, N. (2014). Uncertainty shocks and unemployment dynamics in us recessions. *Journal of Monetary Economics*, 67:78–92.

Caldara, D., Cavallo, M., and Iacoviello, M. (2017). Oil price elasticities and oil price fluctuations.

Canzoneri, M., Collard, F., Dellas, H., and Diba, B. (2016). Fiscal multipliers in recessions. *Economic Journal*, 126(590):75–108.

Christiano, L., Eichenbaum, M., and Rebelo, S. (2011). When is the government spending multiplier large? *Journal of Political Economy*, 119(1):78–121.

Csereklyei, Z., Rubio Varas, M. d. M., and Stern, D. I. (2016). Energy and economic growth: The stylized facts. *Energy Journal*, 37(2):223–255.

Dargay, J. M. and Gately, D. (2010). World oil demands shift toward faster growing and less price-responsive products and regions. *Energy Policy*, 38(10):6261–6277.

Davis, S. J. and Haltiwanger, J. (2001). Sectoral job creation and destruction responses to oil price changes. *Journal of Monetary Economics*, 48(3):465–512.

Diebold, F. X., Lee, J.-H., and Weinbach, G. C. (1994). Regime switching with time-varying transition probabilities. *Business Cycles: Durations, Dynamics, and Forecasting*, pages 144–165.

Edelstein, P. and Kilian, L. (2009). How sensitive are consumer expenditures to retail energy prices? *Journal of Monetary Economics*, 56(6):766–779.

Elder, J. and Serletis, A. (2010). Oil price uncertainty. *Journal of Money, Credit and Banking*, 42(6):1137–1159.

Fazzari, S. M., Morley, J., and Panovska, I. (2015). State-dependent effects of fiscal policy. *Studies in Nonlinear Dynamics & Econometrics*, 19(3):285–315.

Fisher, J. D. and Peters, R. (2010). Using stock returns to identify government spending shocks. *Economic Journal*, 120(544):414–436.

Gately, D. and Huntington, H. G. (2002). The asymmetric effects of changes in price and income on energy and oil demand. *Energy Journal*, pages 19–55.

Gilchrist, S. and Zakrajšek, E. (2012). Credit spreads and business cycle fluctuations. *American Economic Review*, 102(4):1692–1720.

Gordon, R. J. and Krenn, R. (2010). The end of the great depression: Var insight on the roles of monetary and fiscal policy. NBER Working Paper.

Hamilton, J. D. (1983). Oil and the macroeconomy since world war ii. *Journal of Political Economy*, 91(2):228–248.

Hamilton, J. D. (1988). A neoclassical model of unemployment and the business cycle. *Journal of Political Economy*, 96(3):593–617.

Hamilton, J. D. (1989). A new approach to the economic analysis of nonstationary time series and the business cycle. *Econometrica*, 57(2):357–384.

Hamilton, J. D. (1990). Analysis of time series subject to changes in regime. *Journal of Econometrics*, 45(1-2):39–70.

Hamilton, J. D. (1994). *Time Series Analysis*. Princetion University Press, Princeton, NJ.

Hamilton, J. D. (1996). This is what happened to the oil price-macroeconomy relationship. *Journal of Monetary Economics*, 38(2):215–220.

Hamilton, J. D. (2003). What is an oil shock? *Journal of Econometrics*, 113(2):363–398.

Hamilton, J. D. (2005). Oil and the macroeconomy. *The New Palgrave Dictionary of Economics*, 2.

Hamilton, J. D. (2008). Macroeconomics and arch. In *Volatility and Time Series Econometrics: Essays in Honor of Robert Engle*, pages 79–96. Oxford University Press.

Hamilton, J. D. (2009). Causes and consequences of the oil shock of 2007–08. *Brookings Papers on Economic Activity*, (1):215–261.

Hamilton, J. D. (2011). Nonlinearities and the macroeconomic effects of oil prices. *Macroeconomic Dynamics*, 15(S3):364–378.

Hamilton, J. D. (2016). Macroeconomic regimes and regime shifts. *Handbook of Macroeconomics*, 2:163–201.

Herrera, A. M., Lagalo, L. G., and Wada, T. (2011). Oil price shocks and industrial production: Is the relationship linear? *Macroeconomic Dynamics*, 15(S3):472–497.

Herrera, A. M. and Pesavento, E. (2009). Oil price shocks, systematic monetary policy, and the great moderation. *Macroeconomic Dynamics*, 13(1):107–137.

Hooker, M. A. (1996). What happened to the oil price-macroeconomy relationship? *Journal of Monetary Economics*, 38(2):195–213.

Hooker, M. A. (2002). Are oil shocks inflationary? asymmetric and nonlinear specifications versus changes in regime. *Journal of Money, Credit, and Banking*, 34(2):540–561.

Jefferson, P. N. (1998). Inference using qualitative and quantitative information with an application to monetary policy. *Economic Inquiry*, 36(1):108–119.

Jordà, Ò. (2005). Estimation and inference of impulse responses by local projections. *American Economic Review*, 95(1):161–182.

Katayama, M. (2013). Declining effects of oil price shocks. *Journal of Money, Credit and Banking*, 45(6):977–1016.

Kendrick, J. W. (1961). Front matter, productivity trends in the united states. In *Productivity Trends in the United States*, pages 52–0. Princeton University Press.

Kilian, L. (2008). Exogenous oil supply shocks: How big are they and how much do they matter for the us economy? *The Review of Economics and Statistics*, 90(2):216–240.

Kilian, L. (2009). Not all oil price shocks are alike: Disentangling demand and supply shocks in the crude oil market. *American Economic Review*, 99(3):1053–1069.

Kilian, L. and Kim, Y. J. (2011). How reliable are local projection estimators of impulse responses? *Review of Economics and Statistics*, 93(4):1460–1466.

Kilian, L. and Murphy, D. P. (2012). Why agnostic sign restrictions are not enough: Understanding the dynamics of oil market var models. *Journal of the European Economic Association*, 10(5):1166–1188.

Kilian, L. and Murphy, D. P. (2014). The role of inventories and speculative trading in the global market for crude oil. *Journal of Applied Econometrics*, 29(3):454–478.

Kilian, L. and Vigfusson, R. J. (2011a). Are the responses of the us economy asymmetric in energy price increases and decreases? *Quantitative Economics*, 2(3):419–453.

Kilian, L. and Vigfusson, R. J. (2011b). Nonlinearities in the oil price–output relationship. *Macroeconomic Dynamics*, 15(S3):337–363.

Kilian, L. and Vigfusson, R. J. (2013). Do oil prices help forecast u.s. real gdp? the role of nonlinearities and asymmetries. *Journal of Business & Economic Statistics*, 31(1):78–93.

Kim, C.-J. (1994). Dynamic linear models with markov-switching. *Journal of Econometrics*, 60(1):1–22.

Kim, C.-J. and Nelson, C. R. (1999). *State-space Models with Regime Switching: Classical and Gibbs-sampling Approaches with Applications*. MIT press, Cambridge, MA.

Kim, C.-J., Piger, J., and Startz, R. (2008). Estimation of markov regime-switching regression models with endogenous switching. *Journal of Econometrics*, 143(2):263–273.

Koop, G., Pesaran, M. H., and Potter, S. M. (1996). Impulse response analysis in nonlinear multivariate models. *Journal of Econometrics*, 74(1):119–147.

Lee, K. and Ni, S. (2002). On the dynamic effects of oil price shocks: A study using industry level data. *Journal of Monetary Economics*, 49(4):823–852.

Lee, K., Ni, S., and Ratti, R. A. (1995). Oil shocks and the macroeconomy: The role of price variability. *Energy Journal*, 16(4):39–56.

Michaillat, P. (2014). A theory of countercyclical government multiplier. *American Economic Journal: Macroeconomics*, 6(1):190–217.

Mittnik, S. and Semmler, W. (2012). Regime dependence of the fiscal multiplier. *Journal of Economic Behavior & Organization*, 83(3):502–522.

Miyamoto, W., Nguyen, T. L., and Sergeyev, D. (2018). Government spending multipliers under the zero lower bound: Evidence from japan. *American Economic Journal: Macroeconomics*, 10(3):247–77.

Mork, K. A. (1989). Oil and the macroeconomy when prices go up and down: An extension of hamilton's results. *Journal of Political Economy*, 97(3):740–744.

Mountford, A. and Uhlig, H. (2009). What are the effects of fiscal policy shocks? *Journal of Applied Econometrics*, 24(6):960–992.

Owyang, M. T., Ramey, V. A., and Zubairy, S. (2013). Are government spending multipliers greater during periods of slack? evidence from twentieth-century historical data. *American Economic Review*, 103(3):129–134.

Primiceri, G. E. (2005). Time varying structural vector autoregressions and monetary policy. *The Review of Economic Studies*, 72(3):821–852.

Ramey, V. A. (2011). Identifying government spending shocks: It's all in the timing. *Quarterly Journal of Economics*, 126(1):1–50.

Ramey, V. A. and Vine, D. J. (2011). Oil, automobiles, and the us economy: How much have things really changed? *NBER Macroeconomics Annual*, 25(1):333–368.

Ramey, V. A. and Zubairy, S. (2018). Government spending multipliers in good times and in bad: Evidence from us historical data. *Journal of Political Economy*, 126(2):850–901.

Rotemberg, J. J. and Woodford, M. (1996). Imperfect competition and the effects of energy price increases on economic activity. *Journal of Money, Credit and Banking*, 28(4):549–577.

Stock, J. H., Watson, M. W., et al. (2012). Disentangling the channels of the 2007-09 recession. *Brookings Papers on Economic Activity*, 43(1 (Spring)):81–156.

Tenreyro, S. and Thwaites, G. (2016). Pushing on a string: Us monetary policy is less powerful in recessions. *American Economic Journal: Macroeconomics*, 8(4):43–74.

Uhlig, H. (2010). Some fiscal calculus. *American Economic Review*, 100(2):30–34.

Wu, T. and Cavallo, M. (2012). Measuring oil-price shocks using market-based information. IMF Working Paper.

# Chapter 3

# Wage Trickle Down vs. Rent Trickle Down: How does increase in college graduates affect wages and rents?

## 3.1 Introduction

This study investigates how rising shares of college graduates in cities affects welfare distributions across individuals with different levels of education attainment. We do this by examining their changes in both wages and in housing costs. Extant research finds evidence of increasing skill divergence across US cities (Berry and Glaeser 2005, Glaeser et al. 2004). Since the 1980s, cities with initially higher schooling levels have attracted greater shares of adults with college degrees. While prior studies have largely focused on the impact of human capital externalites on wages, they have overlooked the changes in the cost of living, which is a significant element of individual welfare. Not only does this study examine how rising college share simultaneously affects wages and rental costs, and thus provide a broader view of welfare implications, we come closer than past research to reveal causal linkages between wages and college share by exploiting the nature of panel data. We should emphasize the phrase "come closer," because of the inherent difficulty to perfectly control for unobservable factors that is associated with where households choose to live.

Despite widespread evidence that individuals in cities with higher shares of skilled population receive higher wages, there are disagreements about who receives greater benefits from the increase of human capital. For example, Moretti (2004b) finds that wage increases are higher for the less skilled population in highly skilled cities, while Berry and Glaeser (2005) find an opposite result. Data limitations lead to identification challenges for past studies that examine the external effects of increasing human capital. Both Moretti (2004b)[1] andBerry and Glaeser (2005) use Census data to test whether the size of the college share effect on wages differs by individuals' educational attainment. Although the Census has sufficient numbers of observations to externally validate the results, the data only allows cross-city, cross-sectional comparisons. Therefore, prior studies were not able to document what happened to individuals residing within the same cities over time. Because the Census does not follow individuals and was collected once in every 10 years, researchers using Census data cannot fully control for individuals' unobserved ability or their sorting behavior. Unobserved individual characteristics, such as ability, are likely to be correlated with both wages and college share. If the return for unobserved ability is higher in cities with higher college shares, then high quality workers without college degrees may sort into cities with higher college shares. Furthermore, in the long run, individuals can adjust to the externally driven changes in their wages and rental costs by moving to a new location. For example, if housing costs increase more in cities where shares of college graduates increase, then low skilled workers may eventually decide to move to cities where housing is cheaper. These types of sorting will bias cross-sectional coefficients on the external impact of human capital, especially when the gap between the sample periods is large.

We address these problems by using the Panel Study of Income Dynamics (PSID). Although the sample size is much smaller than the Census, the major advantage of using the PSID is that it

[1] Moretti (2004b) uses the NLSY (National Longitudinal Survey of Youth) to control for individual's unobserved ability and sorting. As sample is small with only individuals below age 37, Morretti uses Census data when examining whether the human capital externality effects differ by the level of education.

tracks individuals over time. Thus, we are able to control for both the differences in the level of unobserved ability across individuals and the differences in returns to unobserved ability across cities. We can also observe those who move within and out of the city. As moving involves cost, it might take time for individuals to react to the wage/price impacts of changing shares of highly educated populations. By tracking individuals annually (biannually from 1997), we are able to identify what happens to the earnings of those who stay in the same city.

Our results confirm prior studies that show individuals earn higher wages in cities with higher shares of college graduates and also in cities where the shares of college graduates are increasing. Even after controlling for sorting and unobserved ability, we find that a 1 % increase in the share of college graduates leads to a 1.4 % increase in wages. However, the size of the human capital externality on wages differs across education groups. We find that the wage premium increase of those without a high school diploma is less than a half of that for college graduates, in line with Berry and Glaeser (2005).

These results are in line with the theories of knowledge spillover and skill-bias technological change (SBTC). The interaction with high skilled workers enhances individual productivity via knowledge spillover (Marshall 1890, Jacob 1969, Porter 1990), which leads to higher wages. SBTC suggests that the computer revolution has increased the productivity of high skilled workers while displacing low skilled workers performing routine tasks (Katz and Murphy 1992, Goldin and Katz 2001, Acemoglu 2002, Autor and Dorn 2013), resulting in uneven wage gains across workers with different skills. As influxes of high skilled workers also facilitate the adoption of skilled biased technology (Beaudry et al., 2010), we may observe endogenous relationship between increasing college share and increasing return to skill.

Household welfare not only depends on income but also on the cost of living. Thus, focusing only on wages provide an incomplete picture of welfare changes arising from human capital externalities. As housing accounts for greatest proportion of living cost, we examine simultaneous changes in housing cost in response to influxes of college graduate. As the PSID dataset contains information about monthly rental payments, we test how the increase of college share affects the residual wages (income minus rent) of individuals living in these cities and estimate how the net benefits from human capital externalities are allocated across individuals in different education groups. While high skilled and low skilled workers likely compete in different labor markets, the boundaries in the housing market could be less rigid. If so, it is possible that an income trickle down from an influx of high skilled workers could be lower than a rent trickle down.

We find rental prices also increase more in cities with greater influxes of college graduates. Glaeser et al. (2001) has also found that since the 1970s, rents have gone up more quickly than wages in cities with greater shares of college graduates. Along with getting higher wage premiums, we also find that college graduates pay higher rent premiums in cities with increasing shares of college educated people.

Considering wage and rent growth simultaneously, we demonstrate that increases in rent offset the increases in wage growth in cities where shares of high skilled workers are growing. On average, the influx of college graduates increases rent to income ratios while residual earnings remain unchanged. However, there are significant discrepancies in the growth rate of rent to income ratios across education groups. While rent to income ratio increases about 3.4 % on average, those with the highest educational attainment experience the smallest increase of rent relative to income. The residual earnings of these people also increase the most in response to increasing college share. In contrast, residual earnings of renters with at most a high school degree show no increase in cities that are attracting high skilled workers.

Overall, college graduates receive greater gains from the influx of high skilled workers. In fact, on average, less educated people are no better off in these cities, as the increase of rent cost completely offsets the increase in wages. Additionally, we find that the increase in the welfare gap is greater in cities where the housing supply elasticity is low, as housing supply is slow to adjust to influx of college workers in these places. We also present evidence that less skilled workers are gaining greater utility from urban amenities  measured by eating out and child's school enrollment from living in high skilled cities.

The next section provides a spatial equilibrium model that shows how the increase in college workers can alter the welfare distribution across high and low skilled population. Section 3 describes the data and the methods we use to test the theories. Section 4 presents empirical results, and the final two sections discuss the implications and limitations of our findings and conclude.

## 3.2 Theoretical Model

Our general equilibrium model builds on Rosen-Roback spatial equilibrium framework (Rosen 1979, Roback 1982) but relaxes some adjustments to better reflect reality. In Rosen-Roback world, all workers are identical and indifferent between locations. When a city experiences a local demand or supply shock of labor, the impact of the shock is fully capitalized in the price of land. Therefore, shocks to the local economy do not affect workers' welfare, as changes in housing costs fully offset changes in wages.

Following Moretti (2011), we assume that workers have idiosyncratic preferences for location, which affect their mobility. Housing supply is not necessarily fixed. In other words, local labor supply is not infinite and housing supply elasticity is not zero. In this context, local labor market shocks are not fully capitalized into land prices, and can have different impacts across workers. In equilibrium, only the marginal worker is indifferent between locations, while the inframarginal workers either can benefit or lose from the changes in the local labor market.

As does Moretti, we allow for heterogeneity in skills, assuming there are skilled and unskilled workers. Within a city, workers with different levels of skills compete in different labor markets, but compete in a single housing market. Each city-specific productivity is an endogenous function of the relative size of skilled workers in the city, thus incorporating agglomeration externalities that can occur from endogenous improvements in skilled bias technology or knowledge spillover. Our model takes a step further than Moretti, in that we *simultaneously* allow for heterogeneity of skills and agglomeration externalities.

### 3.2.1 Model Environment

***Utility of Workers*** Consider an economy with two cities, $a$ and $b$. In each city, there are two types of workers with different skill levels, labeled as the skilled ($H$) and the unskilled ($L$). It is assumed that in each period $\tau$, indirect utility $U_{ijc,\tau}$ of worker $i$ with skill level $j \in \{H,L\}$ living in city $c \in \{a,b\}$ depends on her wage ($w_{jc,\tau}$), rent ($r_{c,\tau}$), value of amenity ($A_{jc,\tau}$), and idiosyncratic preference for the city of residence ($e_{ijc,\tau}$),

$$U_{ijc,\tau} = w_{jc,\tau} - r_{c,\tau} + A_{jc,\tau} + e_{ijc,\tau}, \quad j \in \{H,L\}, c \in \{a,b\}, \tau \in \{t-1,t\} \tag{3.1}$$

Because we assume that skilled and unskilled workers compete in the same housing market in this economy, the rents in the utility functions (3.1) are identical for the two skill levels. Suppose the idiosyncratic preference of worker $i$ for city $a$ over $b$ is[2]

$$e_{ija,\tau} - e_{ijb,\tau} \sim \text{logit}(0, s_j), \quad j \in \{H,L\} \tag{3.2}$$

Let $N_{jc,\tau}$ denote log of number of workers with skill level $k$ in city $c$, respectively. Let $N_k = N_{ja,\tau} + N_{jb,\tau}$, which is assumed to be fixed for each $j = H,L$. We further assume that[3] $N_H$=$N_L$=$N$. The magnitudes of $s_H$ and $s_L$ determine the mobility of workers for each skill level. If $s_H$= 0, for instance, skilled workers have no personal attachment to a city and they are perfectly mobile. On the other hand, if $s_L$=$\infty$, unskilled workers are perfectly immobile.

***Technology of Firms*** We assume that skilled and unskilled workers compete in different labor markets - there are two different types of firms hiring skilled and unskilled workers, respectively.

[2]Note that if a random variable $x$ follows logit$(\mu, s)$, the cdf of $x$ is

$$\frac{1}{1+\exp\left(-\frac{x-\mu}{s}\right)}$$

[3]We can allow different value of $N_H$ and $N_L$. However, this generalization does not affect to implication of our model, while making the derivations more complex.

The technology of a representative firm of each type is assumed be Cobb-Douglas:

$$\ln y_{jc,\tau} = X_{jc,\tau} + hN_{jc,\tau} + (1-h)K_{jc,\tau}, \quad j \in \{H,L\}, c \in \{a,b\}, \tau \in \{t-1,t\} \tag{3.3}$$

where $y_{jc,\tau}$, $K_{jc,\tau}$ and $X_{jc,\tau}$ denote output, capital input and technology level of the firms hiring workers with skill level j. We further assume that there exists an externality in technology:

$$X_{jc,\tau} = x_{jc,\tau} + \delta_j N_{Hc,\tau}, \quad j \in \{H,L\}, c \in \{a,b\}, \tau \in \{t-1,t\} \tag{3.4}$$

where $x_{kc,\tau}$ exogenously switches productivity worker with skill level $k$. In (3.4), it is assumed that productivity of firms hiring high skilled workers and low-skilled workers depends on $N_{Hc}$. Since $N_{ja,\tau} + N_{jb,\tau}$ is constant for each $j$, this assumption indicates that the productivity of a city depends on its share of high-skilled workers. Note that the spillover effect of $N_{Hc}$ to productivity is *external*, as the level of technology is not determined endogenously, but each firm optimizes its labor taking the level of $X_{Hc,\tau}$ and $X_{Lc,\tau}$ to be exogenously given.[4]

Our specification of the externality in productivity in (3.4) relates the values of $\delta_H$ and $\delta_L$ to the strength of agglomeration effects of relative share of high-skilled worker in a specific city. According to the theory of knowledge spillover, the relative size of $\delta_H$ and $\delta_L$ determines the relative size of the benefit that skilled and unskilled workers receive from interacting with skilled workers: if skilled workers gain greater benefit, then $\delta_H$ will be greater than $\delta_L$, and vice versa.

***Local Housing Market*** Following Moretti (2011), we assume that one worker demands one house. Unlike the conventional Rosen-Roback model, we assume that housing supply is not

[4]One can understand (3.3) as the aggregate technology function, assuming there are infinitely many identical firms in each type and city. Each firm chooses its own amount of labor input, of which the contribution to aggregate labor is zero. The aggregate high-skilled labor $N_{Hc}$, which cannot be controlled by individual firms, is the sum of labor input of identical firms and externally determines the level of technology $X_{kc,\tau}$ for each $k = H, L$.

fixed. For simplicity, we assume that the two cities have the same housing supply function:

$$r_{c,\tau}=z+k\left(N_{Hc,\tau}+N_{Lc,\tau}\right), \qquad c\in\{a,b\} \tag{3.5}$$

Equation (3.5) implies that high- and low- skilled workers compete in the same housing market in each city. For both skill levels, elasticity of housing supply is identical.

***Local Capital Market*** For simplicity, interest rate is assumed to be fixed internationally. Then the capital demand in each type of firm is given as

$$K_{jc,\tau}=\frac{1}{h}\left[-\ln r+\ln(1-h)+hN_{jc,\tau}+X_{jc,\tau}\right], \qquad j\in\{H,L\}, c\in\{a,b\}, \tau\in\{t-1,t\} \tag{3.6}$$

***Local labor market*** To solve the model, we need supply and demand functions in the local labor market city $b$. A marginal high-skilled worker $i^*$ in period $\tau$ satisfies:

$$e_{Hi^*a,\tau}-e_{Hi^*b,\tau}=\left(w_{Ha,\tau}-w_{Hb,\tau}\right)-\left(r_{a,\tau}-r_{b,\tau}\right)+\left(A_{Ha,\tau}-A_{Hb,\tau}\right) \tag{3.7}$$

Let $m^*_{H,\tau}=e_{Hi^*a,\tau}-e_{Hi^*b,\tau}$. If $e_{Hia,\tau}-e_{Hib,\tau}\leq m^*_{H,\tau}$, high-skilled worker $i$ chooses city $b$ over $a$. From (3.2), we have

$$m^*_{H,\tau}=s_H\left(N_{Hb,\tau}-N_{Ha,\tau}\right). \tag{3.8}$$

Then for each $j=H,L$, (3.7) and (3.8) yield the labor supply function as below:

$$w_{jb,\tau}=w_{ja,\tau}+\left(r_{b,\tau}-r_{a,\tau}\right)+\left(A_{ja,\tau}-A_{jb,\tau}\right)+s_j\left(N^s_{jb,\tau}-N^s_{ja,\tau}\right), \tag{3.9}$$

As for the demand, perfect competition in the two types of local labor markets implies that each wage is equal to its corresponding marginal productivity of labor. From the production function

(3.6), the labor demand function is given as

$$w_{jb,\tau}=\frac{1}{h}\left(x_{jb,\tau}+\delta_j N^d_{Hc,\tau}\right), \quad j\in\{H,L\}, c\in a,b, \tau\in\{t-1,t\}, \tag{3.10}$$

### 3.2.2 Share of skilled workers and workers' welfare

In this subsection, we illustrate the relationship between the shares of skilled workers, and residual income (wage net housing cost). Specifically, we focus on a case when there is a positive shock on productivity of skilled workers.[5] In this case, we can show the larger the share of skilled workers, the greater the increase in wage and residual income inequality in response to the shock, given plausible values of parameters. In order to isolate the effect of change in productivity, we assume that amenities are fixed across time and the same across the cities.

Suppose the productivity of high-skilled workers in city $b$ increases exogenously by $\Delta x_{Hb,t} \equiv x_{Hb,t} - x_{Hb,t-1} = \varepsilon > 0$. Assume that the technology innovation is large enough to ensure that the residual income of high-skilled workers rises in city $b$ and $\Delta N_{Hb,t} > 0$. From the equilibrium condition, we can derive the following equation that shows that changes in the total population in city $b$.

$$\Delta N_{Hb,t} + \Delta N_{Lb,t} = \frac{s_L\varepsilon + 2(s_L\delta_H + s_H\delta_L)\Delta N_{Hb,t}}{2(s_L+s_H)hk + 2s_Ls_Hh} \tag{3.11}$$

Note that since all parameters in equation (3.9) are positive, the increase in total population in city $b$ will push up the rent. (3.9) further can be written as

$$\frac{\partial\Delta N_{Lb,t}}{\partial\Delta N_{Hb,t}} = \frac{s_L\delta_H + s_H\delta_L}{(s_L+s_H)hk + s_Ls_Hh} - 1, \tag{3.12}$$

implying that if the spillover effect of the high-skilled worker's technology is weak (i.e., $\delta_L$ is small), the share of high-skilled worker in city $b$ increases due to $\Delta x_{Hb,t} > 0$. Because the skilled and

[5]Note that we are not identifying the source of the local labor market shock, but examining how the increase of college share (which itself is a result of the local market supply or the demand shock) affects welfare distribution across different skill groups.

unskilled workers compete in the same housing market, if the improvement in the skilled worker does not increase the unskilled workers' wage by a large enough amount, the higher rent caused by the influx of the skilled workers crowds out the unskilled workers who cannot afford the higher housing cost.

Our model implies that the change in the residual income of the unskilled workers is

$$\Delta w_{Lb,t} - \Delta r_{b,t} = \left(\frac{\delta_L}{h} - k\right)\Delta N_{Hb,t} - k\Delta N_{Lb,t}, \tag{3.13}$$

which implies that the unskilled workers can be *worse off* if $\delta_L < hk$, even if $\delta_L > 0$ and the labor demand for unskilled worker increases when $\Delta x_{Hb,t} > 0$. In other words, if the spillover effect of technology for low skilled workers is weak, then increases in skilled workers' productivity can harm unskilled workers. The intuition behind of this is that if the skilled workers' productivity in city *b* does not increase unskilled workers' productivity enough, the labor income of unskilled workers in city b cannot increase enough to compensate for the higher rent caused by increased housing demand of the skilled workers.

To summarize, our model implies that depending on the degree of agglomeration effects or spillover effects of the skilled workers' productivity, it is possible that an increase in the share of skilled workers leads to distributional changes of welfare between skilled and unskilled workers, resulting in the unskilled becoming relatively worse off than the skilled.

## 3.3 Data & Method

***Data*** This study uses the Panel Study of Income Dynamics (PSID) from 1980 to 2013. The PSID has followed a sample of individuals and households in the US since 1968. Between 1968 and 1997, surveys were conducted annually; after 1997 there were conducted biannually. The major advantage of the PSID is that it contains extensive information on each individual's demographic and socioeconomic characteristics, including wages and rental costs. We also have access to the restricted geo-coded data which we use to merge the Decennial Census data to the PSID.

The dependent Variables come from the household level data. The PSID asks the hourly wage only for heads and wives.[6] Thus, our analysis does not include household members who are neither the head or a wife of the head. We include all head and wives between ages 16 and 65 who are not enrolled in school. On average, the annual number of observations in our sample is slightly over 10,500 individuals. The city level data comes from the Decennial Census 1980, 1990, 2000, 2010 and the American Community Survey (ACS) 2008-2012. Our definition of city includes metropolitan and micropolitan statistical area[7] which covers all urban labor markets. The key variable of interest is the share of college graduates for the adult population age 25 and over.[8] We download this variable from the Decennial Census and the American Community Survey at the census tract level and use the crosswalk file from Logan et al. (2014) to adjust 1980, 1990 and 2000 to match 2010 city boundaries. We then match the city level Variables to the geocoded PSID. We interpolated these Variables in the years for which Census figures are not available. For years 2011 and 2013, we used Census 2010 and ACS 2008-2012 data.

Table 3.1 presents the summary statistics for both individual and city level Variables. The inflation adjusted average hourly wage (2013 US dollars) is 22.83 dollars and the inflation adjusted average monthly rent is around 630 dollars. Average inflation adjusted home equity, calculated by subtracting the remaining mortgage principal from the self-reported house value, is approximately 105,000 dollars. Slightly less than a quarter of the sample received 4 years of college education, and about the same number of individuals received at least some level of college education. Almost 40 percent of those in the sample are high school graduates and the remaining did not receive a high

[6]Since the PSID asks about earnings in the previous year, we lead the earnings variable to match it with the current data.

[7]The boundaries of metropolitan and micropolitan statistical areas are defined by the office of Management and Budget. According to the US Census, metro area contains a core urban area of 50,000 or more population, and a micro area contains an urban core with a population at least 10,000 but less than 50,000. Each metro or micro area consists of at least one county including the urban core, and also any adjacent counties that have a high degree of social and economic integration (as measured by commuting to work) with the core urban area.

[8]Other city level Variables include total population and share of black and Hispanic. These Variables are collected from Census 1980, 1990, 2000 and 2010.

**Table 3.1**: Summary Statistics

| Variable | Mean | Std. Dev. |
|---|---|---|
| *Individual Level* | | |
| Hourly Wage | 22.83 | 24.89 |
| Monthly Rent | 629.59 | 432.22 |
| Home Equity | 104986.8 | 160454.7 |
| High School | 0.38 | 0.48 |
| Some College | 0.24 | 0.43 |
| College (BA+) | 0.23 | 0.42 |
| Black | 0.31 | 0.46 |
| Hispanic | 0.09 | 0.29 |
| Experienced (Years Work) | 12.18 | 9.25 |
| Age | 38.18 | 11.16 |
| Head | 0.67 | 0.47 |
| Female | 0.50 | 0.50 |
| Never Married | 0.14 | 0.34 |
| Divorced/Separated | 0.13 | 0.33 |
| Widowed | 0.02 | 0.13 |
| *City Level* | | |
| % BA+ | 0.16 | |
| % Black | 0.16 | 0.11 |
| % Hispanic | 0.09 | 0.11 |
| Population | 2962010 | 4047682 |
| Observation | 175,023 | |

school diploma. In the city level data, our key variable of interest is the percent of college graduates. On average, 16 percent of adults age 25 and above are college graduates, ranging from 4.7 percent in Ashtabula, Ohio in 1980 to 40.3 percent in Boulder, Colorado in 2000.[9]

As the PSID started its survey in 1968 and intentionally focused more on low-income households, the proportion of blacks is significantly higher than the national average, while Hispanics account for less than 1 percent of the sample. To adjust for the overrepresentation of blacks, the PSID provides weights for individuals. However, as fixed effects models require a constant weight within id, it cannot be applied when household weights change over time. Thus, we do not use weights in our main analysis. We also ran weight adjusted regressions by using the household weights when the household first was surveyed. These results do not show considerable changes from what we present and can be provided upon request.

***Method*** To recover the causal impact of college share on wages and rental costs, would we need to control for two omitted Variables: each individual's unobserved ability and sorting behavior. First, it is likely that individuals living in cities with increasing levels of human capital are more likely to have greater unobserved ability. Moretti (2004a) points out that cities that have a particular industrial structure may have greater demand for high skilled workers and also offer more compensation for unobserved ability. Thus, high wages for individuals living in these cities may merely reflect the heterogeneity of individuals' productivity due to unobserved skills. Furthermore, individuals' will migrate to cities where their unobserved ability have greater value. This sorting behavior will increase the wage gaps between those living in a city with greater demand for high skilled workers from those who are living in cities with less demand for the highly educated. Our empirical models deal with this issue by using both individual and city fixed effects and the multiplication of both. The following two equation forms are the two baseline models that we use:

[9]Note that the percent with a 4-year college degree in the PSID sample is higher the average share of college graduates in the city level data. This is because we exclude those who are over 65-year-olds in the PSID sample. The group who, on average, received less education than the younger generation. The fact that we only have data for heads and wives and not other individuals in the households is also likely to have increased the percent of college graduates in the PSID sample.

$$\ln Y_{ict} = \alpha_{ct} X_{it} + \beta CS_{ct} + \gamma CS_{ct} \times EDUC_i + \delta Z_{ct} + \pi_t + \mu_c + \theta_i + \varepsilon_{ict} \tag{3.14}$$

$$\ln Y_{ict} = \alpha_{ct} X_{it} + \beta CS_{ct} + \gamma CS_{ct} \times EDUC_i + \delta Z_{ct} + \pi_t + \mu_c \theta_i + \varepsilon_{ict} \tag{3.15}$$

In both models, $Y_{ict}$ represents three dependent Variables: individual hourly wage, monthly rent, or annual labor income relative to annual rent cost. $X_{it}$ is a vector of individual level characteristics, such as age, race and ethnicity; $CS_{ct}$ represents the share of college educated individuals in city $c$ at year $t$; $EDUC_i$ represents dummy Variables for the level of education (without high school diploma, high school, received less than four years of college education, college graduates); $Z_{ct}$ is a vector of city characteristics that may be correlated with $CS_{ct}$ including MSA population[10]; $\pi_t$ is the year fixed effects and $\varepsilon_{ict}$ is the error term. The coefficients of interest are $\beta$ and $\gamma$, which show whether college share affect wages and rents, and whether the size of this effect differs across groups of people in different education categories.

The above models differ in assumptions of how cities value unobserved ability and how households respond to it. The first model assumes that unobserved skills are equally valued in every city while the second model assumes that returns to unobserved ability vary across cities. In model (3.14), the impact of college share on wages and rents does not differ between movers and stayers, when controlling for observed and unobserved characteristics. On the other hand, in model (3.15) returns for unobservable skills differ across cities and affects individual's sorting behavior. To control for sorting, model (3.15) includes individual $\times$ city dummies which absorb the variations that occurs from the movers. Thus, in model (3.15) the coefficient shows how the changing college share affects an individual who stayed in the same city. If, for example, individuals move to cities where they gain greater returns for unobserved skills, than $\beta$ and $\gamma$ in the wage regression will be higher in model (3.14) than model (3.15). Additionally, if individuals are moving to skilled cities where the housing cost is more expensive, than $\beta$ and $\gamma$ in the rent regression will also be higher in (3.14) than (3.15).

[10] As we control for the share of population, the impact of share of college graduates on our dependent Variables are showing the impact of changes in composition of population.

We should note that our use of fixed effects attenuates, but does not entirely solve, the problem on unobservables. People's unobservables change over time, and it is possible that people move when their unobservables become better than their lifetime average. Nevertheless, the use of fixed effects does reduce the magnitude of omitted variable bias.

## 3.4 Empirical Results

***Hourly Earnings*** Table 3.2 presents the relationship between the share of college graduates and wages. The dependent variable is the log hourly earnings. Column (1) shows that individuals living in cities with higher shares of college graduates have higher wages, even after controlling for individual and city level Variables, including individuals' educational attainment. When the share of college graduates increases by 1 percentage point, hourly earnings go up by 1.26%, which is similar to Moretti's (2004b) finding of a 1.31% increase in hourly earnings. In column (2), we include city fixed effect and find that the external return conditional on city fixed effect drops to 0.60%.[11] This shows that in cities where the college share is growing, individuals' wages are also going up. This specification, however, does not control for individuals' unobserved ability or sorting behavior.

Next, we add individual fixed effects. Individual fixed effects capture any unobserved characteristics such as ability or family background. When the permanent individual characteristics are controlled for, the estimated effect of human capital externality increases back to 1.32%. This shows that the differences in the unobservables do not explain why individuals have higher earnings in cities that attract college graduates. However, there is still a possibility that these results are due to sorting. In reality, individuals do not randomly select places to live but make a choice correlated

[11]Using National Longitudinal Survey of Youth data, Moretti finds that the estimated private return to education conditional on city fixed effects is 1.13% which is almost double of what we find. This may be related to difference in the sample as Moretti examines only population between ages 23 to 37. If we run the same regression for population under 38 our coefficient increases to 0.98%. The difference in the result may also be related to the period of estimation. The size of our coefficient also increases if we only include years before 1995. This suggests that the effect of human capital externality also differs by age groups and time periods. In this study, however, we focus on how the effect differs across the level of education. Using Census data, Morretti finds that a 1 percentage point increase in college share raises average wages by 0.6 to 1.2%, which is similar to the range found in our study.

**Table 3.2**: Effect of Changes in Share of College Graduates on Log Hourly Wage

| Variables | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| % BA+ | 1.264*** | 0.598** | 1.320*** | 1.381*** |
| | (0.077) | (0.293) | (0.243) | (0.255) |
| High School | -0.180*** | -0.185*** | | |
| | (0.005) | (0.010) | | |
| Some College | -0.155*** | -0.150*** | | |
| | (0.011) | (0.016) | | |
| College | 0.270*** | 0.258*** | | |
| | (0.006) | (0.011) | | |
| Black | 0.433*** | 0.415*** | | |
| | (0.007) | (0.012) | | |
| Hispanic | 0.733*** | 0.715*** | | |
| | (0.007) | (0.014) | | |
| Individual FE | | | Y | |
| City FE | | Y | Y | |
| Individual×City FE | | | | Y |
| Year FE | Y | Y | Y | Y |
| Observations | 146,331 | 146,331 | 146,331 | 146,331 |
| R-squared | 0.398 | 0.411 | 0.279 | 0.252 |
| Number of cbsaid | | | | 25,631 |
| Number of id | | | 21,082 | |

Note: Dependent variable is log value of hourly wage. Following Moretti (2004a), robust standard errors, corrected for city year clustering, are in parentheses. (*** $p<0.01$, ** $p<0.05$, * $p<0.1$).

**Table 3.3**: Effect of Changes in Share of College Graduates on Log Hourly Wage (continued from Table 3.2)

| Variables | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Head | 0.046*** | 0.046*** | 0.043*** | 0.044*** |
| | (0.002) | (0.002) | (0.004) | (0.004) |
| Female | -0.001*** | -0.001*** | -0.001*** | -0.001*** |
| | (0.000) | (0.000) | (0.000) | (0.000) |
| Single | 0.011*** | 0.011*** | -0.002** | -0.001 |
| | (0.001) | (0.001) | (0.001) | (0.001) |
| Div/Separated | -0.000*** | -0.000*** | 0.000 | 0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) |
| Widowed | 0.200*** | 0.199*** | | |
| | (0.008) | (0.015) | | |
| Age | -0.157*** | -0.160*** | | |
| | (0.008) | (0.013) | | |
| Age sq./100 | -0.232*** | -0.235*** | | |
| | (0.008) | (0.014) | | |
| Experience | -0.208*** | -0.201*** | | |
| | (0.008) | (0.014) | | |
| Experience sq./100 | -0.252*** | -0.248*** | | |
| | (0.017) | (0.031) | | |
| % Black | -0.074** | 0.056 | 0.489 | 0.667** |
| | (0.029) | (0.351) | (0.302) | (0.320) |
| % Hispanic | 0.0384 | 0.156 | 0.467*** | 0.506*** |
| | (0.029) | (0.174) | (0.162) | (0.177) |
| Log (Population) | 0.061*** | 0.052 | -0.025 | -0.068* |
| | (0.003) | (0.038) | (0.036) | (0.039) |
| Constant | -0.429*** | -0.412 | 0.826* | 1.621*** |
| | (0.046) | (0.443) | (0.478) | (0.544) |
| Individual FE | | | Y | |
| City FE | | Y | Y | |
| Individual×City FE | | | | Y |
| Year FE | Y | Y | Y | Y |
| Observations | 146,331 | 146,331 | 146,331 | 146,331 |
| R-squared | 0.398 | 0.411 | 0.279 | 0.252 |
| Number of cbsaid | | | | 25,631 |
| Number of id | | | 21,082 | |

Note: Dependent variable is log value of hourly wage. Following Moretti (2004a), robust standard errors, corrected for city year clustering, are in parentheses. (*** $p<0.01$, ** $p<0.05$, * $p<0.1$).

with their expected return. For example, if return to ability differs across cities and individuals move to places that offer greatest return, then $\beta$ will be biased upward. In column (4), we include individual$\times$city fixed effect to control for unobserved heterogeneity in the return to ability across city. In this regression, $\beta$ indicates the size of the human capital externality on hourly wages for those who do not move. The size of the $\beta$ in column (4) is not significantly different from column (3), suggesting that the movers and the stayers are gaining similar wage premiums from the increases in college share.

We next examine whether the size of the human capital eternality on wages differs across four education groups: without high school diploma, high school graduate, received some level of college education, with a bachelor degree. The reference group is those without high school diplomas. The first column in Table 3.4 shows that wages are higher in cities with greater shares of college graduates for all for educational attainment groups. Among the four groups, the size of the coefficient for those with a bachelor degree is significantly higher than the remaining three groups. Column (2) shows when share of college graduates increase within the same city, individual wage growth increases with the level of educational attainment.

While this linear pattern changes once the individual fixed effects are included, we still find that bachelor degree holders benefit the most from increases in college share (column (3)). The result shows that those with a high school degree or less gain about 0.56 percent to 0.66 percent increases in hourly wages in response to a 1 percentage point increase in the share of college graduates. Compared to the least educated individuals, those with some level of college education gain a 0.34 percentage point higher increase in hourly earnings from the increases in the level of human capital, while college graduates receive a 1.12 percentage point higher increase in hourly earnings. We find similar results, when controlling for sorting by including individual x city fixed effects. Again, college graduates receive the greatest wage benefits (1.732) from a 1 percentage point increase in college graduates, and high school drop outs receive the smallest benefit (0.698%).

Overall, our results are in line with two theoretical explanations. First, knowledge spillover

**Table 3.4**: Effect of Changes in Share of College Graduates on Hourly Wage by Education Level

| Variables | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| % BA+ | 0.890*** | -0.146 | 0.561** | 0.698** |
| | (0.134) | (0.253) | (0.263) | (0.273) |
| % BA+ * High School | 0.197 | 0.331*** | 0.0994 | 0.087 |
| | (0.126) | (0.125) | (0.082) | (0.084) |
| % BA * Some College | 0.339** | 0.589*** | 0.335*** | 0.349*** |
| | (0.134) | (0.133) | (0.092) | (0.095) |
| % BA * College (BA+) | 0.728*** | 0.866*** | 1.120*** | 1.034*** |
| | (0.145) | (0.145) | (0.111) | (0.114) |
| Individual Control | Y | Y | Y | Y |
| City Control | Y | Y | Y | Y |
| Individual FE | | | Y | |
| City FE | | Y | Y | |
| Individual×City FE | | | | Y |
| Year FE | Y | Y | Y | Y |
| Observations | 146,331 | 146,331 | 146,331 | 146,331 |
| R-squared | 0.398 | 0.412 | 0.280 | 0.253 |
| Number of cbsaid | | | | 25,631 |
| Number of id | | | 21,082 | |

Note: Dependent variable is log value of hourly wage. Individual control includes race, head, sex, marital status, age, experience. City control Variables include the share of black and Hispanic population, and log population for each city. Robust standard errors, corrected for city year clustering, are in parentheses. (*** p<0.01, ** p<0.05, * p<0.1).

exists (Marshall 1890, Jacob 1969, Porter 1990;Moretti 2004a;2004b). Moreover, the benefit of knowledge spillover increases by the level of education. Second, cities where college share is higher and where the share increases also adopt skill-biased technology more intensively, which as a result increases the wages of skilled workers relative to unskilled workers. (Beaudry et al. 2010, Autor and Dorn 2013).[12] Our findings show that highly educated workers experience greater wage gains is consistent Berry and Glaeser (2005).

***Monthly Rents*** Next, we investigate how rent costs are changing in response to increases in college share. Because we focus on renters only, our sample size becomes smaller. Also, instead of including both heads and wives we only include heads since including wives will double count households. The PSID provides information about the house value and monthly mortgage payment. However, since the housing cost of homeowners is mostly decided when the property is first bought, the impact of college share on house costs of homeowners will differ across existing homeowners and new buyers. For this reason, we only focus on renters, who are more likely to experience concurrent changes in housing cost in response to increasing numbers of college graduates. In Table A6, we additionally show how the increase in the share of college graduates affects the welfare distribution of homeowners by examining the changes in their self-reported housing wealth. The result is further discussed in the later part of the study.

From Table 3.5 and onwards, we only present the results using either model which includes both individual and city fixed effects (Columns (1) & (3)) and model which includes individual× city fixed effect (Columns (2) & (4)). We do so as the results without the fixed effects or with only the city fixed effects do not show significant differences from results we do present.

The first two columns in Table 3.5 show that the increase of college share is associated with an increase in monthly rent. The coefficient size of the human capital effect on rent is 2.36% and 2.46% in column (1) and column (2), respectively. Columns (3) and (4) show that rent price increases

[12] Although not presented in the paper, we find that the size of the human capital effect on wages was larger during the period before the year 2000s, when the adoption of the skill-bias technology was high. Once the technology diffuses across the cities the coefficient size for the share of college graduates decreases significantly.

**Table 3.5**: Effect of Changes in Share of College Graduates on Log Monthly Rent

| Variables | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| % BA+ | 2.355*** | 2.462*** | 1.794*** | 1.929*** |
| | (0.428) | (0.512) | (0.435) | (0.519) |
| % BA+ * High School | | | 0.422*** | 0.427*** |
| | | | (0.095) | (0.102) |
| % BA * Some College | | | 0.585*** | 0.530*** |
| | | | (0.106) | (0.116) |
| % BA * College (BA+) | | | 1.127*** | 1.084*** |
| | | | (0.121) | (0.136) |
| Individual Control | Y | Y | Y | Y |
| City Control | Y | Y | Y | Y |
| Individual FE | Y | | Y | |
| City FE | Y | | Y | |
| Individual×City FE | | Y | | Y |
| Year FE | Y | Y | Y | Y |
| Observations | 56,296 | 56,296 | 56,296 | 56,296 |
| R-squared | 0.251 | 0.211 | 0.252 | 0.212 |
| Number of id | 13,930 | | 13,930 | |
| Number of cbsaid | | 16,312 | | 16,312 |

Note: Dependent variable is log value of monthly rent. Individual control includes race, head, sex, marital status, age, experience. City control Variables include the share of black and Hispanic population, and log population for each city. Robust standard errors, corrected for city year clustering, are in parentheses. (*** $p<0.01$, ** $p<0.05$, * $p<0.1$).

are greater for those with higher educational attainment. In both regressions, the rental costs of college graduates increase by approximately 3% in response to a 1 percentage point increase of share of college graduates, which is approximately 1.1 percentage point higher than the rent increases of high school dropouts.

Although this study does not directly examine why there are differences in rent growth for different educational groups, we speculate that it reflects neighborhood sorting *within* cities (we only examine sorting across cities). Numerous studies, including Massey et al. (2009), find that segregation by socioeconomic status has increased over the last three decades, even though segregation by race has declined during this period. Our finding is also in line with the gentrification story: college educated workers gentrify low income neighborhoods, which in turn increases the rent for less skilled workers. Nevertheless, college graduates likely prefer high amenity, affluent neighborhoods, which explains why their rents appear to rise faster than non-college graduates.[13]

***Rent Increase vs. Earnings Increase*** To directly compare the cost and benefits of human capital externalities across groups with different educational attainment, we examine how the increase of college graduates affect rents relative to earnings. We use two dependent Variables to investigate the changes in rent and earning: annual rent over annual labor income and annual labor income minus annual rent. The first is a proxy for rent burden. If the growth rate of rent exceeds the growth rent of income, households will pay higher rent relative to income. However, even when the rent growth is higher than income growth, residual earnings (income minus rent) could increase if the absolute increase of income is higher than the absolute increase of rent. Our second dependent Variables show how the remaining income after paying for the rent changes due to changes in the share of college graduates. We include both heads and wives in our regression but adjust for rental cost depending on the employment status. If both heads and wives are working, we assume that they

[13]It is also possible that educated workers demand rental housing at the higher end. Thus, the influx of college workers drives up the competition and rents for higher cost rental housing. However, some college graduates may prefer lower cost rental apartment which increases rents for lower cost rental housing. These hypotheses require further empirical analysis.

are paying equal amounts of rent of their income and allocate half of the monthly income to each individual. This method adjusts for the double counting of monthly rents and also accounts for the fact that both spouses are more likely to work in high cost cities.[14]

The Table 3.6 presents the results where the dependent variable is the log value of rent over income. The first column shows that if the share of college graduates increases by 1 percentage point, the rent burden for renters increases by 3.42% in column (1) and 3.44% in column (2). The fact that rent burdens increase for stayers (at a similar degree as movers) as college graduate share rises implies that a mechanism other than sorting also explains the changes in rent burden. The next two columns show that higher college graduate shares lead to rent burden increases for all education groups. Compared to the less educated, however, the increase of rent burden is significantly lower for those who received a college degree. Although rental cost growth is lower for less skill workers compared to high skilled workers in cities where college share is increasing, the wages of less skilled workers are growing at an even lower rate. As a result, rent burden increases more for those who received less education living in cities where college share is growing. A series of studies (e.g., Haurin, 1991) shows that the income elasticity of demand for housing is well under one, suggesting that if low income people are spending a greater share of income on housing as housing costs rise, it is not because they are choosing to do so. This also provide some evidence that the housing market is less segmented than the labor market.

Table 3.7 examines how increasing college shares affect residual income, measured by the log value of income minus rent. On average, we do not find any statistical changes in the residual income in response to the increasing share of college graduates. However, columns (3) and (4) show that changes in residual income in response to the increasing college share differs by educational group. The residual income growth is significantly positive and higher for those who receive more

[14] We also run all our regressions in Tables V with only the heads in the sample. Additionally, we combine the labor income of heads and wives and recalculated the rent to income ratio and residual income and re-run the regressions. Overall, the results do not differ significantly from what is shown in Table V. However, we do find that residual income increases in cities where the college graduates are increasing when we use the combine the labor income of head and wives. These results can be provided upon request.

**Table 3.6**: Effect of Changes in Share of College Graduates on Rent-Income Distribution

| Dependent variable: ln(Rent to Income) | | | | |
|---|---|---|---|---|
| Variables | (1) | (2) | (3) | (4) |
| % BA+ | 3.420*** | 3.441*** | 3.562*** | 3.622*** |
| | (0.742) | (0.851) | (0.753) | (0.860) |
| % BA+ * High School | | | 0.065 | 0.057 |
| | | | (0.168) | (0.178) |
| % BA * Some College | | | -0.067 | -0.276 |
| | | | (0.202) | (0.215) |
| % BA * College (BA+) | | | -0.516** | -0.526** |
| | | | (0.229) | (0.251) |
| Individual Control | Y | Y | Y | Y |
| City Control | Y | Y | Y | Y |
| Individual FE | Y | | Y | |
| City FE | Y | | Y | |
| Individual×City FE | | Y | | Y |
| Year FE | Y | Y | Y | Y |
| Observations | 51,023 | 51,023 | 51,023 | 51,023 |
| R-squared | 0.027 | 0.013 | 0.028 | 0.013 |
| Number of id | 13,748 | | 13,748 | |
| Number of cbsaid | | 16,157 | | 16,157 |

Note: Individual control includes race, head, sex, marital status, age, experience. City control Variables include the share of black and Hispanic population, and log population for each city. Robust standard errors, corrected for city year clustering, are in parentheses. (*** p<0.01, ** p<0.05, * p<0.1).

**Table 3.7**: Effect of Changes in Share of College Graduates on Rent-Income Distribution

| Dependent variable: ln(Residual Income) | | | | |
|---|---|---|---|---|
| Variables | (1) | (2) | (3) | (4) |
| % BA+ | -0.779 | -0.309 | -1.273* | -0.866 |
| | (0.691) | (0.798) | (0.710) | (0.816) |
| % BA+ * High School | | | 0.001 | 0.095 |
| | | | (0.166) | (0.168) |
| % BA * Some College | | | 0.385** | 0.589*** |
| | | | (0.186) | (0.190) |
| % BA * College (BA+) | | | 1.395*** | 1.459*** |
| | | | (0.231) | (0.246) |
| Individual Control | Y | Y | Y | Y |
| City Control | Y | Y | Y | Y |
| Individual FE | Y | | Y | |
| City FE | Y | | Y | |
| Individual×City FE | | Y | | Y |
| Year FE | Y | Y | Y | Y |
| Observations | 54,285 | 54,285 | 54,285 | 54,285 |
| R-squared | 0.157 | 0.136 | 0.159 | 0.137 |
| Number of id | 14,092 | | 14,092 | |
| Number of cbsaid | | 16,751 | | 16,751 |

Note: Individual control includes race, head, sex, marital status, age, experience. City control Variables include the share of black and Hispanic population, and log population for each city. Robust standard errors, corrected for city year clustering, are in parentheses. (*** p<0.01, ** p<0.05, * p<0.1).

education. The residual income of who received high school of less education do not increase as cities gain greater share of college graduates. Comparing coefficients in columns (4) to column (3), we also find that those who move from a low skilled city to a high skilled city have a smaller increase in residual income than stayers who remain in a city where educational attainment rises. High school dropouts who move to cities with higher shares of college graduates actually experience a decrease in residual income, as cost of housing increases outweigh the wage gains. These results accord with Ganong and Shoag (2017), who find that unskilled individuals have become less likely to move to cities with high costs of living, as these cities have become more and more unaffordable over time.

***Robustness Check*** To confirm the robustness of our results, we regress our data using IV methods and include local labor demand shifts in our OLS regressions. We also run regressions for different subsamples and examine changes in the welfare distribution through changes in home equity value. Overall, the results remain largely similar to our main analyses.

While we control for individual's unobserved ability and sorting using fixed effects, we cannot claim causality due to the possibility of an omitted variable that may cause spurious results. In addition to local demand shifts, it is still feasible that there is a variable that simultaneously affects both the increase in college graduates and the increase in wages.[15] To address this issue, we use the presence of land-grant universities as an instrumental variable, as it is highly correlated to the present share of college graduates but is unlikely to be influenced by the current employment environment (Moretti, 2004b). The first stage regression results in column (1) of Table 3.8 show that the presence of a land-grant university is significantly associated with the share of college graduates. Both F and t-statistics confirm that the instrument is valid.

IV results show that both wages and rents increases with college share and the rent response is greater than the wage response, in line with our previous results. However, there are several limitations with the IV method. As the land grand university variable is a dummy we cannot use both it and city fixed effects. Furthermore, using the instrumental variable, we are only able to conduct cross-city comparisons and cannot compare between different educational groups using interaction terms. Small sample sizes limit us to running separate regressions for each education group. Therefore, it is not possible to use the Land-grant based IV method to to examine how city-wide educational attainment changes the distribution of welfare.

In the second robustness check, we directly control for industry-specific labor demand, which

[15]Since the passage of the Morrill Acts in 1862 and 1890, 73 land-grant universities have been established. All 50 states have a minimum of one land-grant school. These institutions were created to strengthen higher education, with focuses on engineering, agriculture and military science. To be a valid instrument, the existence of a land-grant university should not be correlated the unobserved quality of workers with the same level of education. Moretti (2004b) points out several factors that justifies using the land-grant university as an instrument: land-grant university were established more than 100 years ago, the program was implemented at the federal level, the universities were often established in rural areas and the location did not depend on natural resources or other factors that could make the region wealthier. Other studies also suggest that the geographical locations of land-grant universities were randomly selected (Nevins 1962, Williams 1991).

**Table 3.8**: Effect of Changes in Share of College Graduates: IV Results

| Variables | (1) % BA | (2) ll(Hourly Wage) | (3) ln(Monthly Rent) | (4) ln(Rent/Income) | (5) ln(Income-Rent) |
|---|---|---|---|---|---|
| Land Grant Univ. | 0.0400*** | | | | |
| | (0.002 ) | | | | |
| % BA+ | | 1.316*** | 2.408*** | 0.350 | 1.151 |
| | | (0.482) | (0.585) | (0.887) | (0.911) |
| City Control | Y | Y | Y | Y | Y |
| Individual FE | Y | Y | Y | Y | Y |
| Year FE | Y | Y | Y | Y | Y |
| F Stat | 635.49 | | | | |
| Number of cbsaid | 131,248 | 131,248 | 51,091 | 50,550 | 48,981 |
| Number of id | 19,474 | 19,474 | 12,787 | 13,542 | 12,886 |

Note: Robust standard errors, corrected for city year clustering, are in parentheses. (*** $p<0.01$, ** $p<0.05$, * $p<0.1$).

is correlated with both the increase in the share of college graduates and the increase in wages. We follow Katz and Murphy (1992) and Moretti (2004b), which use the Bartik shock to control for exogenous shifts in relative demand for different education groups. For example, a national increase in the demand for skilled workers in a certain industry will lead to a greater positive labor demand shock of skilled workers in cities that employs a larger share of the labor force in that industry. The index is based on nationwide employment growth for each industry, weighted by the city-specific employment share in those industries.[16]

Table 3.9 shows the wage regression which includes the two separate Bartik shocks for college graduates and for those who received high school or less education. In line with our expectation, we

[16]Using Decennial Census 1980, 1990, 2000 and ACS 2008-12 and ACS 2011-15, we create the following Bartik index for both college graduates and for those who received high school or less education:

$$\text{Bartik}_{jc} = \sum_{s=1}^{66} \theta_{sc} \Delta E_{js},$$

where Bartikjc predicts employment change for workers in educational group $j$ in city $c$; $\theta_{sc}$ is the share of total hours worked in industry $s$ (two digit sic-code) in the 1980, 1990, 2000 and 2010; $\Delta E_{js}$ is the change in the log of total hours of employers in education group j who worked in industry in s between 1980, 1990, 2000, 2010 and the following years (1990, 2000, 2010, 2013). As we did with other city level data from the Census and the ACS, we merge the two Bartik shocks to the PSID data and interpolate the data in the years for which data is missing.

**Table 3.9**: Effect of Changes in Share of College Graduates on Hourly Wage: Bartik Shock

| Variables | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| % BA+ | 1.293*** | 1.544*** | 0.790*** | 1.030*** |
| | (0.293) | (0.311) | (0.304) | (0.323) |
| % BA+ * High School | | | -0.069 | -0.009 |
| | | | (0.073) | (0.075) |
| % BA * Some College | | | 0.140* | 0.239*** |
| | | | (0.083) | (0.0849) |
| % BA * College (BA+) | | | 0.852*** | 0.846*** |
| | | | (0.094) | (0.097) |
| Bartik_BA | 3.051** | 2.648 | 3.582** | 3.197** |
| | (1.531) | (1.627) | (1.528) | (1.629) |
| Bartik_HS | -1.971 | -2.359 | -2.450 | -2.790 |
| | (1.608) | (1.697) | (1.606) | (1.699) |
| Individual Control | Y | Y | Y | Y |
| City Control | Y | Y | Y | Y |
| Individual FE | Y | | Y | |
| City FE | Y | | Y | |
| Individual×City FE | | Y | | Y |
| Year FE | Y | Y | Y | Y |
| Observations | 126,412 | 126,412 | 126,412 | 126,412 |
| R-squared | 0.271 | 0.247 | 0.273 | 0.248 |
| Number of id | 19,539 | | 19,539 | |
| Number of cbsaid | | 23,153 | | 23,153 |

Note: Dependent variable is log value of hourly wage. Robust standard errors, corrected for city year clustering, are in parentheses. (*** $p<0.01$, ** $p<0.05$, * $p<0.1$).

find that an increase in local demand for college graduates increases the average wages while an increase in local demand for those who received high school education at most decreases average wages. However, incorporating the local demand shifts does not significantly change the relationship between college share and wages for all education groups. Again, we find that the more educated individuals receive greater wage benefit from increasing numbers of college graduates.

If individuals face increases in their rent due to influxes of college graduates, but maintain a high preference for their current location, they can also move within the same city to make their housing more affordable. The sorting within the city can also affect our results, although it is more likely to cause a downward bias in the college share on monthly rent coefficient. In order to eliminate the effect of moving within the same city, we excluded the observation when households move within a city and rental prices change due to the moving. The coefficients in the Tables 3.10-3.13 do show significant differences across the four education groups from those in our main results, although the percent of rent increase in response to college share increase do become slightly smaller. This suggests that some residents do self-select to move into more affordable housing in response to the rising college share, but this *within* city sorting behavior does not alter our results significantly.

Finally, we examine changes in the welfare distribution of homeowners by looking at changes in home equity. Our main analysis focuses on changes in housing cost for renters only, as housing cash flow costs for owners tend to stay fixed after purchase.[17] Not only is the cost of housing relatively stable for owners compared to renters (Sinai and Souleles, 2005), homeowners can also build housing wealth from living in places where house prices are rising. This can also alter the welfare distribution across individuals with different educational attainment.

Table 3.14 shows that homeowners in cities where college share increases do experience an increase of housing wealth,[18] which is self-reported in the PSID. We find that a 1 percentage point increase in college share leads to greater than a 4 percent increase in home equity. Within a city, neighborhoods with greater shares of high skilled people are more likely to attracted high skilled workers. Thus, house prices can rise more in these neighborhoods, further benefitting the highly skilled. Columns (3) and (4) show that college graduates, indeed, experience a greater increase in home equity compared to less educated households. Furthermore, the homeownership rate rises with skills.[19] Thus, if house values increase more in cities where college share is rising, the welfare gap between high and low skilled workers will further increase in these cities, as owners (who are more likely to be college graduates) gain greater housing wealth, while renters experience a greater increase in rents.

[17]The exception to this is property tax costs, which can rise (or fall) after purchase, and maintenance costs, which tend to move with inflation.

[18]To control for the impact from moving within the city, we control for inner city moves using a dummy variable. As with monthly rent regressions, the unit of analysis is households.

[19]In our sample, homeownership rates for each level of educational attainment are: high school dropouts: 38 %, high school graduates: 47%, those who received some college education: 52% and college graduates: 68%.

**Table 3.10**: Effect of Changes in Share of College Graduates on Log Hourly Wage: Within City Moves Excluded

| Variables | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| % BA+ | 1.342*** | 1.461*** | 0.858*** | 0.981*** |
| | (0.0253) | (0.270) | (0.267) | (0.284) |
| % BA+ * High School | | | -0.079 | -0.045 |
| | | | (0.079) | (0.081) |
| % BA * Some College | | | 0.082 | 0.142 |
| | | | (0.087) | (0.087) |
| % BA * College (BA+) | | | 0.813*** | 0.798*** |
| | | | (0.101) | (0.103) |
| Individual Control | Y | Y | Y | Y |
| City Control | Y | Y | Y | Y |
| Individual FE | Y | | Y | |
| City FE | Y | | Y | |
| Individual × City FE | | Y | | Y |
| Year FE | Y | Y | Y | Y |
| Observations | 123,539 | 123,539 | 123,539 | 123,539 |
| R-squared | 0.287 | 0.264 | 0.289 | 0.265 |
| Number of cbsaid | 21,133 | | 21,133 | |
| Number of id | | 25,738 | | 25,738 |

Note: Dependent variable is log value of monthly wages. Robust standard errors, corrected for city year clustering, are in parentheses. (*** p<0.01, ** p<0.05, * p<0.1).

**Table 3.11**: Effect of Changes in Share of College Graduates on Log Monthly Rents: Within City Moves Excluded

| Variables | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| % BA+ | 2.790*** | 2.953*** | 2.241*** | 2.472*** |
| | (0.458) | (0.567) | (0.470) | (0.580) |
| % BA+ * High School | | | 0.365*** | 0.362** |
| | | | (0.132) | (0.146) |
| % BA * Some College | | | 0.526*** | 0.399** |
| | | | (0.140) | (0.157) |
| % BA * College | | | 1.153*** | 1.023*** |
| | | | (0.153) | (0.180) |
| Individual Control | Y | Y | Y | Y |
| City Control | Y | Y | Y | Y |
| Individual FE | Y | | Y | |
| City FE | Y | | Y | |
| Individual × City FE | | Y | | Y |
| Year FE | Y | Y | Y | Y |
| Observations | 40,371 | 40,371 | 40,371 | 40,371 |
| R-squared | 0.285 | 0.239 | 0.286 | 0.240 |
| Number of cbsaid | 12,919 | | 12,919 | |
| Number of id | | 15,097 | | 15,097 |

Note: Dependent variable is log value of monthly rent Robust standard errors, corrected for city year clustering, are in parentheses. (*** p<0.01, ** p<0.05, * p<0.1).

**Table 3.12**: Effect of Changes in Share of College Graduates on Rent-Income Distribution Within City Moves Excluded

| Dependent variable: ln (Rent to Income) | | | | |
|---|---|---|---|---|
| Variables | (1) | (2) | (3) | (4) |
| % BA+ | 2.430*** | 2.195** | 2.573*** | 2.471** |
| | (0.850) | (0.988) | (0.869) | (1.010) |
| % BA+ * High School | | | 0.059 | -0.029 |
| | | | (0.205) | (0.220) |
| % BA * Some College | | | -0.185 | -0.476* |
| | | | (0.249) | (0.269) |
| % BA * College | | | -0.364 | -0.507 |
| | | | (0.279) | (0.315) |
| Individual Control | Y | Y | Y | Y |
| City Control | Y | Y | Y | Y |
| Individual FE | Y | | Y | |
| City FE | Y | | Y | |
| Individual × City FE | | Y | | Y |
| Year FE | Y | Y | Y | Y |
| Observations | 37,278 | 37,278 | 37,278 | 37,278 |
| R-squared | 0.037 | 0.017 | 0.037 | 0.017 |
| Number of cbsaid | 12,939 | | 12,939 | |
| Number of id | | 15,123 | | 15,123 |

Note: Robust standard errors, corrected for city year clustering, are in parentheses. (*** $p<0.01$, ** $p<0.05$, * $p<0.1$).

**Table 3.13**: Effect of Changes in Share of College Graduates on Rent-Income Distribution Within City Moves Excluded

| Dependent variable: ln (Residual Income) | | | | |
|---|---|---|---|---|
| Variables | (1) | (2) | (3) | (4) |
| % BA+ | -0.271 | 0.671 | -0.731 | 0.093 |
| | (0.766) | (0.907) | (0.798) | (0.939) |
| % BA+ * High School | | | 0.051 | 0.190 |
| | | | (0.211) | (0.216) |
| % BA * Some College | | | 0.300 | 0.597** |
| | | | (0.243) | (0.251) |
| % BA * College | | | 1.252*** | 1.381*** |
| | | | (0.288) | (0.317) |
| Individual Control | Y | Y | Y | Y |
| City Control | Y | Y | Y | Y |
| Individual FE | Y | | Y | |
| City FE | Y | | Y | |
| Individual × City FE | | Y | | Y |
| Year FE | Y | Y | Y | Y |
| Observations | 39,493 | 39,493 | 39,493 | 39,493 |
| R-squared | 0.177 | 0.154 | 0.178 | 0.155 |
| Number of cbsaid | 13,301 | | 13,301 | |
| Number of id | | 15,777 | | 15,777 |

Note: Robust standard errors, corrected for city year clustering, are in parentheses. (*** p<0.01, ** p<0.05, * p<0.1).

**Table 3.14**: Effect of Changes in Share of College Graduates on Home Equity  Home Owners

| Variables | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| % BA+ | 4.903*** | 5.206*** | 4.192*** | 4.497*** |
| | (0.713) | (0.725) | (0.742) | (0.754) |
| % BA+ * High School | | | 0.026 | 0.0101 |
| | | | (0.181) | (0.185) |
| % BA+ * Some College | | | 0.146 | 0.194 |
| | | | (0.196) | (0.199) |
| % BA+ * College (BA+) | | | 0.892*** | 0.912*** |
| | | | (0.197) | (0.200) |
| Individual Control | Y | Y | Y | Y |
| City Control | Y | Y | Y | Y |
| Individual FE | Y | | Y | |
| City FE | Y | | Y | |
| Individual × City FE | | Y | | Y |
| Year FE | Y | Y | Y | Y |
| Observations | 63,611 | 63,611 | 63,611 | 63,611 |
| R-squared | 0.348 | 0.324 | 0.349 | 0.325 |
| Number of id | 10,071 | | 10,071 | |
| Number of cbsaid | | 11,058 | | 11,058 |

Note: Dependent variable is log value of income minus rent. Robust standard errors, corrected for city year clustering, are in parentheses. (*** p<0.01, ** p<0.05, * p<0.1).

These findings are in line with our main findings that show college graduates gain greater welfare from increasing numbers of college graduates. College graduate renters receive higher wage growth than rent growth. College graduate owners also experience an increase in their housing wealth, as home prices rises in response to the rising college share. The less skilled, on the other hand, are not only less likely to be owners, but even when they do own, they also experience a smaller increase in home equity from living in cities where college share is rising.

***Amenities*** While our theoretical model assumes amenities to be fixed, Diamond (2016) points out the share of college graduates has an endogenous effect on urban amenities. Her study shows that amenities improve more in cities with higher proportions of college graduates. While residual income of low skilled worker does not increase in these cities, they may gain utility from improved urban amenities which arise in the presence of college graduates. While studies have suggested that urban amenities are normal goods (Costa and Kahn 2000, Diamond 2016), there is little empirical evidence that supports this argument. While it is unclear why low skilled workers would gain greater benefit from the improvement in urban amenities than the high skilled workers, we partially address this issue by examining two of the major amenities in urban life, restaurants and schools. Diamond's work shows that eating and drinking places increased significantly with the increase in college share. Government spending per K-12 students also increased more in cities with greater shares of college graduates.

As the PSID provides information on how much households spend monthly for eating out, we first use the variable to examine whether there are changes in the households' spending on eating out in cities where the share of college graduates is increasing. Table 3.15 shows that, on average, increasing numbers of college graduates does not affect households' amount spent eating out. However, columns (3) and (4) show that eating out increases for only high skilled workers in cities where college graduates are growing in share. This is in line with the results in Table 3.7, which shows only high skilled workers had residual income increases in cities where college share rises. With no increase in income to spend, the low skilled workers are not likely to adjust their consumption in restaurants, and hence do not benefit from having more restaurants.

While less educated populations are not enjoying the improvement in their dining options, they may still be receiving greater utility gains from other amenity improvement. For example, the children of low skilled parents gain greater long-term benefits in future employment and higher wages from enrolling in better schools. While it is not possible to investigate this long-term outcome, we do examine whether living in cities with increasing share of college graduates has a positive impact on children's school enrollment. To examine this hypothesis, we link children data in the PSID to the parent data and test whether children between ages 16 and 24 are more likely to be enrolled in school if they reside in cities where the share of college graduates are rising. Because the vast majority of 16-24 year olds remain in school, it is difficult to identify regressions using individual fixed effects (many individuals have all "ones" or all "zeros" in the enrolled in school category, which makes it collinear with a fixed effect) . However, to control for the age effect on school enrollment, we include child's age in all our regressions. We also include controls for the level of education for heads as well as black and Hispanic dummies.

Column (5) and (6) in Table 3.15 presents the results of the likelihood of child's school enrollment using a logit regression, where the dependent variable equals 1 if the child is enrolled in school. Column (5) shows that children living in cities that attract college graduates do not have higher likelihoods of being enrolled in school. Furthermore, column (6) shows that the likelihood of school enrollment in response to higher college share is insignificant, regardless of parent's educational level. While we show that the less educated do not benefit from amenity improvements in two contexts, our findings may arise from having small samples, especially in the examination of children's school enrollment. Furthermore, we cannot completely rule out the possibility that skilled and unskilled workers are gaining different benefits from other forms of urban amenities. As our study has limited evidence, further research is required to identify who benefits the most from the changing amenities.

**Table 3.15**: Effect of Changes in Share of College Graduates on Eating out & School Enrollment

| | Monthly Eating Out | | | | School Enrollment 16-24 | Enrollment Children |
|---|---|---|---|---|---|---|
| Variables | (1) | (2) | (3) | (4) | (5) | (6) |
| % BA+ | 0.105 | 0.548 | -0.290 | 0.144 | 2.451 | 3.248 |
| | (0.373) | (0.404) | (0.386) | (0.420) | (4.107) | (4.488) |
| % BA+ * High School | | | -0.149 | -0.150 | | -4.190** |
| | | | (0.092) | (0.096) | | (1.704) |
| % BA+* Some College | | | 0.170* | 0.293*** | | 1.512 |
| | | | (0.101) | (0.107) | | (2.272) |
| % BA+ * College (BA+) | | | 0.724*** | 0.735*** | | 2.261 |
| | | | (0.122) | (0.135) | | (2.475) |
| Individual Control | Y | Y | Y | Y | Y | Y |
| City Control | Y | Y | Y | Y | Y | Y |
| Individual FE | Y | | Y | | Y | |
| City FE | Y | | Y | | Y | |
| Individual × City FE | | Y | | Y | | Y |
| Year FE | Y | Y | Y | Y | Y | Y |
| Observations | 112,609 | 112,609 | 112,609 | 112,609 | 33,518 | 32,398 |
| R-squared | 0.141 | 0.135 | 0.141 | 0.135 | | |
| Number of cbsaid | 18,562 | | 18,562 | | | |
| Number of id | | 22,328 | | 22,328 | | |

Note: Dependent variable is log monthly amount spend eating out. Individual control includes race, head, sex, marital status, age, experience. City control Variables include the share of black and Hispanic population, and logpopulation for each city. Robust standard errors, corrected for city year clustering, are in parentheses. (*** p<0.01, ** p<0.05, * p<0.1).

***Housing Supply Elasticity*** Finally, we test if our results are driven by cities with low housing supply elasticity. Since the housing supply is more likely to be less responsive to changes in the demand in these cities, housing cost may rise faster in places. According to Gyourko et al (2013), cities with inelastic housing supply and greater preference for the location experienced greater increases in house prices over the past 50 years. These places also experience a greater increase in the share of high income households.

We spilt our sample into groups and test whether rent burden and residual income response to the changes in college share in cities differs between cities with high and low housing supply. The housing supply elasticity data comes from Saiz (2010). This index incorporates information of land availability and local regulations and creates a single measure of how difficult it is to build new housing in a city.

The results in Table 3.16 and 3.17 compare changes in rent to income ratio and changes in residual income between two groups of cities. As expected, rent burden increases more significantly in cities where housing supply is less elastic. In fact, in cities with high housing supply elasticity, we find that rent burdens do not increase. The net residual income also decreases only in cities where housing supply elasticity is lower. The results suggest that our finding is mainly driven by cities where housing supply is inelastic, as housing supply does not respond quickly to the increases in housing demand, including increases arising from rising college share. Meanwhile, both tables present similar patterns between the two groups of cities: High skilled workers experience a relatively small increase in rent burden and large increase in residual wages compared to the low skilled workers. Again, this suggests there are significant differences in the welfare gains across groups with different educational attainment in response to rising college share.

## 3.5 Discussion

Our study examines how changes in the college share can alter wage and rent distributions within a city. This is different from Moretti (2013) who shows if housing costs are incorporated the

**Table 3.16**: Effect of Changes in Share of College Graduates on Rent-Income Distribution Cities with High or Low Housing Supply Elasticity

| Dependent variable: ln (Rent to Income) | | | | |
|---|---|---|---|---|
| | High Housing Supply Elasticity | | Low Housing Supply Elasticity | |
| Variables | (1) | (2) | (3) | (4) |
| % BA+ | 1.153 | 1.786 | 6.173*** | 6.185*** |
| | (1.456) | (1.591) | (1.407) | (1.515) |
| % BA+ * High School | 0.256 | 0.356 | -0.0895 | -0.241 |
| | (0.298) | (0.311) | (0.223) | (0.226) |
| % BA * Some College | 0.316 | 0.406 | -0.512* | -0.794*** |
| | (0.328) | (0.347) | (0.284) | (0.293) |
| % BA * College (BA+) | -0.619 | -0.377 | -0.592* | -0.641* |
| | (0.391) | (0.421) | (0.335) | (0.355) |
| Individual Control | Y | Y | Y | Y |
| City Control | Y | Y | Y | Y |
| Individual FE | Y | | Y | |
| City FE | Y | | Y | |
| Individual×City FE | | Y | | Y |
| Year FE | Y | Y | Y | Y |
| Observations | 22,730 | 22,730 | 23,211 | 23,211 |
| R-squared | 0.032 | 0.014 | 0.022 | 0.017 |
| Number of cbsaid | 6,672 | | 6,544 | |
| Number of id | | 7,343 | | 7,046 |

Note: Individual control includes race, head, sex, marital status, age, experience. City control Variables include the share of black and Hispanic population, and log population for each city. Robust standard errors, corrected for city year clustering, are in parentheses. (*** p<0.01, ** p<0.05, * p<0.1).

**Table 3.17**: Effect of Changes in Share of College Graduates on Rent-Income Distribution Cities with High or Low Housing Supply Elasticity

| Dependent variable: ln (Residual Income) | | | | |
|---|---|---|---|---|
| | High Housing Supply Elasticity | | Low Housing Supply Elasticity | |
| Variables | (1) | (2) | (3) | (4) |
| % BA+ | -0.627 | -1.432 | -2.974** | -2.401 |
| | (1.352) | (1.475) | (1.397) | (1.496) |
| % BA+ * High School | -0.026 | -0.08‘ | 0.099 | 0.299 |
| | (0.263) | (0.266) | (0.235) | (0.234) |
| % BA * Some College | 0.239 | 0.194 | 0.565** | 1.024*** |
| | (0.278) | (0.287) | (0.268) | (0.269) |
| % BA * College (BA+) | 1.572*** | 1.185*** | 1.390*** | 1.862*** |
| | (0.355) | (0.352) | (0.358) | (0.372) |
| Individual Control | Y | Y | Y | Y |
| City Control | Y | Y | Y | Y |
| Individual FE | Y | | Y | |
| City FE | Y | | Y | |
| Individual×City FE | | Y | | Y |
| Year FE | Y | Y | Y | Y |
| Observations | 24,429 | 24,429 | 24,170 | 24,170 |
| R-squared | 0.161 | 0.142 | 0.141 | 0.135 |
| Number of cbsaid | 6,907 | | 6,679 | |
| Number of id | | 7,643 | | 7,224 |

Note: Individual control includes race, head, sex, marital status, age, experience. City control Variables include the share of black and Hispanic population, and log population for each city. Robust standard errors, corrected for city year clustering, are in parentheses. (*** $p<0.01$, ** $p<0.05$, * $p<0.1$).

real wage inequality (measured by residual wage) between skilled and unskilled workers decreases. As skilled workers are more likely to reside in high cost cities, including housing costs can indeed reduce inequality, as Moretti (2013) suggests. However, as moving is costly, low income households do not immediately move to a cheaper location, as housing becomes more expensive in areas where the college share is rising. Thus, inequality *within* a city can rise in cities that attracts college graduates, while the rise in college share in certain locations could lower aggregate welfare inequality at the national level. Furthermore, as the educated are more likely to be homeowners and experience greater increases in housing wealth in high skilled cities, it is unclear how the concentration of college workers in certain cities is affecting the welfare distribution at the national level, if we also include changes in housing wealth into welfare calculations.

Our findings suggest that policy makers need to systematically address the distributional consequences arising from human capital externalities. Policies need to simultaneously consider changes occurring both in the labor and the housing market from the rising college share.

Providing housing subsidies to low income households could be one solution to mitigate the widening welfare distribution in cities attracting skilled workers. This could help less educated workers gain wage benefits from living in cities with higher college share without experiencing substantial rent increase. The policy can also help less educated workers accessing high skilled cities where there could be more job opportunities and better urban amenities. However, currently, only about 28 percent of those eligible for housing subsidies receive such subsidies (Leopold et al., 2010), which is small to have an impact on distributional outcomes.

Since the financial crisis, housing supply has become more constrained. As land and construction costs increase, the number of new housing starts has fallen dramatically. Although the number has risen from 2011, the current level is still below the levels of the 1960s, when the US total population was 60 percent of what it is in 2018. Furthermore, as the costs of building increased, a greater portion of housing construction is occurring at the higher end of the market, which is driving up housing cost at the lower end of the market (Choi et al., 2018). Our study implies that restrictions in housing supply could further increase welfare gaps in cities that attract educated workers. Reforming zoning and land-use regulations to induce greater housing supply could be another possible solution to reduce welfare gaps in cities that attract high skilled workers.

## 3.6 Conclusion

By extending the Rosen-Roback framework, we show that increases in college share can lead to change income and residual income distributions within a city. In agreement with our theoretical model, this paper shows that costs and benefits arising from human capital externalities differ across different subsets of the population. For highly educated people, living in cities that attract college graduates raises wages more than rents, although the percent increase in rental cost is slightly higher than the percent increase in wages. For those without college degrees, not only do higher levels of college graduates in a city produces rent increases that are greater than wage increases in percentages, the rent increases completely offset wage increases. Our results show that, in percentage terms, the rent trickle down from an increase in college graduates is higher than the wage trickle down. In other words, rent spillovers from college share rise every bit as fast as wage spillovers.

Overall, our study finds that increasing college share favors high skilled over low skilled workers. In addition to the changes in wages, when we take into account the changes in housing cost and housing wealth, the welfare gap between the skilled and the unskilled further widens in cities with rising college shares. While both the high and the low skilled gain wage premiums from living in high skilled cities, because of increasing housing cost, high skilled cities may become less affordable for the low skilled. In the long term, this could further increase inequality across different educational groups.

## 3.7 Acknowledgement

Chapter 3 is coauthored with Jung Hyun Choi and Richard K. Green. The dissertation author was the primary author of this chapter. And this chapter, in full is currently begin prepared for submission for publication of the material.

# Reference


Acemoglu, D. (2002). Technical change, inequality, and the labor market. *Journal of Economic Literature*, 40(1):7–72.

Autor, D. H. and Dorn, D. (2013). The growth of low-skill service jobs and the polarization of the us labor market. *American Economic Review*, 103(5):1553–97.

Beaudry, P., Doms, M., and Lewis, E. (2010). Should the Personal Computer Be Considered a Technological Revolution? Evidence from U.S. Metropolitan Areas. *Journal of Political Economy*, 118(5):988–1036.

Berry, C. R. and Glaeser, E. L. (2005). The divergence of human capital levels across cities. *Papers in Regional Science*, 84(3):407–444.

Choi, J. H., Goodman, L., and Bai, B. (2018). Four ways todays high home prices affect the larger economy. Urban Institute.

Costa, D. L. and Kahn, M. E. (2000). Power Couples: Changes in the Locational Choice of the College Educated, 19401990. *The Quarterly Journal of Economics*, 115(4):1287–1315.

Diamond, R. (2016). The Determinants and Welfare Implications of US Workers' Diverging Location Choices by Skill: 1980-2000. *American Economic Review*, 106(3):479–524.

Ganong, P. and Shoag, D. (2017). Why has regional income convergence in the U.S. declined? *Journal of Urban Economics*, 102(C):76–90.

Glaeser, E. L., Kolko, J., and Saiz, A. (2001). Consumer city. *Journal of Economic Geography*, 1(1):27–50.

Glaeser, E. L., Saiz, A., Burtless, G., and Strange, W. C. (2004). The rise of the skilled city. *Brookings-Wharton Papers on Urban Affairs*, pages 47–105.

Goldin, C. and Katz, L. (2001). *Decreasing ( and then Increasing) Inequality in America: A Tale of Two Half Centuries*, pages 37–82. University of Chicago Press, Chicago.

Gyourko, J., Mayer, C., and Sinai, T. (2013). Superstar Cities. *American Economic Journal: Economic Policy*, 5(4):167–199.

Haurin, D. R. (1991). Income variability, homeownership, and housing demand. *Journal of Housing Economics*, 1(1):60 – 74.

Jacob, J. (1969). *The Economy of Cities*. New York: Vintage.

Katz, L. F. and Murphy, K. M. (1992). Changes in Relative Wages, 19631987: Supply and Demand Factors. *The Quarterly Journal of Economics*, 107(1):35–78.

Leopold, J., Getsinger, L., Blumenthal, P., Abazajian, K., and Jordan, R. (2010). The housing affordability gap for extremely low-income renters in 2013. Urban Institute.

Logan, J. R., Xu, Z., and Stults, B. J. (2014). Interpolating u.s. decennial census tract data from as early as 1970 to 2010: A longitudinal tract database. *The Professional Geographer*, 66(3):412–420. PMID: 25140068.

Marshall, A. (1890). *Principles of Economics*. London: Macmillan.

Massey, D. S., Rothwell, J., and Domina, T. (2009). The changing bases of segregation in the united states. *The ANNALS of the American Academy of Political and Social Science*, 626(1):74–90.

Moretti, E. (2004a). Estimating the social return to higher education: evidence from longitudinal and repeated cross-sectional data. *Journal of Econometrics*, 121(1):175 – 212. Higher education (Annals issue).

Moretti, E. (2004b). Workers' education, spillovers, and productivity: Evidence from plant-level production functions. *American Economic Review*, 94(3):656–690.

Moretti, E. (2011). *Handbook of Labor Economics*, volume 4, chapter Chapter 14 - Local Labor Markets, page 12371313. Elsevier.

Moretti, E. (2013). Real wage inequality. *American Economic Journal: Applied Economics*, 5(1):65–103.

Nevins, A. (1962). *The State Universities and Democracy*. University of Illinois Press, Champaign, IL.

Porter, M. (1990). *The Competitive Advantage of Nations*. New York: Free Press.

Roback, J. (1982). Wages, rents, and the quality of life. *Journal of Political Economy*, 90(6):1257–1278.

Rosen, S. (1979). *Wage-Based Indexes of Urban, Quality of Life*. Johns Hopkins Univ. Press.

Saiz, A. (2010). The Geographic Determinants of Housing Supply. *The Quarterly Journal of Economics*, 125(3):1253–1296.

Sinai, T. and Souleles, N. S. (2005). Owner-Occupied Housing as a Hedge Against Rent Risk. *The Quarterly Journal of Economics*, 120(2):763–789.

Williams, R. L. (1991). *The Origins of Federal Support for Higher Education*. The Pennsylvania State University Press, Pennsylvania.