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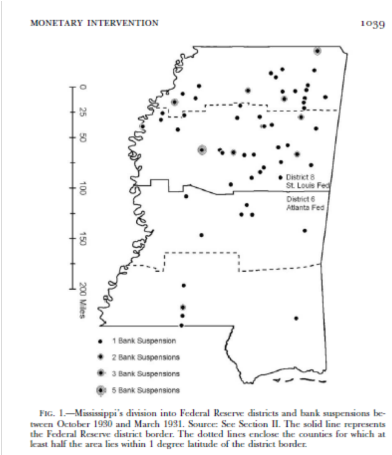
MONETARY INTERVENTION 1059

0 25 50 100 150 200 Miles

District 8 St. Louis Fed

District 6 Atlanta Fed

1 Bank Suspension
2 Bank Suspensions
3 Bank Suspensions
5 Bank Suspensions

FIG. 1.—Mississippi's division into Federal Reserve districts and bank suspensions between October 1930 and March 1931. Source: See Section II. The solid line represents the Federal Reserve district border. The dotted lines enclose the counties for which at least half the area lies within 1 degree latitude of the district border.

Macroeconomic Theory I

Section 7 - Evidence-based Macroeconomics

Daniele Girardi
University of Massachusetts Amherst

Spring 2021

This Section will draw heavily on:

- J. D. Angrist and J.-S. Pischke (2010) 'The Credibility Revolution in Empirical Economics: How Better Research Design is Taking the Con out of Econometrics'
  - Every econ PhD student should have read this paper.
- E, Nakamura and J. Steinsson (2018) 'Identification in Macroeconomics'

and provide examples from:

- Chorodow-Reich, Coglianese and Karabarbounis (2019) 'The macro effects of unemployment benefits extensions'
- Nakamura & Steinsson (2014) 'Fiscal Stimulus in a Monetary Union: Evidence from US Regions'
- Parker et al (2013) 'Consumer Spending and the Economic Stimulus Payments of 2008 '
- Chodorow-Reich (2014) 'The employment effects of credit market disruptions'

The case for an empirical turn in Macro

- Lucas critique was correct...
- ...but the research program it sparked led to arguably outlandish models;
  - Krugman: last 30 years of macro research "*spectacularly useless at best, and positively harmful at worst.*"
  - P. Romer: "*For more than three decades, macroeconomics has gone backwards*."

The case for an empirical turn in Macro

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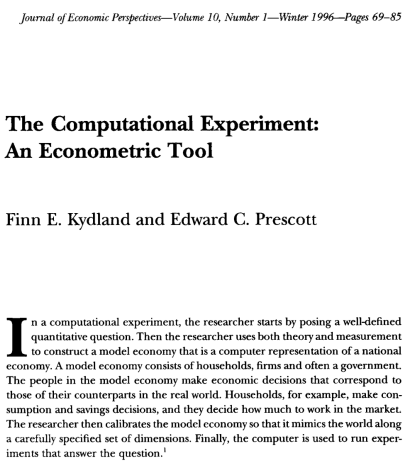
  - P. Romer: "*For more than three decades, macroeconomics has gone backwards*."
- One of the problems: general lack of interest in *direct* empirical evidence, in favor of 'computational experiments'.

The ‘computational experiments’ approach

- Lucas’ proposed solution to the Lucas’ critique;
- Lucas (1980): “*One of the functions of theoretical economics is to provide fully articulated, artificial economic systems that can serve as laboratories in which policies that would be prohibitively expensive to experiment with in actual economies can be tested out at much lower cost.*”

The 'computational experiments' approach

- Computational experiments:
  1. Choose a research question;
  2. build a (DSGE) theoretical model of the economy;
  3. 'calibrate' the model to match some aspect of the data;
  4. simulate the effect of changing some parameter within the model (e.g. tax rate or MP rule) to answer the original research question.



Journal of Economic Perspectives—Volume 10, Number 1—Winter 1996—Pages 69–85

The Computational Experiment: An Econometric Tool

Finn E. Kydland and Edward C. Prescott

In a computational experiment, the researcher starts by posing a well-defined quantitative question. Then the researcher uses both theory and measurement to construct a model economy that is a computer representation of a national economy. A model economy consists of households, firms and often a government. The people in the model economy make economic decisions that correspond to those of their counterparts in the real world. Households, for example, make consumption and savings decisions, and they decide how much to work in the market. The researcher then calibrates the model economy so that it mimics the world along a carefully specified set of dimensions. Finally, the computer is used to run experiments that answer the question.[1]

Federal Reserve Bank of Minneapolis
Quarterly Review Fall 1986

Theory Ahead of Business Cycle Measurement*

Edward C. Prescott
Adviser
Research Department
Federal Reserve Bank of Minneapolis
and Professor of Economics
University of Minnesota

An ‘evidence-based’ approach

- First emerged outside macro;
  - labor, education, ...
- focus on quality of empirical research designs;
- a key idea: isolate exogenous variation in the treatment of interest, rather than trying to account for all relevant right-hand variables;
  - randomized controlled trials (RCTs)
  - discontinuity-based identification
  - instrumental variables
  - diff-in-diff
- ‘empiricism’ has shifted the consensus on important topics
  - minimum wages
  - institutions and growth
  - immigration
  - ...

Towards evidence-based Macroeconomics?

- Pre-2008: Macro largely insulated from the 'credibility revolution' and the empirical turn of economics
  - (broadly speaking and with important exceptions)

- *"Macroeconomics has taken a turn towards theory in the last 10–15 years. Most young macroeconomists are more comfortable with proving theorems than with getting their hands on any data or speculating on current events."* (Ricardo Reis, 2008)

Towards evidence-based Macroeconomics?

- 2008-09 –> attempts to tease out causal connections between events in housing, credit & labor markets;
  - ‘computational experiments’ seemed inadequate for that task;
  - focus on credible identification strategies;
  - findings often scream for new theories;
- “*The theory-centric macro fortress appears increasingly hard to defend.*”(Angrist & Pischke, 2010)
- In the last years the tide turned into a flood.

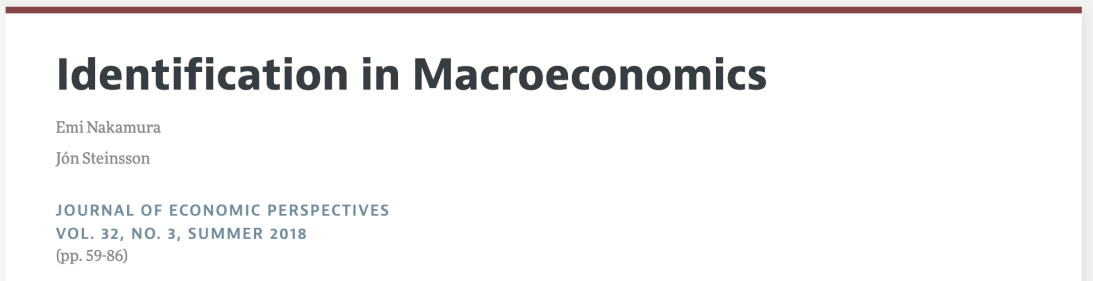
Identification in Macroeconomics

- Big obstacle: identification in Macro is especially hard
  - Hard to find exogenous variation in aggregate macro variables.

Identification in Macroeconomics

- Big obstacle: identification in Macro is especially hard
  - Hard to find exogenous variation in aggregate macro variables.
- Microdata and ideas borrowed from applied micro can help overcome this issue;
  - exploit cross-sectional variation, IVs and ‘natural experiments’;
  - micro/meso-evidence often informative about macro questions;
  - contrast with traditional VAR/structural approaches, which require specifiying a correct complete model for the DGP.

Identified moments



Identification in Macroeconomics

Emi Nakamura

Jón Steinsson

JOURNAL OF ECONOMIC PERSPECTIVES
VOL. 32, NO. 3, SUMMER 2018
(pp. 59-86)

- Causal effects as *'identified moments'*
- Traditional calibration
  - unconditional moments as portable statistics;
  - *can your macro model match simple variances and covariances of time-series data?*
- Identified moments
  - causal effects as portable statistics;
  - *are the causal relations implied by (a certain part of) your macro model consistent with evidence?*

Advantages of identified moments

- more power in rejecting unrealistic models;
- often able to tell you which particular (block of) assumptions fails.
- allows to leverage cross-sectional evidence with potentially low external validity or that does not directly identify aggregate effects of interest
- Example: the 'regional cross-sectional multiplier'
  - Not the aggregate multiplier that we (usually) really care about
  - NK and RBC theories can have similar implications for the aggregate fiscal multiplier when monetary policy fully reacts (low), but have very different implications for the regional cross-sectional multiplier!
  - using 'regional cross-sectional multiplier' as identified moment can be very informative about the aggregate fiscal multiplier under the ZLB.

Volume 134, Issue 1
February 2019

< Previous Next >

The Macro Effects of Unemployment Benefit Extensions: a Measurement Error Approach

Gabriel Chodorow-Reich, John Coglianese, Loukas Karabarbounis

The Quarterly Journal of Economics, Volume 134, Issue 1, February 2019, Pages 227–279,
https://doi.org/10.1093/qje/qjy018
Published: 20 August 2018

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Abstract

By how much does an extension of unemployment benefits affect macroeconomic outcomes such as unemployment? Answering this question is challenging because U.S. law extends benefits for states experiencing high unemployment. We use data revisions to decompose the variation in the duration of benefits into the part coming from actual differences in economic conditions and the part coming from measurement error in the real-time data used to determine benefit extensions. Using only the variation coming from measurement error, we find that benefit extensions have a limited influence on state-level macroeconomic outcomes. We apply our estimates to the increase in the duration of benefits during the Great Recession and find that they increased the unemployment rate by at most 0.3 percentage point.

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Volume 134, Issue 1

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- Q: *What is the effect of increasing the generosity of unemployment insurance (UI) benefits on the unemployment rate?*
- Reverse causality is a big challenge
  - UI typically made more generous in downturns
  - For US states, Federal law links *mechanically* max duration of UI to state-wide unemployment rate.
- Idea: exploit state-level variation caused by measurement error in official unemployment rata.
  - Subsequent data revisions allow to identify measurement errors that may have triggered UI extensions.

Example: Louisiana vs. Wisconsin in Apr 2013

- 2008 emergency compensation program:
  UI extended by 14 more weeks if state unemployment > 6%;
- Real-time data: Louisiana does not cross threshold, Wisconsin does.
- Later turns out they actually had the same unemployment.

TABLE I

APRIL 2013 EXAMPLE

		Louisiana	Wisconsin
Real-time data	Unemployment rate (moving average)	5.9%	6.9%
	duration of benefit extensions	14 weeks	28 weeks
Revised data	Unemployment rate (moving average)	6.9%	6.9%
	duration of benefit extensions	28 weeks	28 weeks
	UI error	−14 weeks	0 weeks

Separating 'real' unemployment from measurement error

- Observed unemployment insurance (UI) duration:

$$T^*_{s,t} = f_{s,t}(u^*_{s,t}) \quad \text{with} \quad u^*_{s,t} = u_{s,t} + \hat{u}_{s,t} \tag{1}$$

- Hypothetical 'error-free' UI duration:

$$T_{s,t} = f_{s,t}(u_{s,t}) \tag{2}$$

  - calculated empirically by taking subsequent revised data as proxy for $u_{s,t}$

- Use the component of UI duration that depends only on measurement error as the exogenous regressor

$$y_{t,s} = \alpha + \beta \hat{T}_{s,t} \quad \text{with} \quad \hat{T} = T^* - T \tag{3}$$

- (Note: this is a simplification of what they actually do - they have more dynamic specification and identify 'innovations' in $\hat{T}$...but this conveys the idea and is sufficient for our purposes.)

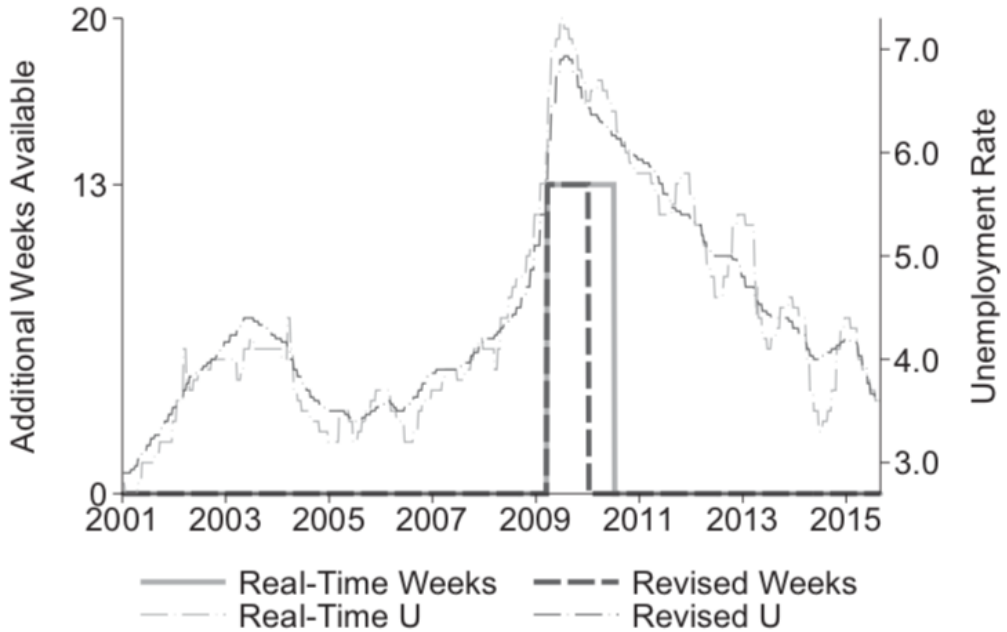

FIGURE I

Extended Benefits and Unemployment in Vermont

The figure plots the actual duration of benefits $T^*_{s,t}$ and the duration based on the revised data $T_{s,t}$ (left axis) together with the real-time $u^*_{s,t}$ and revised unemployment rates $u_{s,t}$ (right axis).

- *Vermont*: Under revised data, UI extension should have been discontinued at beginning of 2010.
- However, under real-time data, it remained in place until mid-2010.

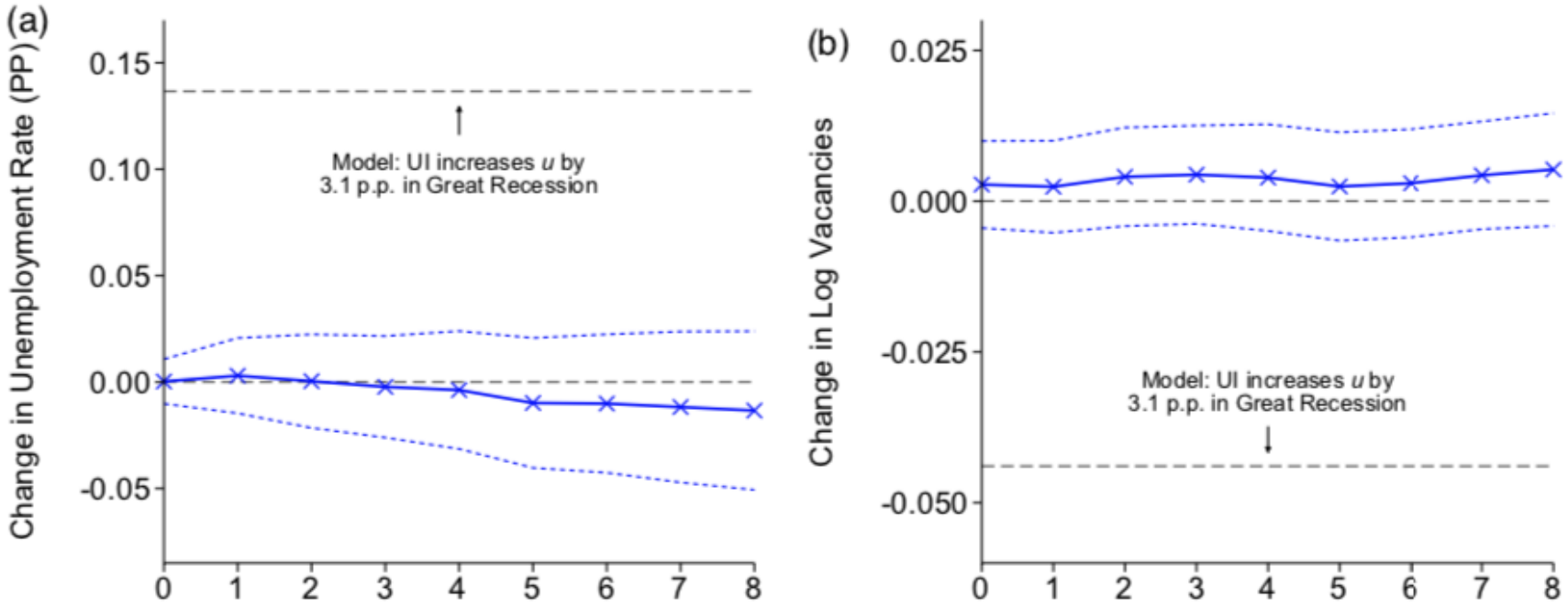

FIGURE III

Impulse Responses of Unemployment Rate and Log Vacancies

The figure plots the coefficients on $\epsilon_{s,t}$ from the regression $y_{s,t+h} = \beta(h)\epsilon_{s,t} + \sum_{j=1}^{12} \gamma_j(h) u_{s,t-j} + d_s(h) + d_t(h) + v_{s,t+h}$, where $y_{s,t+h} = u_{s,t+h}$ is the unemployment rate (left panel) or $y_{s,t+h} = \log v_{s,t+h}$ is log vacancies (right panel). The dashed lines denote the 90% confidence interval based on two-way clustered standard errors.

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Some remarks

- *Key identification assumptions*
  1. Measurement errors unrelated to 'true' economic activity;
  2. Revised data closer to the truth than real-time data;
- *Findings*
  - little/no effect on *u* and other labor market variables.
- *How does this provide an identified moment*?
  - Authors write a search-and-match model with unemployment insurance;
  - identify the assumption that determines whether UI affects unemployment: opportunity cost of working;
  - opportunity cost of working must be low and UI a small part of it, to match the (null) effects.

Volume 129, Issue 1
February 2014

Article Contents

EDITOR'S CHOICE

The Employment Effects of Credit Market Disruptions: Firm-level Evidence from the 2008–9 Financial Crisis *

FREE

Gabriel Chodorow-Reich

The Quarterly Journal of Economics, Volume 129, Issue 1, February 2014, Pages 1–59,
https://doi.org/10.1093/qje/qjt031
Published: 15 October 2013

PDF Split View Cite Permissions Share ▾

Abstract

This article investigates the effect of bank lending frictions on employment outcomes. I construct a new data set that combines information on banking relationships and employment at 2,000 nonfinancial firms during the 2008–9 crisis. The article first verifies empirically the importance of banking relationships, which imply a cost to borrowers who switch lenders. I then use the dispersion in lender health following the Lehman crisis as a source of exogenous variation in the availability of credit to borrowers. I find that credit matters. Firms that had precrisis relationships with less healthy lenders had a lower likelihood of obtaining a loan following the Lehman bankruptcy, paid a higher interest rate if they did borrow, and reduced employment by more compared to precrisis clients of healthier lenders. Consistent with frictions deriving from asymmetric information, the effects vary by firm type. Lender health has an economically and statistically significant effect on employment at small and medium firms, but the data cannot reject the hypothesis of no effect at the largest or most transparent firms. Abstracting from general equilibrium effects, I find that the withdrawal of credit accounts for between one-third and one-half of the employment decline at small and medium firms in the sample in the year following the Lehman bankruptcy.

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  - Informative about finance-macro links;
- Exploits the large 2008-09 shock
  - shock originated *outside* the non-FIRE corporate sector;
  - uses matched firm-level data on loans and employment;

Chodorow-Reich (2014)

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  - shock originated *outside* the non-FIRE corporate sector;
  - uses matched firm-level data on loans and employment;
- *Findings*: credit-market disruptions reduced credit availability and employment at non-financial firms;
  - firms with pre-crisis relations with lenders damaged by the crisis received less loans and cut employment more during the crisis, relative to other firms.

Why did a financial crisis cause a Great Recession?

- State-of-the-art DSGE models provided little guidance...
- ...so people had to get their hands dirty with data;
- Mian and Sufi: 'household-finance channel'
  - declines in household net wealth caused sharp decrease in household consumption;
- here another channel: firms cut employment because of decreased credit availability;

Data

- Syndicated loan market;
- Individual loans (Dealscan);
- Bank characteristics (Fed reports, Bankscope, CRSP);
- Firm-level employment data (BLS longitudinal database (LBD) - confidential);
- Matches loan & employment data at firm-level.

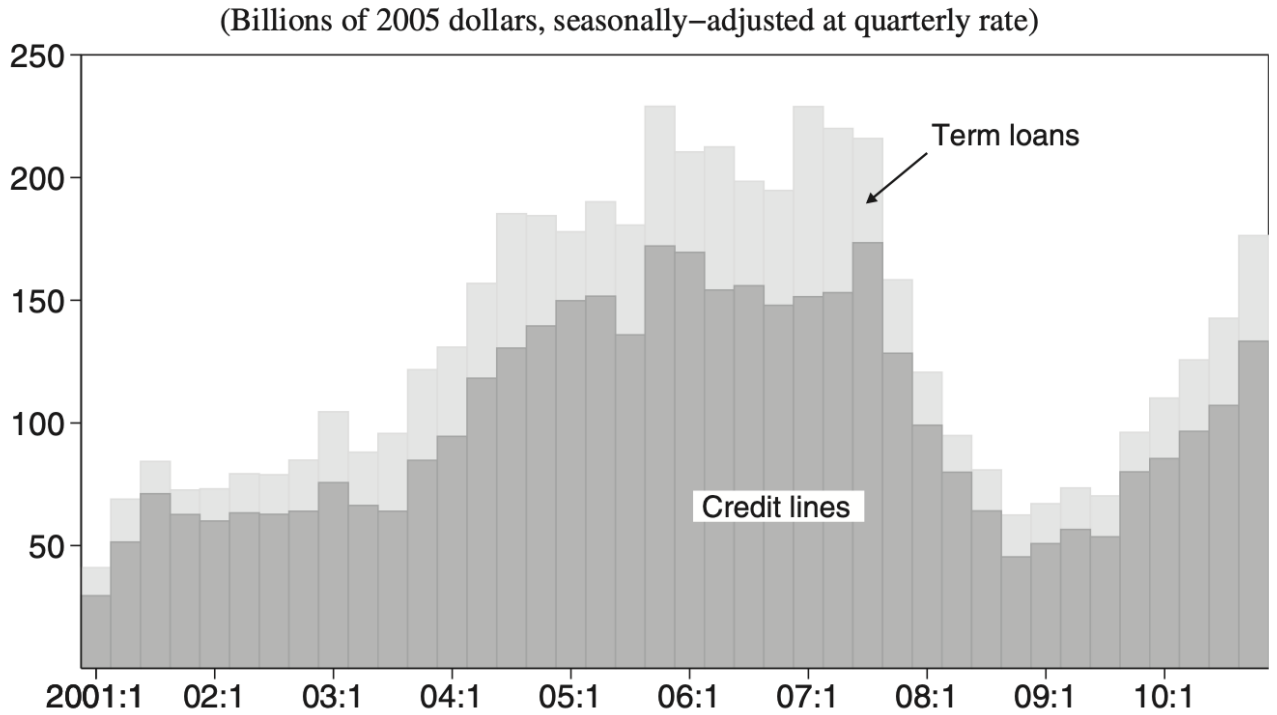

FIGURE II

Aggregate New Lending from Top 43 Lenders

The figure shows the face value of new loans to non-FIRE borrowers for working capital or general corporate purposes in which one of the 43 most active lenders had a lead role. Values seasonally adjusted by author using Census-X12.

Premise: 'Sticky' banking relations

- Borrowers (firms) & lenders (banks) form durable relationships.
  - Asymmetric information → moral hazard/adverse selection.
- Test for sticky banking relations:

$$\begin{aligned} Lead_{b,i} = \alpha_b &+ \gamma_1\left[Previous\ lead_{b,i}\right] \\ &+ \gamma_2\left[Previous\ participant_{b,i}\right] \\ &+ \gamma_3\left[Previous\ lead_{b,i}\ X\ Public\ (Unrated)\right] \\ &+ \gamma_4\left[Previous\ lead_{b,i}\ X\ Rated\right] + \epsilon_{b,i}, \end{aligned} \tag{1}$$

where $Lead_{b,i} = 1$ if bank b serves as the lead bank for borrower i, and $Previous\ lead_{b,i} = 1$ if bank b served as the lead bank for i's previous loan. The estimated value of γ_1 is 0.71.

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- When a firm takes up a new loan, the bank that served previously as main lender has a 71 p.p. greater likelihood of being main lender also in the new loan (after controlling for a bank's market share).
  - Stronger if firm non-publicly traded and without credit rating;

Identification strategy

Possible 'structural' approach:

$$g^{y}_{i,s} = f(L_{i,s}, X_i, U_i, \epsilon_i)$$

- $g^{y}_{i,s}$ = employment growth at firm i, related to bank s;
- $L_{i,s}$ = indicator for whether i receives a loan from s during crisis;
- X_i = observable firm characteristics;
- U_i = unobservable firm characteristics;

$$L_{i,s} = h(R_s, X_i, U_i, \eta_i)$$

- R_s internal cost of funds at bank s;

- Employ R_s, as an instrument for $L_{i,s}$.
- Would work iff $U_i \perp R_s$.

Identification strategy

- Difficulties in implementing 'structural' approach empirically;
- So C-R considers reduced-form relation

$$g^{y}_{i,s} = g(M_s, X_i, U_i, \epsilon_i, \eta_i)$$

  - lender health → employment:
  - M_s = observable measure of *overall* loan supply of bank s;

- *Identification assumption:*

$$U_i \perp M_s$$

  - Plausible: disruptions did not come from corporate loans.
  - Robustness tests using IVs.

Identification strategy

- How to measure M_s?
  - % change in number of loans made by bank s to borrowers other than i between pre-crisis and crisis periods
  - $\frac{\text{Oct 2008-Jun 2009}}{\text{Oct 2005-Jun 2007}}$
  - For each firm, take average over previous lenders

Identification strategy

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  - % change in number of loans made by bank *s* to borrowers other than *i* between pre-crisis and crisis periods
  - $\frac{\text{Oct 2008-Jun 2009}}{\text{Oct 2005-Jun 2007}}$
  - For each firm, take average over previous lenders
- Instrumental variables (for robustness):
  1. Bank's exposure to Lehman Brothers
     - Ivashina & Scharfstein (2010)
  2. Exposure to subprime mortgages (ABX)
     - *loading factor*
  3. Bank balance-sheet items unrelated to corporate loans
     - trading account losses; real estate charge-offs; deposit/liabilities ratio.

Covariate balance

BALANCING OF COVARIATES IN THE SAMPLE

	Quantile of lender health				Memo: std. dev.
	1	2	3	4	
Mean employment change in Borrower's industry, 2008:3–2009:3	−0.086	−0.081	−0.085	−0.089	0.083
Borrower's county, 2008:3–2009:3	−0.056	−0.056	−0.056	−0.056	0.009
Share with bond market access	0.455	0.540	0.458	0.236	0.494
Share private, no bond market access	0.418	0.331	0.363	0.525	0.492
Share public, no bond market access	0.127	0.129	0.179	0.239	0.374
Mean all in drawn spread	266	155	156	199	133
Median sales at close ($2005 billions)	0.366	0.837	0.701	0.285	4.146
Mean year of last precrisis loan	2005.83	2005.98	2006.03	2006.05	1.50
Share with loan due during crisis	0.193	0.188	0.183	0.205	0.394

Notes. The table splits the sample into four quantiles based on the change in the annualized number of loans made by the borrower's last precrisis syndicate between the periods October 2005 to June 2007 and October 2008 to June 2009. Employment change by borrower industry computed at the four-digit SIC level using six-digit NAICS employment levels from the Quarterly Census of Wages and Employment and a SIC-NAICS concordance table available from the BLS. Employment change by borrower county computed by averaging the employment change in all counties in which a firm operates establishments using establishment employment shares as weights. The last column reports the standard deviation of the variable summarized in each row.

- *Takeaway:* Non-fire firms linked to troubled banks are no different from firms linked to healthy banks;

Findings

If pre-crisis lender reduces loan supply...

1. *firm is less likely to get a loan during crisis;*
    - and if it gets it, it pays a higher interest rate;

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   - employment at firms linked to very affected banks (10th pct) fell by 4 to 5 p.p. more than at firms linked to less affected banks (90th pct);

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   - employment at firms linked to very affected banks (10th pct) fell by 4 to 5 p.p. more than at firms linked to less affected banks (90th pct);

- *Heterogeneity:*
  - strong for small-medium firms with no access to bond market;
  - small and not-significant for large firms with access to bond market;
- Results are very similar when using the three instrumental variables (separately or together) for loan supply.

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- This channel may explain why employment fell much more at small & medium firms during crisis.

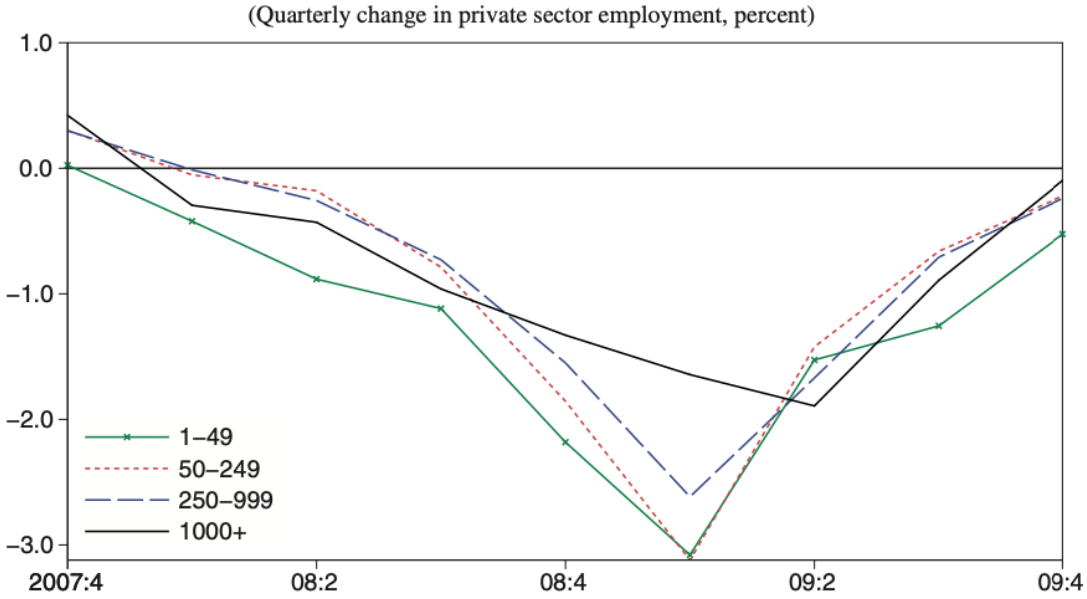

FIGURE III

Employment Losses by Firm Size

The figure shows the percent change in employment by firm size class. The numerator is the change in employment using a dynamic sizing methodology. The denominator is the average level of employment during the two quarters. The BLS only reports employment levels for the first quarter of each year; the denominators for intervening quarters are linearly interpolated.

Source: Bureau of Labor Statistics (Business Employment Dynamics).

How much did this matter for the Great Recession?

- Can we get to aggregate effects?
- Calculate two $\hat{g}^{y}_{i,s}$ for each firm
  - 1 based on actual credit supply M_s firm was facing.
  - 1 based on the M_s of most 'liberal' lenders (counterfactual).
- Ignore GE effects, assume sample is representative (big if!)
  - appendix model suggests that GE effects are negligible;
  - *external validity*? small firms in the sample seem likely to be more dependent on credit than other small firms.
- Then, credit-channel can explain between 1/3 and 1/2 of employment decline at small & medium firms during crisis;
  - between 1/5 and 1/3 overall.

Fiscal Stimulus in a Monetary Union: Evidence from US Regions

Emi Nakamura

Jón Steinsson

AMERICAN ECONOMIC REVIEW
VOL. 104, NO. 3, MARCH 2014
(pp. 753-92)

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Article Information

Abstract

We use rich historical data on military procurement to estimate the effects of government spending. We exploit regional variation in military build-ups to estimate an "open economy relative multiplier" of approximately 1.5. We develop a framework for interpreting this estimate and relating it to estimates of the standard closed economy aggregate multiplier. The latter is highly sensitive to how strongly aggregate monetary and tax policy "leans against the wind." Our open economy relative multiplier "differences out" these effects because monetary and tax policies are uniform across the nation. Our evidence indicates that demand shocks can have large effects on output.

UMASS AMHERST

Fiscal Stimulus in a Monetary Union: Evidence from US Regions

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- Use variation in State-level military procurement.
  - 'less endogenous' than public spending, but still endogenous;
  - IV strategy using differential impact of federal military buildups.
- open economy relative multiplier as an *identified moment*.
  - $\neq$ closed-economy aggregate multiplier;
  - powerful in discriminating among competing macro models

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Emi Nakamura

Jón Steinsson

- Q: *Government spending multiplier at the (US) State level.*
- Use variation in State-level military procurement.
  - 'less endogenous' than public spending, but still endogenous;
  - IV strategy using differential impact of federal military buildups.
- open economy relative multiplier as an *identified moment*.
  - $\neq$ closed-economy aggregate multiplier;
  - powerful in discriminating among competing macro models
- *Main finding:* open economy relative multiplier ≈ 1.6
  - consistent with NK models with large aggregate ZLB multiplier;
  - inconsistent with supply-dominated models;
  - demand shocks matter.

How large is the fiscal multiplier?

- Wide range of views.
- *Theoretically:* it depends
  - small in RBC models;
  - either small or large in NK models, mainly based on the MP rule.
- *Empirically:* aggregate time-series evidence inconclusive and based on heroic identification assumptions.
  - VAR models need to include all possible factors affecting Y and G;
  - time-series 'military build-up' literature relies heavily on WWII and Korean War, assumes war has no other economic impact;
  - no way to credibly control for monetary policy reaction function.
- Cross-regional variation in the impact of national military buildups can provide 'natural experiment'
  - filters out MP reaction function & changes in Fed taxes.

Identification

- Estimate the effect of a *relative* increase in Fed spending in a State on a State's *relative* output.
- Endogeneity
  - States may advocate for more Fed spending when they have high unemployment (↓ bias).
  - Strong economy may make State-based contractors more competitive (↑ bias);

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  - Strong economy may make State-based contractors more competitive (↑ bias);
- IV strategy:
  - Focus on military procurement spending.
  - When Fed military spending increases, some States get a disproportionate share for structural reasons.
  - Interaction of aggregate Federal military procurement with State FEs provides exogenous IV.
  - *Key assumption:* The US does not embark on a military buildup because states that receive a disproportionate amount of military spending are doing poorly relative to other states.

State-level military procurement data

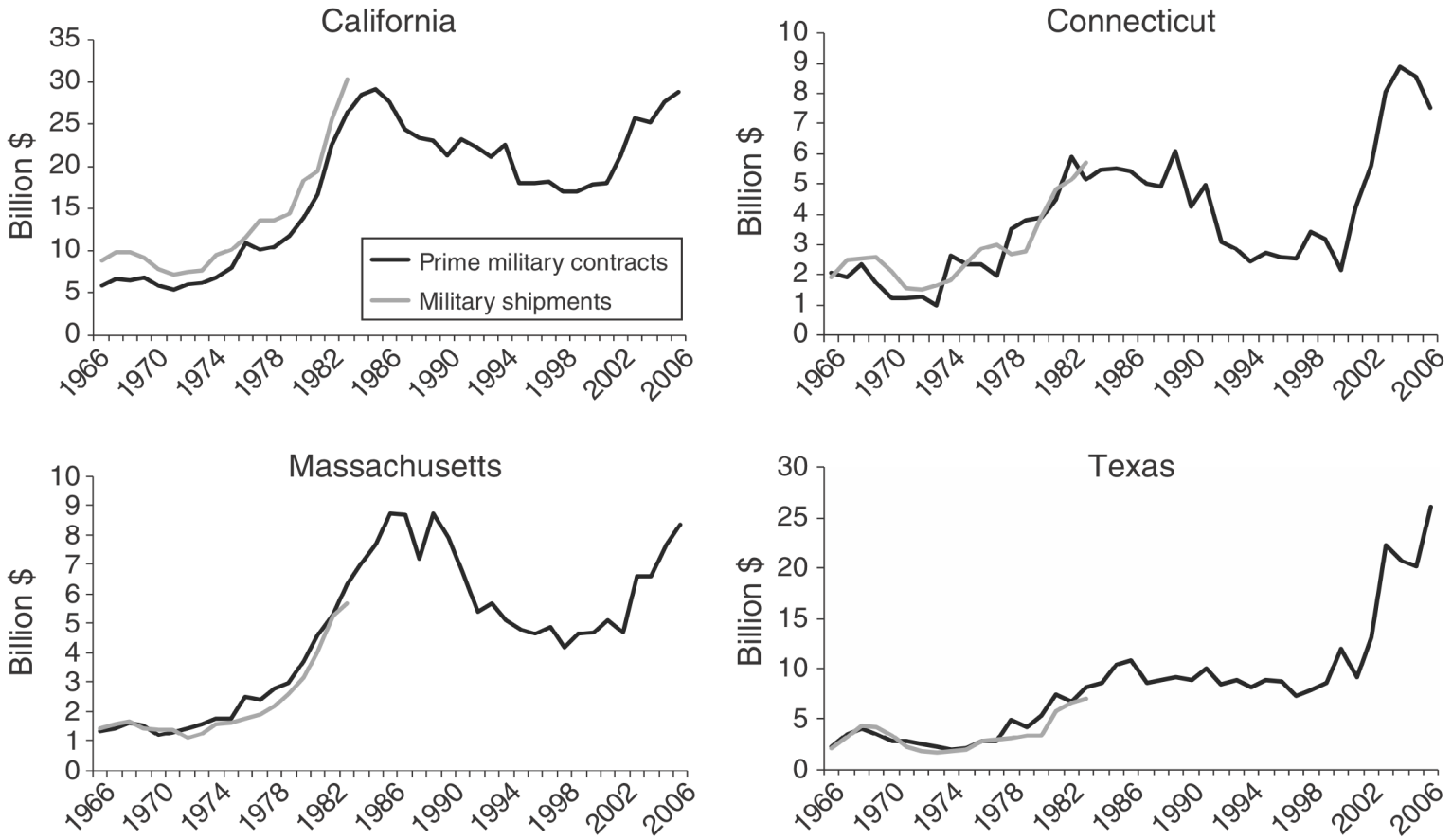

FIGURE 2. PRIME MILITARY CONTRACTS AND MILITARY SHIPMENTS

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State-level military procurement data

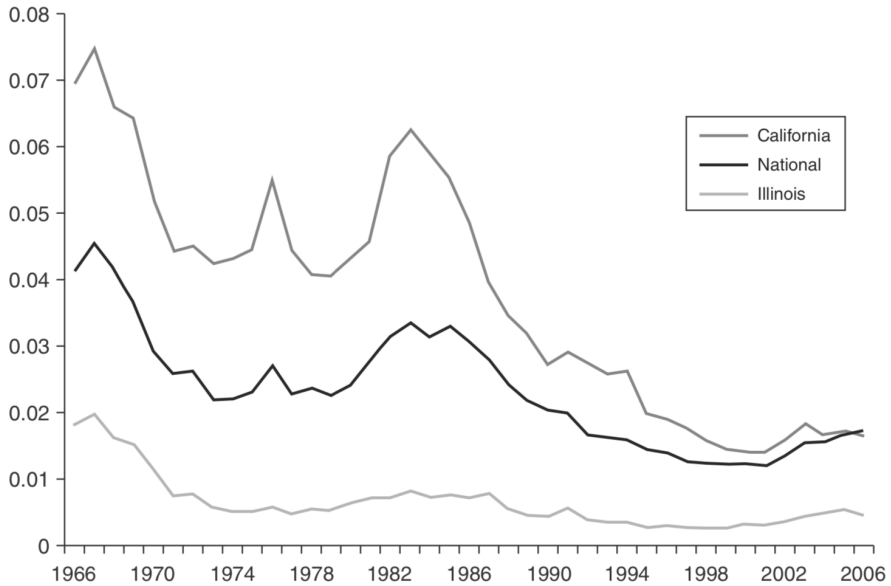

FIGURE 1. PRIME MILITARY CONTRACT SPENDING AS A FRACTION OF STATE GDP

- Some States (like CA, CT or MA) receive a systematically higher % of Federal military spending.
- When aggregate US military spending rises by 1 % of US GDP, Fed procurement rises on average by...
  - ...3 % of State GDP in CA
  - ...0.5% of State GDP in IL

Specification

- Main regression:

$$\frac{Y_{it} - Y_{it-2}}{Y_{it-2}} = \alpha_i + \gamma_t + \beta \frac{G_{it} - G_{it-2}}{Y_{it-2}} + \epsilon_{it} \tag{4}$$

  - Y_{it} = per capita output/employment in region i in year t;
  - G_{it} = government military procurement in region i in year t;

- $\frac{G_{it}-G_{it-2}}{Y_{it-2}}$ instrumented with interaction of total national procurement and state-level dummies.

Findings

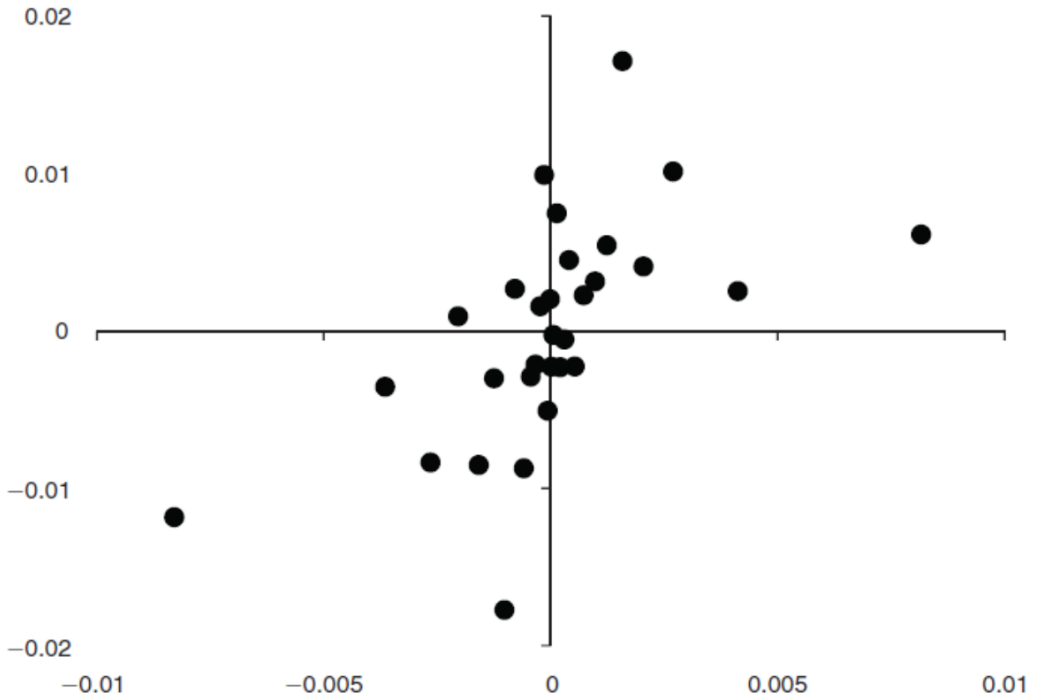

FIGURE 3. QUANTILES OF CHANGE IN OUTPUT VERSUS PREDICTED CHANGE IN MILITARY SPENDING

Notes: The figure shows averages of changes in output and predicted military spending (based on our first-stage regression), grouped by 30 quantiles of the predicted military spending variable. Both variables are demeaned by year and state fixed effects.

Findings

- *open economy relative multiplier (OERM)* = 1.5/1.6;
  - When *relative* per capita government purchases in a region rises by 1 percent of regional output, *relative* per capita output in that region rises by roughly 1.5 percent.
- *Interpretation:* like an export shock in a small open economy with fixed exchange rate.
- Can we draw implications about the *closed-economy aggregate multiplier* (CEAM)?
- Use the OERM as an *identified moment*.

The OERM as an identified moment

- DSGE model of a 2-States monetary union.
- Two main versions:
  1. separable preferences;
  2. GHH preferences (C and L as complements - more Keynesian).
- Different assumptions about pricing and MP rule:
  - sticky prices (Calvo) vs. flexible prices;
  - dovish vs. hawkish monetary policy (in sticky-price model).
- Estimate OERM and CEAM from the different versions, and see how they compare.

The OERM as an identified moment

TABLE 6—GOVERNMENT SPENDING MULTIPLIER IN SEPARABLE PREFERENCES MODEL

	Closed economy aggregate multiplier	Open economy relative multiplier
Panel A. Sticky prices		
Volcker-Greenspan monetary policy	0.20	0.83
Constant real rate	1.00	0.83
Constant nominal rate	∞	0.83
Constant nominal rate ($\rho_g = 0.85$)	1.70	0.90
Panel B. Flexible prices		
Constant income tax rates	0.39	0.43
Balanced budget	0.32	0.43

Notes: The table reports the government spending multiplier for output deflated by the regional CPI for the model presented in the text with the separable preferences specification. Panel A presents results for the model with sticky prices, while panel B presents results for the model with flexible prices. The first three rows differ only in the monetary policy being assumed. The fourth row varies the persistence of the government spending shock relative to the baseline parameter values. The fifth and sixth rows differ only in the tax policy being assumed.

The OERM as an identified moment

TABLE 7–GOVERNMENT SPENDING MULTIPLIER IN GHH MODEL

	Closed economy aggregate multiplier	Open economy relative multiplier
Panel A. Sticky prices		
Volcker-Greenspan monetary policy	0.12	1.42
Constant real rate	7.00	1.42
Constant nominal rate	∞	1.42
Constant nominal rate ($\rho_g = 0.50$)	8.73	2.04
Panel B. Flexible prices		
Constant income tax rates	0.00	0.30
Balanced budget	−0.18	0.30

Notes: The table reports the government spending multiplier for output deflated by the regional CPI for the model presented in the text with the GHH preferences specification. Panel A presents results for the model with sticky prices, while panel B presents results for the model with flexible prices. The first three rows differ only in the monetary policy being assumed. The fourth row varies the persistence of the government spending shock relative to the baseline parameter values. The fifth and sixth rows differ only in the tax policy being assumed.

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Consumer Spending and the Economic Stimulus Payments of 2008

Parker, Souleles, Johnson & McClelland (2013, AER)

- What is the MPC out of an *entirely anticipated* increase in income?
- Permanent income theory of consumption (as implicit in the Euler equation) predicts no effect;
- Liquidity constraints and/or limited rationality predict positive effects;
- Exploit random variation in the timing of 2008 fiscal stimulus payments across households;
- They find very sizable MPC out of anticipated income shocks.

The 2008 Economic Stimulus Payments (ESP)

- Winter 2007/2008: first signs of recession;
- Congress approves $150 billion Economic Stimulus Act (ESA);
- $100 billion is direct payments (tax rebates) to households (ESP);
- payments sent between April and July 2008;
- the precise month in which a household received the payment (by mailed check or electronic transfer) depends on last digits of SSN, *which are randomly assigned*;
- all households were sent beforehand (beginning of March) a letter telling them how much money they would receive;
- so in a given month you can compare households who already received the payment with households who yet have to receive it; and assignment to these two groups is random!

The 2008 Economic Stimulus Payments (ESP)

TABLE 1—THE TIMING OF THE ECONOMIC STIMULUS PAYMENTS OF 2008

Payments by electronic funds transfer		*Payments by mailed check*	
Last two digits of taxpayer SSN	Date ESP funds transferred to account by	Last two digits of taxpayer SSN	Date check to be received by
00–20	May 2	00–09	May 16
21–75	May 9	10–18	May 23
76–99	May 16	19–25	May 30
		26–38	June 6
		39–51	June 13
		52–63	June 20
		64–75	June 27
		76–87	July 4
		88–99	July 11

Source: Internal Revenue Service (http://www.irs.gov/newsroom/article/0,,id=180247,00.html).

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The Consumer Expenditure Survey (CE)

- Monthly survey of household expenditures;
- run by Bureau of Labor Statistics (BLS)
- 2,000 households interviewed each month (representative sample);
- in that period (June 2008 to March 2009) added question about having received any ESP payment, when, and how much;
- (authors worked with BLS to add the questions in order to write the paper!)

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Specification

$$C_{i,t+1} - C_{i,t} = \delta_t + \beta_1' X_{i,t} + \beta_2 ESP_{i,t+1} + u_{i,t+1} \quad (5)$$

- i indexes households; t indexes time;
- δ_t = time dummies (for each period in the monthly sample);
- C is households consumption expenditures;
- X is a vector of households-level controls;
- ESP is amount of stimulus payments received.
- β_2 = impact on spending of the (anticipated) arrival of an ESP.
  - It's causal because timing randomly assigned within the sample period;
  - but does not give the multiplier: it does not include any anticipated or lagged effects.

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Findings

- On average households spent more than half (50 to 90 percent) of the ESP in the same three-months period in which they received it;
- 12 to 30 percent in non-durables;
- the rest in durables (mainly cars and car repairs);
- MPC higher for lower income and older households;
- notably, previous study of 2001 tax rebates with similar (compelling) methodology found analogous results;

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The MPC out of anticipated income shock as an 'identified moment'

- causal estimate of the MPC out of an anticipated income shock clearly relates to the 'consumption block' of a macro model;
  - utility function & budget constraint of households;
- strong rejection of the permanent income hypothesis, and thus of the utility function used in all baseline macro models, and the resulting Euler equation!
- need liquidity constraints or limited rationality ('hand to mouth' consumers) to match this 'identified moment'
- Kaplan & Violante (2014) explicitly use these estimates to discriminate between different theoretical consumption models; their preferred one has credit constraints, illiquid assets and incomplete insurance market;
- Angeletos et al. (2001) argue that models in which households face self-control problems can help match the estimated MPC in the data.