

Monetary Policy and the Added Worker Effect*

John Leahy

University of Michigan & NBER

Tereza Ranošová

Deutsche Bundesbank

Abstract

We find that monetary shocks have weaker impact in states in which fewer married women work. Employment and total payroll fall by less and the unemployment rate rises by less. We argue that a plausible explanation is the “added worker effect” in which a weak labor market attachment of the secondary earner enables flexible labor supply adjustments that insure household demand. We provide three pieces of evidence to support this hypothesis. First, proxies for added worker effect behavior that we build from household panels state by state are negatively correlated with the share of married women working. Second, employment responds less to a monetary policy shock in states where these proxies are higher. Third, in states with a low share of married women working, more of them are drawn into the labor force after a negative shock.

Keywords: Added worker effect, intrahousehold insurance, monetary policy

JEL classification: J21, J11, E24, E52

*Contact address: Deutsche Bundesbank, Wilhelm-Epstein-Strasse 14, 60431 Frankfurt am Main, Germany. E-Mail: jvleahy@umich.edu, tereza.ranosova@bundesbank.de. We would like to thank Sara Casella, Mel Stephens, Aditi Thapar, Michele Tertilt, Ana Lamo and Patrick Hürtgen for helpful comments. We are grateful to the National Science Foundation for their support under grant SES-1919362. The views expressed in this paper are those of the author(s) and do not necessarily coincide with the views of the Deutsche Bundesbank or the Eurosystem.

1 Introduction

Macroeconomists typically assume that the economy is comprised of a representative agent or a collection of individual agents. Actual economies are comprised of families, and families can adjust to their economic environment in ways that individuals cannot. They can pool income and insure consumption. They can conserve resources by sharing ownership of durables such as homes and automobiles. They can adjust their work effort along both the extensive and intensive margin. They can more easily coordinate work effort across market and home production within a period and over time. The question arises whether these margins of adjustment matter for the behavior of aggregate quantities of interest to macroeconomists.

In this paper we argue that families matter. In particular, we show that the impact of monetary policy depends on the fraction of married women who work. The greater is this fraction, the greater is the impact of monetary policy on aggregate employment. A priori, it is not obvious how the employment of married women should affect monetary policy. On the one hand, women tend to work in industries that are less cyclically sensitive, so more married women working might indicate an economy that is less sensitive to shocks. On the other hand, married women may be less firmly attached to the labor market, so more of them working might make a state's employment more sensitive to shocks. We argue that neither of these intuitions explains our result. We show that the result survives when we control for industrial structure, and we show that when the fraction of married women choosing to work is high, married women are also more firmly attached to the labor market.

Instead, our proposed explanation relies on the “added worker effect” ([Lundberg, 1985](#); [Stephens, 2002](#); [Parker and Skoufias, 2004](#); [Juhn and Potter, 2007](#)), in which married women adjust their labor supply when the household is hit with a shock. Female labor supply is therefore a form of insurance for the family. Our story is that when the share of married women working is high, their attachment to the labor market is high and their employment decisions are similar to those of men. When the share is low, their attachment is also low. They are closer to the margin of working or not, or how much. Their decisions of whether to work respond to the needs of the family. In this way, the added worker effect mitigates the impact of aggregate shocks. Monetary policy shocks therefore have greater impact on the economy when the employment of married women is high and the added worker effect is weak.

We begin by presenting aggregate evidence on the relationship between the employment of married women and the impact of monetary policy. This evidence is interesting in its own right regardless of the underlying economic mechanism. We consider the impact of U.S. monetary shocks across U.S. states. To identify monetary policy shocks, we follow recent practice and consider changes in the federal funds rate over a short window around policy announcements. Our baseline runs from 1991 to 2019 and we repeat every result on the shorter sample that ends in 2008, before the federal funds rate hit the zero lower bound. We ask whether these shocks have greater impact on aggregate employment in a state when the share of married women that are employed in that state is large. We measure how much married women work in a state as the share of married women that are employed relative to the overall share of the state's population that is employed. This relative measure abstracts from state characteristics that impact the labor force partic-

ipation decisions of all demographic groups. We find that the impact of these shocks on employment is greater, the greater is the (relative) fraction of married women that were employed before the shock. Employment and total payroll fall further, and the unemployment rate rises more. The differences are large. One standard deviation of the employment share of married women across states is about 3 percentage points, and a state that much above the mean loses a further 3 percent of employment six quarters after the shock, about 60 percent of the response of national employment to the same shock.

One might ask why married women and not women in general. The relative employment share of married women is only weakly correlated with overall labor force participation, so it does not stand in for how much the state works. And when the relative employment share of unmarried women enters the regression alongside it, the coefficient on married women is unchanged, while the share of unmarried women has little effect on the impact of monetary policy. This is the reason we focus on married women.

While we have plausibly exogenous monetary shocks, our measures of employment of married women are endogenous. Lacking instruments, we do the next best thing and consider a number of confounding variables. We therefore control for specific state characteristics that evolve over time and have been shown to alter the impact of monetary policy. We consider variables related to the age structure of a state (Leahy and Thapar, 2022) since both marriage and work have a life-cycle component. We consider variables that reflect the industrial structure of a state (Carlino and DeFina, 1998) since working women may work in less cyclically sensitive industries. We also consider state income (Cravino, Lan, and Levchenko, 2020), since the need to insure against shocks to income may be correlated with the level of income. We consider variables that reflect the housing market (Beraja, Fuster, Hurst, and Vavra, 2019) since marriage and home ownership may be correlated. We also control for public insurance, which differs across states and may affect how much married women work, through the generosity of unemployment insurance (Boone, Dube, Goodman, and Kaplan, 2021) and the progressivity of the tax and transfer system (Fleck, Heathcote, Storesletten, and Violante, 2025). The impact of a monetary policy shock increases with the share of married women working even after controlling for these possible confounding effects. In some sense, this is not surprising since it turns out that the relative share of married women working is only weakly correlated with many of these state characteristics.

We also ask what drives the differences across states in how much married women work. We look for state characteristics that predict it and are slow moving, such as how educated wives are relative to their husbands and how much married women worked before our sample begins. Each of them delivers the baseline result when it takes the place of the employment share of married women in the regression. When the two enter together, the effect of the employment share of married women is unchanged and the predictor drops out. We take this to mean that our result is not driven by incidental business cycle variation in how much married women work, and that it is that share itself that matters.

We then turn to the added worker effect as a potential mechanism. It stands for household insurance through the labor supply of the secondary earner, and it requires married women who are marginal, close to the decision of whether to work. Where more married women work they are often attached to the labor market in much the same way men are. Thus, the employment share of married women is a plausible proxy for the strength of this insurance. To investigate this hypothesis, we build more direct proxies for added

worker effect behavior, state by state, from the panel dimension of the Current Population Survey and from the Survey of Income and Program Participation. The classic measure is how often a wife enters the labor force in the month her husband loses his job, and we construct it together with several variants. We also measure attachment by the number of transitions between employment, unemployment and out of the labor force, with a larger number of transitions taken to indicate low attachment. Two results follow. First, every one of these proxies for the strength of the added worker effect is negatively related to the share of married women working across states. Second, employment responds less to a monetary policy shock in states where these proxies are higher.

The added worker effect can weaken a state’s response in two ways. The labor supply of married women adjusts, which offsets part of the fall in employment directly. And households that can draw on this insurance cut their spending less, which supports aggregate demand and with it labor demand for everyone. We argue that the second dominates. The direct margin is far too small to account for the differences we estimate. Moreover, the employment of everyone in the state responds, married women and everyone else alike, which is what the insurance through aggregate demand would predict. The direct margin is still visible. Fewer married women are drawn into the labor force after a tightening where more married women work. For everyone else the differential runs the other way.

Section 2 surveys the relationship between our work and the literature. Section 3 discusses our specification and data sources. Section 4 presents the aggregate evidence on the relationship between the share of married women working and the impact of monetary policy, together with its robustness. Section 5 turns to the added worker effect as a potential mechanism. Section 6 concludes.

2 Literature

Our paper contributes to several literatures. The first is the literature on the added worker effect. [Lundberg \(1985\)](#), [Stephens \(2002\)](#), [Parker and Skoufias \(2004\)](#), [Juhn and Potter \(2007\)](#) study this effect at the household level.¹ They show that when a husband becomes unemployed, his wife is more likely to enter the labor force. In addition, [Ellieroth \(2019\)](#) and [Casella \(2022\)](#) argue that wives respond to increases in unemployment risk not just by working more, but also by quitting less. [Birinci \(2026\)](#) shows that more generous unemployment benefits crowd out this insurance. The literature has also pointed to forces that may increase or reduce the importance of the added worker effect over time. [Mankart and Oikonomou \(2016\)](#) and [Mankart, Oikonomou, and Pascucci \(2022\)](#) argue that idiosyncratic income risk has increased over time and that this increase has made the insurance provided by the added worker effect more valuable. [Casella \(2022\)](#) argues that women have become more firmly attached to the labor market and thus less marginal over time, that this has muted the added worker effect, and thus made the economy more cyclically sensitive.² At a broad level, our empirical results illustrate the macroeconomic

¹The added worker effect traces back at least to [Woytinsky \(1942\)](#) who argued that labor force participation was boosted by a number of added workers who entered the labor force when their spouses lost their jobs.

²In [Casella \(2022\)](#) the relationship between women’s attachment and cyclical sensitivity is U-shaped. When few women work, more attachment makes the economy less cyclically sensitive. Once women are sufficiently attached, more attachment makes it more cyclically sensitive. The turning point is around

importance of the added worker effect. More specifically, our results lend support to the mechanism proposed by [Casella \(2022\)](#) by showing that as married women become more attached to the labor market, the importance of this effect diminishes and the economy becomes more sensitive to monetary policy shocks.

Several papers have studied the consequences of the added worker effect in general equilibrium models. [Mankart and Oikonomou \(2017\)](#) and [Guner, Kulikova, and Valladares-Esteban \(2025\)](#) show that the added worker effect explains the acyclicity of aggregate labor force participation. [Blundell, Pistaferri, and Saporta-Eksten \(2016\)](#) show that insurance within couples is important to smooth consumption. [Wang \(2019\)](#) studies a coordinated joint search model including the added worker effect. [Wu and Krueger \(2021\)](#) show the consequences for optimal tax policies. The two most important papers, from our perspective, are [Casella \(2022\)](#) and [Bardóczy \(2022\)](#). [Casella \(2022\)](#), discussed above, models the interaction between the added worker effect and labor force attachment of married women. [Bardóczy \(2022\)](#) shows that in a heterogeneous household model, the added worker effect only needs to be observed in a small fraction of households for it to have a large impact on the evolution of aggregates. This is consistent with our finding that the greater is the attachment of married women to the labor force (and hence the weaker is the added worker effect), the more volatile is employment for all workers, men and women.

We also contribute to the literature connecting the rapid change in women’s labor market attachment to the economy’s sensitivity to shocks more broadly. For example, [Fukui, Nakamura, and Steinsson \(2023\)](#), [Albanesi \(2019\)](#) and [Olsson \(2025\)](#) argue that the large trend increase in women’s labor force participation over the second half of the 20th century made recoveries from recessions look fast along this transition. In other words, recoveries of men have always been slow, but the aggregate recoveries started to look markedly slower once women’s convergence slowed and their participation stopped adding to employment growth.

More broadly, our paper relates to the literature studying the role of labor market flows (and specifically of labor supply) in responses to monetary policy. [Graves, Huckfeldt, and Swanson \(2023\)](#) estimate the response of labor market flows to monetary policy shocks and find evidence that, in contrast to the consensus view, a contractionary monetary policy shock leads to a significant increase in labor supply. [White \(2018\)](#) show that a full stock-flow accounting reveals that job loss is the largest driver of monetary policy’s effects on the labor market. These effects are heterogeneous. [Broer, Kramer, and Mitman \(2025\)](#) show that in Germany, job and income losses rise more following a monetary contraction for lower-earnings households. [Faia, Shabalina, and Wiczer \(2024\)](#) show that within the subpopulation of below-median income, monetary policy tightening raises separations more among older, female and non-white individuals. Overall, papers such as [Elsby, Hobijn, and Şahin \(2015\)](#) use flow-analysis to argue that the participation margin is important for aggregate employment dynamics. We show that cross-state variation in how much married women tend to change labor market status predicts a differential response of state-level employment to monetary shocks. Moreover, we show that cross-state variation in the share of married women working pre-shock predicts cross-state variation in the entry probability of married women following a monetary shock.

Finally, we contribute to the literature that considers features of economies that alter the U.S. level of 1990, where our sample starts.

the impact of monetary shocks. [Carlino and DeFina \(1998\)](#) document heterogeneity in the impact of monetary policy shocks across U.S. states, and attribute these differences to industrial mix. [Imam \(2015\)](#) and [Leahy and Thapar \(2022\)](#) argue that the impact of monetary policy shocks depends on the age structure of the economy. [Maggio, Kermani, Keys, Piskorski, Ramcharan, Seru, and Yao \(2017\)](#) and [Beraja et al. \(2019\)](#) argue that the structure of mortgages and recent house price movements may affect the transmission of monetary policy. [Cravino et al. \(2020\)](#) have argued that income may matter as richer individuals consume a greater share of sticky-price goods. We add the employment share of married women to this list.

Our results may seem counter-intuitive at first. A broad literature documents that employment of women is more stable and less cyclical, especially due to the differences in the sectoral composition (see [Doepke and Tertilt \(2016\)](#) for an overview). It might thus seem obvious that economies where women form a larger share of the workforce should be less volatile. In fact, [Mennuni \(2019\)](#) shows that across U.S. states and the time period 1982–2013 higher female labor force participation is associated with lower volatility of output. In contrast, we show that economies where women are already well attached to the labor market, and thus form a higher share of the overall workforce on average, are more sensitive to monetary policy shocks. There is no inherent contradiction between these findings. On the one hand, female-dominated sectors may be less likely to be hit by sector-specific shocks. On the other hand, states with a larger fraction of married women employed may respond more strongly to a common monetary shock.

3 Data and empirical strategy

We begin by considering the impact of monetary policy shocks across U.S. states, and how the impact of these shocks varies with the employment decisions of married women. We show that monetary policy shocks have a greater impact on employment and payroll in states in which a higher share of married women worked pre-shock. We treat the U.S. states as a panel of economies. There are several advantages to this. The primary advantage is that the U.S. is a monetary union so that all states are subject to the same monetary policy. This means that we can remove the first-order effect of monetary policy using time fixed-effects and focus on the interaction of monetary policy shocks with local demography. A common monetary policy also removes many sources of heterogeneity. We do not have to worry about heterogeneity across states in monetary policy rules. We do not have to worry about monetary policy responding to the demographic makeup of each state. We do not have to worry about differences that might arise from differences in the identification of monetary policy shocks across different monetary authorities. Relative to nation states, the U.S. states also have a homogeneous legal and institutional structure. The problems that remain, such as immigration, trade, and heterogeneity of industrial structure are present, more or less, in nations as well, and we will attempt to control for some of these influences in what follows.

Our baseline regression is a panel version of a Jorda projection ([Jorda, 2005](#)):

$$\Delta \ln y_{s,t,k} = \beta^k \cdot \epsilon_t \cdot S_{s,t-1}^{MW} + \gamma^k \cdot S_{s,t-1}^{MW} + \alpha_t + \alpha_s + u_{s,t+k}. \quad (1)$$

Here s indexes the U.S. state, t indexes time, and k indexes the horizon. $\Delta y_{s,t,k}$ is the

percent change in the dependent variable in state s between date $t - 1$ and date $t + k$. The dependent variable is typically some measure of employment or total payroll. ϵ_t is a monetary policy shock. We discuss the construction of this shock in the next section. $S_{s,t-1}^{MW}$ is a measure of how much married women work. Our main measure is the ratio of the employment rate of married women relative to the employment rate in the economy as a whole:

$$S_{s,t}^{MW} = \frac{E_{s,t}^{MW}}{P_{s,t}^{MW}} \bigg/ \frac{E_{s,t}}{P_{s,t}}$$

where $P_{s,t}$ is the population of state s at date t , $E_{s,t}$ is the number of employed persons in state s at date t , $P_{s,t}^{MW}$ is the total number of married women in state s at date t , and $E_{s,t}^{MW}$ is the number of married women who are employed in state s at date t . We limit these counts to individuals age 16 and older. To remove seasonality, $S_{s,t}^{MW}$ is a four quarter moving average.³

The state effects α_s capture persistent differences across states in labor force participation or industrial structure, and time fixed effects α_t account for factors that are common to all states such as interest rates, the exchange rate, and the state of the business cycle. The variable $u_{s,t+k}$ is the error term. Note that since we include time effects, we do not include the money shock ϵ_t separately in the regression.

We run this panel regression separately for each horizon k . The coefficients of interest are the β^k which capture how the impact of a monetary policy shock in period t on the dependent variable k periods in the future varies with employment decisions of married women. Since our monetary policy shocks represent shocks to interest rates, positive shocks reflect contractionary monetary policy. Positive values of β^k therefore imply that an increase in the share of married women working reduces the effect of a shock to monetary policy on the growth of the dependent variable. Negative values of β^k , on the other hand, imply that the contractionary effects of an increase in interest rates become stronger. Estimating equation (1) for each $k = 0, 1, 2, \dots, K$ illustrates how an increase in the proportion of married women working affects the response of y to a monetary policy shock at different horizons k . Coefficients are multiplied by 100 and thus the magnitudes represent percentage points changes in the growth of y . S^{MW} is multiplied by 100, thus the magnitudes represent an effect of increasing the relative share of married women working by one percentage point. The shock is measured in percentage points of the federal funds rate, so the coefficient is the response to a one percentage point surprise. A typical surprise is about a tenth of that.

We follow the high-frequency approach to the identification of monetary policy shocks employed by [Gurkaynak, Sack, and Swanson \(2005\)](#), [Gertler and Karadi \(2015\)](#) and [Nakamura and Steinsson \(2018\)](#).⁴ As highlighted by [Nakamura and Steinsson \(2018\)](#), there are some concerns regarding the interpretation of these high-frequency shocks. We therefore consider the robustness of our results to other shocks in [Appendix B. Ottonello and Winberry \(2020\)](#) discuss the construction of the shocks in detail. Here we provide a brief overview.

³Results are qualitatively similar if we instead use $E_{s,t}^{MW}/P_{s,t}^{MW}$ as our measure of labor force participation. $S_{s,t}^{MW}$ has the advantage of controlling for factors that affect employment of both men and women married and unmarried, such as the age structure of the population.

⁴Interest rate futures have been used to identify monetary policy shocks by [Rudebusch \(1998\)](#), [Kuttner \(2001\)](#), [Söderström \(2001\)](#), and [Thapar \(2008\)](#).

There are two steps in the construction. First, we obtain raw shocks from the movement in the federal funds rate implied by the current month federal funds futures contract in a relatively short window of time around the FOMC policy announcements. We consider the change in the futures rate from 15 minutes before the announcement to 45 minutes after the announcement. By considering a narrow window around the FOMC announcement, one can be reasonably certain that no other news caused the change in the futures rates. Our baseline uses the federal funds rate factor of [Swanson \(2021\)](#). He measures how a range of interest rates, from federal funds futures to the ten year Treasury yield, move in a thirty minute window around each FOMC announcement between 1991 and 2019, and extracts three factors from these responses. The first is rotated to correspond to the surprise change in the federal funds rate, and it is the one we use. We scale it so that one unit is one percentage point of the funds rate. The two series are close where they overlap, with a correlation of 0.95 across announcements.

Second, following [Ottonello and Winberry \(2020\)](#), we aggregate the high-frequency shocks to a quarterly frequency using a weighted moving average of the shocks in the current quarter and the previous quarter. These weights take into consideration the fact that employment and personal income are the result of outcomes and decisions that accumulate over the entire quarter and that the monetary policy shock can only affect the subset of these decisions that are made subsequent to the shock. The weights are chosen so that an announcement at the end of quarter $t - 1$ is essentially the same as an announcement at the beginning of quarter t .⁵

The identified shocks are small but not insignificant. The average quarterly shock over our 1991–2019 sample is close to zero, with a standard deviation of 8 basis points. Over the shorter 1990–2008 sample the average is -5 basis points with a standard deviation of 11 basis points.⁶ [Leahy and Thapar \(2022\)](#) show that these shocks have the anticipated effect on aggregate activity. Positive shocks lead to a temporary increase in interest rates and a reduction in GDP, income and employment (see Figure 1 of that paper). They therefore look like monetary policy shocks.

Appendix A contains a detailed description of the data used in this paper. Here we provide a brief overview. Our primary data source for state-level private sector employment and payroll is the Census Bureau’s Longitudinal Employer-Household Dynamics (LEHD) Program. The LEHD data provide the number of jobs and total payroll by state, industry and demographic groups. Note that this is an unbalanced panel, as states entered the panel at different times.

We construct our state-level measures by demographic group, among them S^{MW} itself, employment and participation rates, and transitions between labor market states, from the Current Population Survey (CPS) IPUMS data ([Flood, King, Rodgers, Ruggles, Warren, Backman, Chen, Cooper, Richards, Schouweiler, and Westberry \(2024\)](#)). For the analysis of employment we use the whole sample in each month. For the analysis of transitions we utilize the panel dimension within CPS.⁷ For the analysis of wages, we take advantage of

⁵Note that this second step introduces some serial correlation in the shocks which we will have to control for in the estimation.

⁶Approximately 40% of shocks are contractionary while 60% are expansionary, and there are 70% more shocks than actual changes in the federal funds rate. Very often the absence of a change in interest rates is itself a shock.

⁷Each respondent is interviewed for four consecutive months, then leaves the sample for an eight month break and is then added again for four months.

the “earner study” subsample.⁸ The earner study includes the respondent’s hourly wage (for those who are paid hourly), usual weekly earnings on the main job (for those not paid hourly) and usual hours worked a week at the main job.

In some of the robustness exercises, we use state-level descriptors built from their primary sources: industry and finance shares from the Quarterly Workforce Indicators, the private employment share from the Bureau of Labor Statistics, personal income per capita from the Bureau of Economic Analysis, the homeownership rate from the Census Bureau, age structure and shares working from the CPS, unemployment insurance generosity from [Boone et al. \(2021\)](#) and tax progressivity from [Fleck et al. \(2025\)](#). Before presenting the results for estimating equation (1), we briefly discuss some of the properties of S^{MW} . Table B.2 in the Appendix presents correlations of the share of married women working with other measures of labor force participation and with the state characteristics we use as controls. Importantly, controlling for the overall share of the population that is working greatly reduces the correlation with other measures of labor force participation. For example, the correlation between S^{MW} and overall labor force participation is essentially zero.

Our baseline sample is a quarterly panel of U.S. states from 1991 Q4 to 2019 Q2. We begin in 1991 Q4 as that is the start of the shock series. We end our sample period in 2019 Q2 as that is its end, and before the pandemic. In Appendix D we repeat every result on the shorter sample that ends in 2008 Q4, once the federal funds rate hit the zero lower bound.

Table 1: Cross-state distribution of S^{MW} , 1991Q4–2019Q2

	State $\times$ quarter ($N = 5,661$)	State means ($N = 51$)
Mean	0.954	0.954
Std. dev.	0.043	0.035
Interquartile range	0.055	0.042
Minimum	0.811	0.861
Maximum	1.115	1.020

Notes: $S_{s,t}^{MW}$: employment rate of married women aged 16+ relative to the employment rate of the population aged 16+, CPS, four-quarter moving average. State means: each state’s mean over the sample.

4 Main result

In this section we show that monetary policy has a larger effect on aggregate employment in states where more married women work, and that this result is robust to a wide range of alternative measures, samples and specifications.

Figure 1 presents the results for estimating equation (1) taking as the dependent variable state employment or state payroll. Each figure plots the estimated coefficient

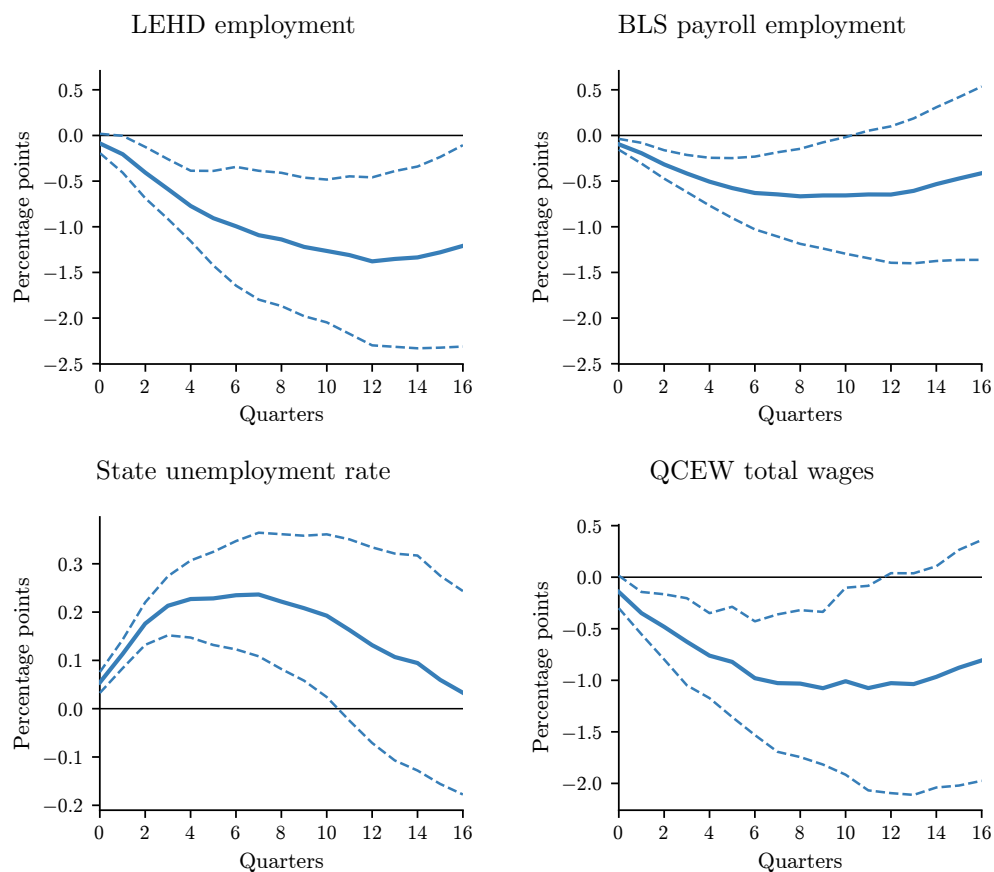
⁸The “earner study” subsample consists of individuals who are about to rotate out of the panel, i.e. those who have been in the sample for 4 or 8 months respectively, who are currently employed and over 15 years old (self-employed individuals are excluded). No earnings or income information is available in the core survey. Extensive income information is included in the March CPS ASEC sample, however this would only allow analysis on an annual level.

β^k against the horizon k . The four panels differ in the outcome: employment from the LEHD and from the BLS payroll survey, the state unemployment rate from the LAUS, and total wages from the QCEW.

The solid lines in the figure illustrate the effect that a one percentage point increase in S^{MW} has on the response of the outcome variable to an increase in the federal funds rate. This response is over and above the common effect that monetary policy has on all states. This common component has been removed from the regression by time fixed effects. [Leahy and Thapar \(2022\)](#) show that this common effect has the standard properties of a monetary policy shock: employment and payroll both decline temporarily in response to an increase in interest rates. Negative values in the figure therefore indicate that a higher employment rate for married women causes a variable to fall further than average, whereas positive values typically indicate that the variable falls by less. The dashed lines in the figure represent the 95 percent confidence intervals based on [Driscoll and Kraay \(1998\)](#) standard errors. These are meant to be robust to general forms of spatial and temporal correlation in the error terms.

The figure tells us that when the employment rate of married women is higher in a state, monetary policy shocks have a greater effect on labor market outcomes in that state. Employment falls further, the unemployment rate rises more, and total wages fall further. Table [B.1](#) in Appendix [B](#) presents the coefficient estimates and standard errors behind the two employment panels.

Figure 1: Effect of an increase in the share of married women working on the response of employment to a monetary policy shock



Notes: Logs, except the unemployment rate (LAUS, pp). BLS payroll: total nonfarm. QCEW: total quarterly wages, all ownerships, seasonally adjusted. Plotted against the horizon k in quarters: the coefficient on shock $\times S_{s,t-1}^{MW}$ in the local projection of the outcome from $t-1$ to $t+k$, with state and time fixed effects, $\times 100$, so the effect of a one percentage point higher S^{MW} on the response to a one percentage point surprise. 95 percent confidence intervals, Driscoll-Kraay. Shock: the policy rate surprise extended through the zero lower bound, 1991Q4–2019Q2.

To gauge the size of this effect, we compare it to the aggregate employment response to the same shock. Estimating that response directly, national private employment falls by about 5 percent six quarters after a one percentage point surprise, close to the response Leahy and Thapar (2022) report, whose employment series falls by about 5 percent at its trough.⁹ Across states, one standard deviation of S^{MW} is about 3.5 percentage points. A state one standard deviation above the mean therefore loses a further 3 percent of employment, about 60 percent of the aggregate response. The same calculation on payroll employment gives about 40 percent. Read across time rather than across states, the same slope would be large. S^{MW} rose by about 6 percentage points nationally between 1990 and 2008, which would deepen the response to a given shock by roughly one aggregate response. Read this way, monetary policy became more powerful over these decades as married women became less marginal.¹⁰

We have shown that the impact of monetary policy is larger in states in which the share of married women who work is large relative to the overall share of the population that is employed. A natural question is whether this result stems from behavior of married women or whether there is something about the labor market attachment of women in general that makes monetary policy more potent for aggregate employment. To answer this question we define S^{NMW} to be the (relative) share of unmarried women employed in the state,

$$S_{s,t}^{NMW} = \frac{E_{s,t}^{NMW}}{P_{s,t}^{NMW}} \bigg/ \frac{E_{s,t}}{P_{s,t}}$$

and we add S^{NMW} to our baseline regression:

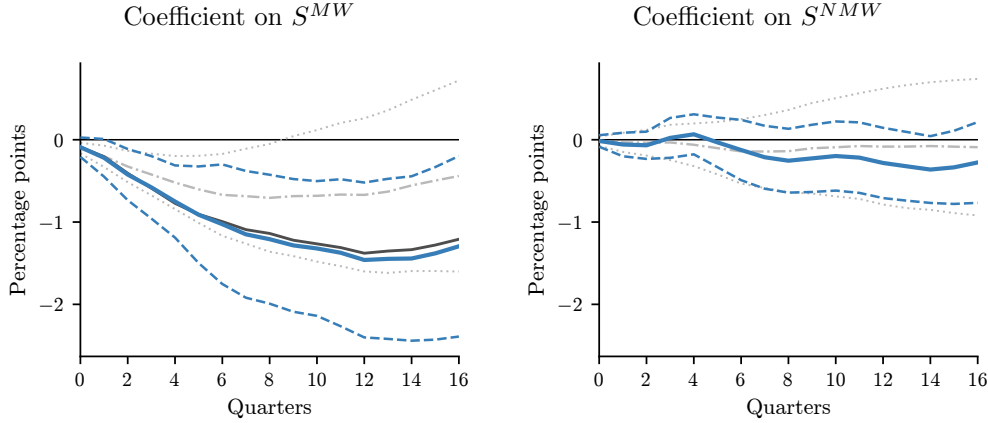
$$\begin{aligned} y_{s,t,k} = & \beta^k \cdot \epsilon_t \cdot S_{s,t-1}^{MW} + \gamma^k \cdot S_{s,t-1}^{MW} + \\ & + \theta^k \cdot \epsilon_t \cdot S_{s,t-1}^{NMW} + \delta^k \cdot S_{s,t-1}^{NMW} + \alpha_t + \alpha_s + u_{s,t,k} \end{aligned} \quad (2)$$

The coefficients of interest are β^k and θ^k , which capture how S^{MW} and S^{NMW} affect the impact of monetary policy shocks. One can think of this as a horse race between the two variables. Figure 2 presents the results. The left-hand panel shows the impact of S^{MW} , which looks very similar to our baseline results, so adding S^{NMW} does little to alter it. The right-hand panel shows the impact of S^{NMW} , which has little impact on monetary policy once we control for S^{MW} . We interpret these results as justification for our focus on married women. Here and in the figures that follow, the blue line is estimated on LEHD employment and the grey line underneath on payroll employment.

⁹Appendix B reports our aggregate local projection. At six quarters our estimate on their shock and sample is -2.0 percent, against -1.9 percent in their Figure 1.

¹⁰This comparison applies a cross-state relationship to the time series. Over a change of that size the relationship is likely not linear. As discussed in Section 2, Casella (2022) makes it U-shaped.

Figure 2: Share of married women working and share of unmarried women working



Notes: Both interactions in one local projection of log state employment (LEHD solid, BLS payroll grey dash-dotted); $\times 100$. S^{NMW} : as S^{MW} , unmarried women 16+. 95 percent confidence intervals, Driscoll-Kraay.

It is possible that the effect of the share of married women on monetary policy is driven by some third variable correlated with the share. Definitively demonstrating a causal link between female employment and monetary policy requires an instrument. Given that we lack one, our next set of experiments attempts to control for possible confounders. These regressions take the form:

$$\Delta \ln y_{s,t,k} = \beta^k \cdot \epsilon_t \cdot S_{s,t-1}^{MW} + \gamma^k \cdot S_{s,t-1}^{MW} + \theta^k \cdot \epsilon_t \cdot X_{s,t-1} + \delta^k \cdot X_{s,t-1} + \alpha_t + \alpha_s + u_{s,t+k}. \quad (3)$$

where X is the confounding variable. Figures B.2 and B.3 report these regressions. In all cases, the impact of the employment share of married women on monetary policy is little changed. Table B.2 lists the confounders and their correlation with S^{MW} , and most are only weakly correlated with it. The potential confounders are of five kinds. Our focus on married women might confound the impact of marriage with the impact of gender or labor force participation, so we control for the share of women, of men and of all adults in the workforce. [Carlino and DeFina \(1998\)](#) argue that differences in the impact of monetary policy across states are due in part to differences in the mix of industries, and the response of industries to monetary policy may be correlated with the gender composition of their workforce, so we control for the share of manufacturing, construction, services and financial services. [Leahy and Thapar \(2022\)](#) argue that the age structure of the state is correlated with the impact of monetary policy, and age may also be correlated with marriage and the employment decisions of married women, so we control for the share of the population 25 to 40, 40 to 65, and 65 and up. [Cravino et al. \(2020\)](#) argue that higher income individuals consume goods with stickier prices, and as marriage may also be correlated with income we control for state personal income per capita. [Beraja et al. \(2019\)](#) argue that there is a relationship between mortgage refinance and the impact of monetary policy, and as marriage and ownership are correlated we control for the home-ownership rate. Public insurance also differs across states and may affect how much married women work. We therefore control for the generosity of unemployment insurance, following [Boone et al. \(2021\)](#), and for the progressivity of the tax and transfer system, measured by [Fleck et al. \(2025\)](#).

We perform a number of additional robustness exercises. We show that the baseline estimates are robust to measuring employment in other sources, to running the regressions at monthly frequency (Figure B.1 in the appendix), to the sample that ends before the zero lower bound (Appendix D), to using only within-region variation, and to dropping the smallest states (Figure B.9 in the appendix). We also show that they are robust to alternative methods of identifying monetary shocks. In particular, we show that estimating (1) with the Romer and Romer (2004) shocks yields very similar results (Figure B.4 in the appendix). The forward guidance factor of Swanson (2021) does too, and the asset purchase factor points the same way, though the effect is not statistically significant (Figure B.5 in the appendix). The shocks of Miranda-Agrippino and Ricco (2021) give coefficients near zero over our sample. However, they recover the baseline pattern when the NBER recession quarters, 2007Q4 to 2009Q2, are removed from the shock (Figure B.7 in the appendix).¹¹

The differences across states in how much married women work may have many sources. While we have plausibly exogenous monetary shocks, our measure of the employment of married women is potentially endogenous. We therefore look for state characteristics that strongly predict S^{MW} and are not themselves tied to the business cycle, to better understand what variation drives our result. Table B.3 reports them. Two families of characteristics are of particular interest. One is how educated wives are relative to their husbands, which we measure by the education gap within couples and by how often the wife is the more educated spouse. The other is the level of S^{MW} before our sample begins, measured in 1990 and as its average over the 1980s. We also consider the presence of young children, household size, the Hispanic and foreign-born shares of the population, how urban the state is, and commuting times. Each predicts S^{MW} in the expected direction. We then use each of them in place of S^{MW} as the moderator in equation (1). A clear pattern emerges. Every one delivers the same result as our baseline. Employment falls further after a tightening in the states where the predictor says married women work more. This suggests that our result is not driven by incidental business cycle variation in S^{MW} . Moreover, when S^{MW} and the predictor enter together in a horse race, as in equation (2), the effect of S^{MW} is virtually unchanged while the effect of the predictor weakens significantly and typically drops out altogether (Figure B.12 in the appendix). This suggests that each of these characteristics captures a part of the variation in S^{MW} , but that in the end the effect runs through S^{MW} itself.

While we cannot establish causality definitively, the evidence leaves room for a causal role for the employment share of married women. We now present evidence that the added worker effect might be a mechanism behind why the share of married women working impacts the effect of monetary policy.

¹¹These series differ from ours mainly during the 2008 recession, where they imply a strong tightening. S^{MW} is strongly correlated with how severe that recession was in a state, including the fall in house prices (Figure B.8 in the appendix). We do not know why. Since the recession accounts for much of the variation in employment, those quarters are enough to flip the estimate. Outside them our result is robust to using these shocks. The shocks of Bauer and Swanson (2023) behave in the same way.

5 The added worker effect as a potential mechanism

Broadly speaking, the added worker effect stands for household insurance through the labor supply of the secondary earner. Households that have access to this margin are less affected by shocks. Aggregate demand in an economy where households have access to this insurance is therefore less affected by a shock (Bardóczy, 2022). Casella (2022) shows that this form of insurance requires married women who are marginal, close to the decision of whether to work and how much. Where S^{MW} is high, married women are attached to the labor market in much the same way men are, and their labor supply no longer flexibly adapts to the needs of the household. Theory therefore predicts that S^{MW} is a proxy for the strength of this insurance.¹² In this section we present evidence that supports this reading. Specifically, we build a set of proxies for added worker effect behavior from household panels, state by state. First, we show that they correlate negatively with S^{MW} across states. Second, we show that employment responds less to a monetary policy shock where they are higher.

The classic evidence for the presence of the added worker effect is that wives enter the labor force when their husbands lose their job (Lundberg, 1985; Stephens, 2002; Parker and Skoufias, 2004; Juhn and Potter, 2007; Mankart and Oikonomou, 2016). We construct this measure state by state using the panel dimension of the CPS, which follows each household for four consecutive months. We use married couples in which the wife is 25 to 55 and follow each couple across two consecutive months. For couple i in state s and month t , let $D_{it} = 1$ if the husband is employed in $t - 1$ and unemployed in t , and let $N_{it} = 1$ if the wife is out of the labor force in $t - 1$ and in the labor force in t . We define a proxy for the added worker effect as

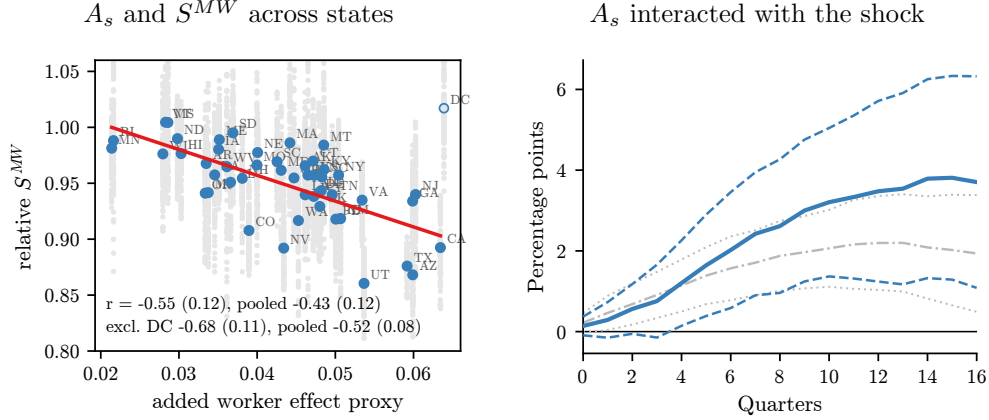
$$A_s = P(N_{it} = 1 \mid D_{it} = 1),$$

the share of wives who enter the labor force in the month their husband loses his job, pooled over 1976 to 2023. Across states it averages 4.35 percentage points, with a standard deviation of 1.03. Table C.1 reports this measure and other proxies we consider.

If a high S^{MW} indicates a weaker added worker effect, it should be negatively related to the alternative proxy we construct. The left panel of Figure 3 plots the two against each other. The correlation is negative, -0.55 across all states, and -0.68 without the District of Columbia, which is a clear outlier. The relationship is robust to measuring the proxy on 1990 to 2019, the years our shocks cover. It also holds if we do not condition on the husband's job loss and simply count how often the two occur together, $P(N_{it} = 1, D_{it} = 1)$. Moreover, constructing this measure in the Survey of Income and Program Participation (on the 38 states it identifies) confirms the pattern. The SIPP follows couples for a longer period of time than the CPS, so we also build a broader measure there. We count the wife as entering if she moves from outside the labor force into it in any month of a window running from three months before the husband's job loss to three months after it. Figure C.1 in the appendix shows all of them.

¹²In theory the relationship is U-shaped. Where almost no married women work they are not marginal either. The turn lies below the levels the United States has seen since 1990, so our sample sits on one arm of the U.

Figure 3: Added worker effect proxy across states: CPS, 1976–2023



Notes: $A_s = P(N_{it} = 1 \mid D_{it} = 1)$, married women 25–55. $D_{it} = 1$: husband employed at $t - 1$, unemployed at t . $N_{it} = 1$: wife out of the labor force at $t - 1$, in it at t . Left: Dots: state means. Grey: quarterly S^{MW} . Red: OLS fit. r with S^{MW} , across states on state means and pooled over state-quarters, standard errors in parentheses (pooled: clustered by state); second line excl. DC. Right: the effect of a one percentage point higher A_s on the response of state employment to a one percentage point monetary policy surprise, k quarters after the shock, in percent. LEHD solid, BLS payroll grey dash-dotted. DC excluded. 95 percent confidence intervals, Driscoll–Kraay.

One might object that some of these wives would have entered the labor force anyway. We thus also compute an alternative proxy,

$$P(N_{it} = 1 \mid D_{it} = 1) - P(N_{it} = 1 \mid D_{it} = 0),$$

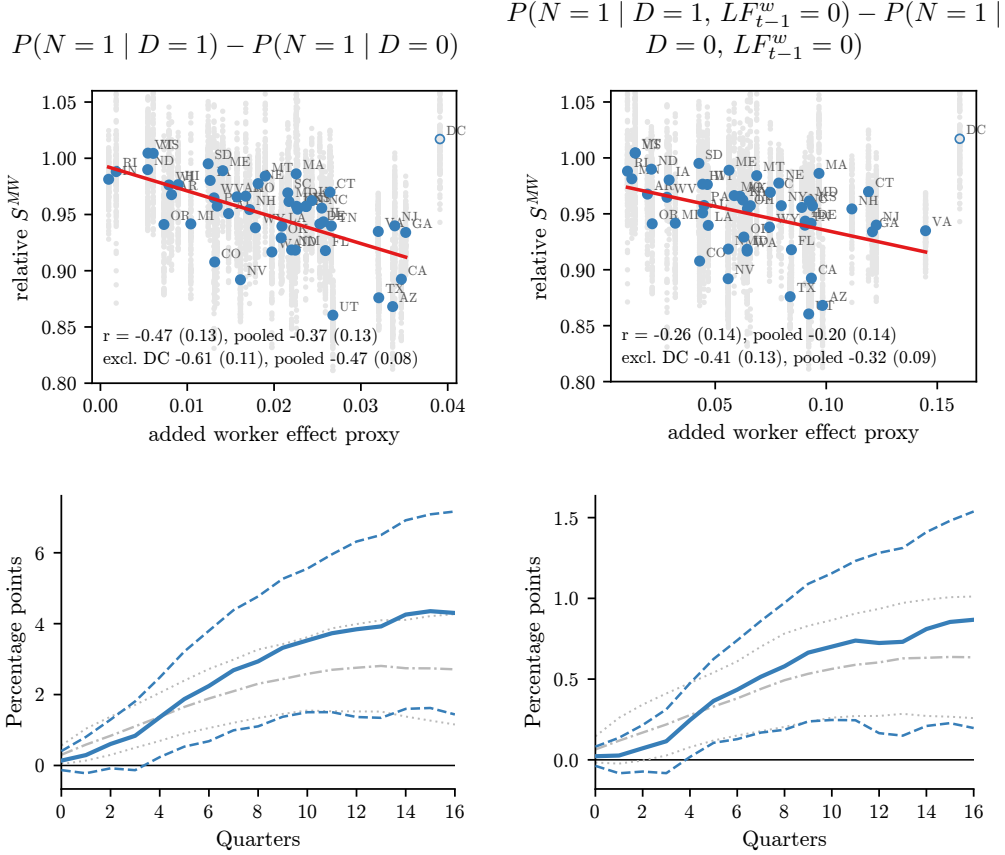
the entry rate of wives whose husband has just lost his job minus the entry rate of wives whose husband has not.¹³ Across states it averages 1.94 percentage points, with a standard deviation of 0.91. This alternative proxy also correlates negatively with S^{MW} , at -0.47 across all states and -0.61 without the District of Columbia. Mankart and Oikonomou (2016) go further and condition on the wife being out of the labor force to begin with,

$$P(N_{it} = 1 \mid D_{it} = 1, LF_{it-1}^w = 0) - P(N_{it} = 1 \mid D_{it} = 0, LF_{it-1}^w = 0).$$

We compute this measure following their specification. Across states it averages 6.75 percentage points, with a standard deviation of 3.45. It is important to emphasize that this conditional probability differential does not measure the strength of the added worker effect in the aggregate. If almost all women were always in the labor force, this margin of the added worker effect would be almost completely absent in that economy, while this measure is very high. It is an intensity within a restricted pool. Yet this proxy also correlates negatively with S^{MW} across states, at -0.26 and at -0.41 without the District of Columbia, though more weakly than the other measures.

¹³This measure is essentially equivalent to the measure reported in footnote 10 of Mankart and Oikonomou (2016).

Figure 4: Alternative definitions of the added worker effect proxy



Notes: CPS 1976–2023, married women 25–55. $D_{it} = 1$: husband employed at $t - 1$, unemployed at t . $N_{it} = 1$: wife out of the labor force at $t - 1$, in it at t . Left: the entry differential, how much more often wives enter when the husband loses his job.^a Right: the same difference among wives out of the labor force at $t - 1$, the object of [Mankart and Oikonomou \(2016\)](#), constructed with their specification, a weighted linear probability model with controls for the wife’s age, education and children and year fixed effects. Top row: Dots: state means. Grey: quarterly S^{MW} . Red: OLS fit. r with S^{MW} , across states on state means and pooled over state-quarters, standard errors in parentheses (pooled: clustered by state); second line excl. DC. Bottom row: the effect of a one percentage point higher value of the measure on the response of state employment to a one percentage point monetary policy surprise, k quarters after the shock, in percent. LEHD solid, BLS payroll grey dash-dotted. DC excluded. 95 percent confidence intervals, Driscoll–Kraay.

^aClose to the object in footnote 10 of [Mankart and Oikonomou \(2016\)](#), which multiplies their intensity by the share of wives out of the labor force.

Our hypothesis is that when married women work less they are only marginally attached to the labor market, so that they can time their labor supply for the purpose of the household's income smoothing. If that is right, married women should change their labor market status more frequently in states where fewer of them work. Let t^m index months and t quarters, and let I_{i,t^m} be the status of married woman i , equal to one if she is employed, two if she is unemployed and three if she is out of the labor force. We count all changes of status within quarter t ,

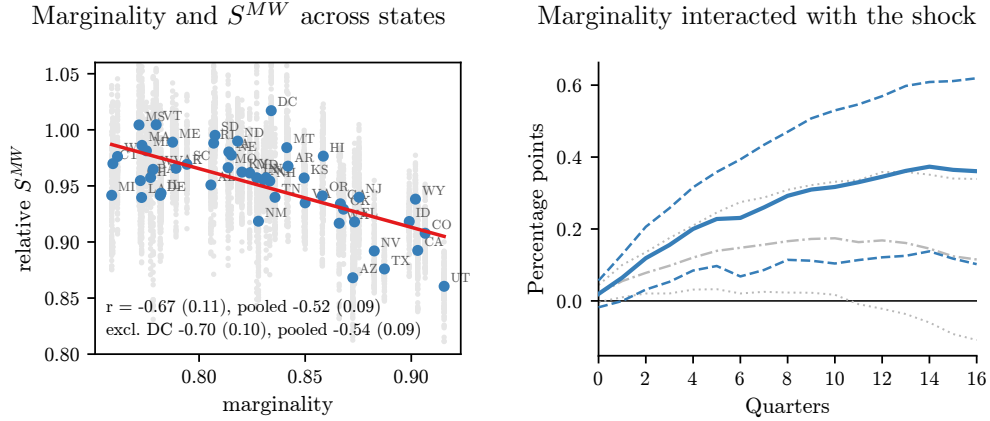
$$T_{s,t}^{MW} = \sum_{t^m \in t, i \in s} \sum_{j=1,2,3} \mathbf{1}\{I_{i,t^m} = j\} \mathbf{1}\{I_{i,t^m-1} \neq j\},$$

scale them by the population of married women, and divide by the same rate for all adults,

$$M_{s,t}^{MW} = \frac{T_{s,t}^{MW}}{P_{s,t}^{MW}} \bigg/ \frac{T_{s,t}}{P_{s,t}}$$

(mean 82.7 percent across states, standard deviation 4.4).¹⁴ This proxy too correlates negatively with S^{MW} , at -0.67 . States where married women are less likely to work are states where they change their labor market status more often.

Figure 5: Marginality of married women



Notes: M^{MW} : relative marginality of married women's transitions, CPS 16+. Left: Dots: state means. Grey: quarterly S^{MW} . Red: OLS fit. r with S^{MW} , across states on state means and pooled over state-quarters, standard errors in parentheses (pooled: clustered by state); second line excl. DC. Right: the effect of a one percentage point higher M^{MW} on the response of state employment to a one percentage point monetary policy surprise, k quarters after the shock, in percent. LEHD solid, BLS payroll grey dash-dotted. 95 percent confidence intervals, Driscoll-Kraay.

Our hypothesis says that states where the added worker effect is stronger should respond less to an aggregate shock, so that θ^k should be positive in

$$\Delta \ln EMP_{s,t+k} = \theta^k \epsilon_t X_{s,t-1} + \delta^k X_{s,t-1} + \alpha_{t,k} + \alpha_{s,k} + u_{s,t+k},$$

where X stands for each of our proxies in place of S^{MW} (for the measures without time variation the level term is absorbed by the state fixed effect). Figures 3, 4 and 5 show

¹⁴As with S^{MW} , we use four-quarter moving averages of T and P .

exactly that. Employment falls by less after a tightening in states where the proxy is higher. The pattern holds for every proxy we construct, in the CPS and in the SIPP, for the entry measures and for marginality alike. It builds over the same horizons as our baseline, reaching its largest value after three to four years, and it shows up in BLS payroll employment as well as in the LEHD. Appendix C shows the same for the alternative samples.

Since we believe that a low S^{MW} and a high value of any of the proxies both indicate a strong added worker effect, it is natural to ask what happens when the two enter the same projection, which is equation (3) with a proxy in the place of the confounder. Figure C.3 in the appendix reports β^k and θ^k . The coefficient on S^{MW} weakens moderately, by about seven percent. The coefficient on the proxy falls much more, though it stays positive and significant at longer horizons for several of the measures. Two readings are consistent with this. Under the first, all of these measures are imperfectly correlated proxies for the same latent mechanism. Because S^{MW} is much less noisy (since the other proxies condition on a rare event), it might be a better proxy, despite the others being arguably more direct. It is also important to realize that the added worker effect, defined as household insurance through the labor supply of the secondary earner, is broader than what we can measure.¹⁵ Of course, it is also possible that S^{MW} captures something more than the added worker effect.

The added worker effect can weaken a state’s response to a shock in two ways. The labor supply of married women adjusts, which offsets part of the fall in employment directly. And households that can draw on this insurance cut their consumption less, which supports aggregate demand and with it labor demand for everyone. The first route is far too small to account for what we estimate. A back-of-the-envelope calculation puts the jobs taken by these women at under one percent of the difference in the employment response.¹⁶ Moreover, in Figure 6 we show that a high S^{MW} changes how the whole economy responds to a monetary shock, not only how married women respond. Specifically, we compute three rates from the CPS for people aged 25 to 55, seasonally adjusted. The employment rate is employment over population, the participation rate is the labor force over population, and the unemployment rate is the unemployed over the labor force. We compute each of them for married women, for everyone else, and for the whole population, and put each in the place of log employment in the projection,

$$y_{s,t+k} - y_{s,t-1} = \beta^k \epsilon_t S_{s,t-1}^{MW} + \gamma^k S_{s,t-1}^{MW} + \alpha_{t,k} + \alpha_{s,k} + u_{s,t+k},$$

where y is the rate in question. The coefficient β^k is now in percentage points of that rate, per percentage point of S^{MW} . Employment falls further where S^{MW} is high, and it does so for married women and for everyone else alike. Participation is the one place where

¹⁵For example, in Casella (2022) the response to the risk of unemployment, and not only to its realization, is crucial. In reality, household shocks take many more forms than just the rare event of the husband becoming unemployed. Moreover, the response can come through entry, exit or the intensive margin. Our proxies capture a small slice.

¹⁶Let E be total employment. Married men are about a third of it, and six quarters after a tightening their employment falls by about 5.9 percent per unit of the shock, so the job losses amount to $0.059 \times 0.33 E \approx 0.02 E$. A fraction p of the affected wives enters the labor force, and across states p has a standard deviation of 1.3 percentage points. Two states one standard deviation apart therefore differ by $0.013 \times 0.02 E = 0.00025 E$ in the employment of these women, which is 0.03 percentage points against the 3.3 percentage points by which their employment responses differ at that horizon.

the two groups part. For married women it falls weakly, which is what the direct route would produce. For everyone else the coefficient is positive. Unemployment rises further in both groups and in the population as a whole. Overall, the evidence is consistent with the added worker effect working by shielding aggregate demand.

If the direct margin is at work, states with a lower S^{MW} should draw more married women into the labor force after a shock. We measure these flows from the one-month links of the CPS. For group g in state s we count the people aged 25 to 55 who are outside the labor force in one month and in it in the next, cumulate these moves from t to $t + k$, and divide by the group's population over the same window,

$$CE_{s,t,k}^g = \frac{1}{\bar{P}_{s,t,k}^g} \sum_{j=0}^k \sum_{i \in g,s} \mathbf{1}\{\text{out of the labor force at } t+j-1, \text{ in it at } t+j\}.$$

We then estimate

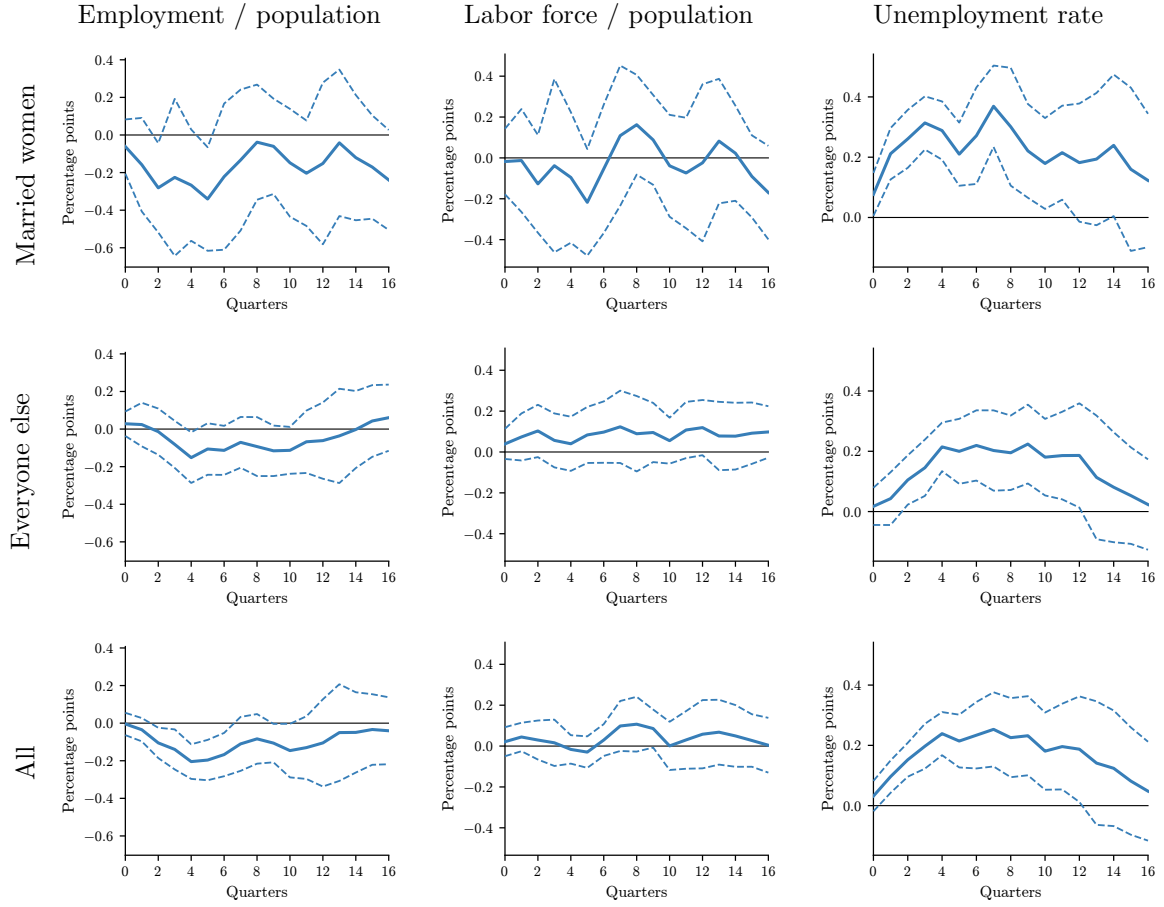
$$CE_{s,t,k}^g = \theta^k \epsilon_t S_{s,t-1}^{MW} + \delta^k S_{s,t-1}^{MW} + \alpha_{t,k} + \alpha_{s,k} + u_{s,t+k},$$

where the outcome is the flow cumulated over the window.¹⁷ A tightening draws fewer married women into the labor force where S^{MW} is high than where it is low. This is what the mechanism implies, since it is in the states with a low S^{MW} that married women are close to the margin and adjust their labor supply when the household's income is hit. The differential is immediate and precisely estimated over the first year, after which the bands widen. For everyone else the differential is if anything positive, so what we see is specific to married women and not a general shift in labor supply. The same pattern holds, and is more persistent, on the sample that ends before the zero lower bound (Appendix D).

Taken together, the evidence of this section supports reading S^{MW} as a proxy for the strength of household insurance through the labor supply of married women. Every measure of added worker effect behavior we build is negatively related to S^{MW} across states, and every one of them moderates the response of employment in the same direction as S^{MW} itself. The response reaches the employment of everyone in the state, not only that of married women, which is what the insurance operating through aggregate demand implies. The labor supply margin itself is visible in who enters the labor force after a tightening, though it is far too small on its own to account for the differences we estimate.

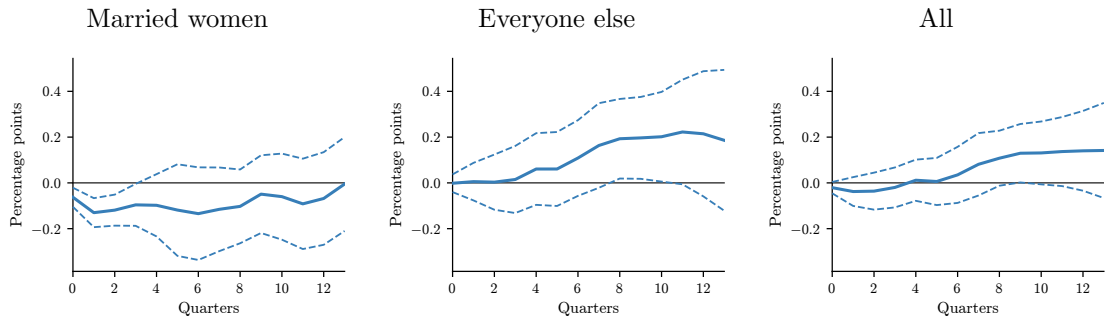
¹⁷Appendix C reports the exits and the net of the two. Entries minus exits would in principle return the change in the participation rate of Figure 6. The two do not line up exactly, since the flows are built only from respondents matched across adjacent months, while the rates use the whole sample.

Figure 6: Employment, labor force participation and unemployment, ages 25–55



Notes: CPS 25–55, seasonally adjusted, population-weighted (2010). Plotted against the horizon k in quarters: the coefficient on shock $\times S_{s,t-1}^{MW}$ in the local projection of the ratio from $t-1$ to $t+k$, with state and time fixed effects, $\times 100$, so the effect of a one percentage point higher S^{MW} on the response to a one percentage point surprise. 95 percent confidence intervals, Driscoll–Kraay.

Figure 7: Cumulative labor force entry



Notes: CPS 25–55, one-month linked transitions, population-weighted (2010). Outcome: entries over t to $t+k$, share of the group's population. Shock $\times S_{s,t-1}^{MW}$; state and time fixed effects; $\times 100$. 95 percent confidence intervals, Driscoll–Kraay.

6 Conclusion

In this paper we provide empirical evidence that the added worker effect matters on the aggregate. As argued by [Casella \(2022\)](#), the added worker effect is a more potent insurance mechanism when married women are closer to the margin of working or not, or how much. In economies where married women are already firmly attached to the labor market, they are not close enough to the margin of labor supply to be induced into counter-cyclical employment choices. As a result, this margin of insurance against shocks is shut down. Aggregate employment is more affected by shocks both because the direct counter-cyclical labor supply response of married women is missing and the lack of insurance motivates a stronger aggregate demand response.

We exploit cross-sectional variation in the response of U.S. states to a monetary policy shock to provide evidence for this mechanism. We study how the impact of monetary policy varies with the share of married women who have worked prior to the shock. We find that the economy’s response is more muted the lower the share of married women employed before the shock. We propose that this muted response reflects, at least in part, a stronger added worker effect. Three further findings support that reading. First, every proxy for added worker effect behavior that we build from household panels is negatively correlated with the share of married women working across states. Second, employment responds less to a monetary policy shock where these proxies are higher. Third, a tightening draws fewer married women into the labor force where more of them work, while the employment of everyone in the state responds. This is consistent with both the direct labor supply effect and the indirect labor demand effect of the insurance.

Families matter for the transmission of monetary policy. How much a family can insure itself shapes how much of an aggregate shock reaches its demand, and the labor supply of married women is one of the margins it insures with. Where married women are close to that margin, employment responds less to a shock. Where they work as men do, the margin is not available. States where more married women work are also somewhat richer, so a tightening falls a little harder where income per capita is higher. The composition of households is therefore part of what determines the power of monetary policy.

References

- Albanesi, S. (2019). Changing business cycles: The role of women’s employment. *NBER Working Paper Series*, 1–64.
- Bardóczy, B. (2022). Spousal insurance and the amplification of business cycles. *Working Paper*.
- Bauer, M. D. and E. T. Swanson (2023, 3). An alternative explanation for the ”fed information effect”. *American Economic Review* 113, 664–700.
- Beraja, M., A. Fuster, E. Hurst, and J. Vavra (2019, 2). Regional heterogeneity and the refinancing channel of monetary policy*. *The Quarterly Journal of Economics* 134, 109–183.
- Birinci, S. (2026, March). Spousal Labor Supply Response to Job Displacement and Implications for Optimal Transfers. Working Paper 2019-020, Federal Reserve Bank of St. Louis.
- Blundell, R., L. Pistaferri, and I. Saporta-Eksten (2016, 2). Consumption inequality and family labor supply. *American Economic Review* 106, 387–435.
- Boone, C., A. Dube, L. Goodman, and E. Kaplan (2021, 5). Unemployment insurance generosity and aggregate employment. *American Economic Journal: Economic Policy* 13, 58–99.
- Broer, T. ., J. V. Kramer, and K. Mitman (2025). The curious incidence of monetary policy across the income distribution. *Review of Economics and Statistics (Forthcoming)*.
- Carlino, G. and R. DeFina (1998, 11). The differential regional effects of monetary policy. *The Review of Economics and Statistics* 80, 572–587.
- Casella, S. (2022). Women’s labor force participation and the business cycle. *Working Paper*.
- Cravino, J., T. Lan, and A. A. Levchenko (2020, 4). Price stickiness along the income distribution and the effects of monetary policy. *Journal of Monetary Economics* 110, 19–32.
- Doepke, M. and M. Tertilt (2016). *Families in Macroeconomics* (1 ed.), Volume 2. Elsevier B.V.
- Driscoll, J. C. and A. C. Kraay (1998, 11). Consistent covariance matrix estimation with spatially dependent panel data. *The Review of Economics and Statistics* 80, 549–560.
- Ellieroth, K. (2019). Spousal insurance, precautionary labor supply, and the business cycle. *Working Paper*.
- Elsby, M. W., B. Hobijn, and A. Şahin (2015, 5). On the importance of the participation margin for labor market fluctuations. *Journal of Monetary Economics* 72, 64–82.

- Faia, E., E. Shabalina, and D. Wiczer (2024, 5). Heterogeneous effects of monetary policy on job flows across income, race, gender, and age. *AEA Papers and Proceedings* 114, 550–54.
- Favara, G., S. Gilchrist, K. F. Lewis, and E. Zakrajšek (2016). Updating the Recession Risk and the Excess Bond Premium. FEDS Notes. Washington: Board of Governors of the Federal Reserve System, October 6, 2016, <https://doi.org/10.17016/2380-7172.1836>.
- Fleck, J., J. Heathcote, K. Storesletten, and G. L. Violante (2025, October). Fiscal Progressivity of the U.S. Federal and State Governments. Technical report, Working paper.
- Flood, S., M. King, R. Rodgers, S. Ruggles, R. J. Warren, D. Backman, A. Chen, G. Cooper, S. Richards, M. Schouweiler, and M. Westberry (2024). Ipums cps: Version 12.0 [dataset]. minneapolis, mn: Ipums.
- Fukui, M., E. Nakamura, and J. Steinsson (2023). Women, wealth effects, and slow recoveries. *American Economic Journal: Macroeconomics* 15, 269–313.
- Gertler, M. and P. Karadi (2015). Monetary policy surprises, credit costs, and economic activity. *American Economic Journal: Macroeconomics* 7, 44–76.
- Gorodnichenko, Y. and M. Weber (2016, 1). Are sticky prices costly? evidence from the stock market. *American Economic Review* 106, 165–99.
- Graves, S., C. K. Huckfeldt, and E. T. Swanson (2023, 10). The labor demand and labor supply channels of monetary policy. *NBER Working Papers*.
- Guner, N., Y. Kulikova, and A. Valladares-Esteban (2025). Does the added worker effect matter? *Review of Economic Dynamics*.
- Gurkaynak, R. S., B. P. Sack, and E. T. Swanson (2005). Do actions speak louder than words? the response of asset prices to monetary policy actions and statements. *SSRN Electronic Journal*.
- Imam, P. A. (2015, 3). Shock from graying: Is the demographic shift weakening monetary policy effectiveness. *International Journal of Finance and Economics* 20, 138–154.
- Jorda, O. (2005, 3). Estimation and inference of impulse responses by local projections. *American Economic Review* 95, 161–182.
- Juhn, C. and S. Potter (2007). Is there still an added worker effect? *NBER Working Paper*.
- Kuttner, K. N. (2001, 6). Monetary policy surprises and interest rates: Evidence from the fed funds futures market. *Journal of Monetary Economics* 47, 523–544.
- Leahy, J. V. and A. Thapar (2022). Age structure and the impact of monetary policy. *American Economic Journal: Macroeconomics*.

- Lundberg, S. (1985, 1). The added worker effect. *Journal of Labor Economics* 3, 11–37.
- Maggio, M. D., A. Kermani, B. J. Keys, T. Piskorski, R. Ramcharan, A. Seru, and V. Yao (2017, 11). Interest rate pass-through: Mortgage rates, household consumption, and voluntary deleveraging. *American Economic Review* 107, 3550–88.
- Mankart, J. and R. Oikonomou (2016, 6). The rise of the added worker effect. *Economics Letters* 143, 48–51.
- Mankart, J. and R. Oikonomou (2017, 10). Household search and the aggregate labour market. *Review of Economic Studies* 84, 1735–1788.
- Mankart, J., R. Oikonomou, and F. Pascucci (2022). The rise of household insurance. *Working Paper*.
- Mennuni, A. (2019, 7). The aggregate implications of changes in the labour force composition. *European Economic Review* 116, 83–106.
- Miranda-Agrippino, S. and G. Ricco (2021, 7). The transmission of monetary policy shocks. *American Economic Journal: Macroeconomics* 13, 74–107.
- Nakamura, E. and J. Steinsson (2018, 8). High-frequency identification of monetary non-neutrality: The information effect. *The Quarterly Journal of Economics* 133, 1283–1330.
- Olsson, J. (2025, 1). Singles, couples, and their labor supply: Long-run trends and short-run fluctuations. *American Economic Journal: Macroeconomics* 17, 1–34.
- Ottonello, P. and T. Winberry (2020). Financial heterogeneity and the investment channel of monetary policy. *Econometrica* 88, 2473–2502.
- Parker, S. W. and E. Skoufias (2004, 8). The added worker effect over the business cycle: evidence from urban mexico. *Applied Economics Letters* 11, 625–630.
- Ramey, V. A. (2016, 1). Macroeconomic shocks and their propagation. *Handbook of Macroeconomics* 2, 71–162.
- Romer, C. D. and D. H. Romer (2004, 8). A new measure of monetary shocks: Derivation and implications. *American Economic Review* 94, 1055–1084.
- Rudebusch, G. D. (1998, 11). Do measures of monetary policy in a var make sense? *International Economic Review* 39, 907.
- Söderström, U. (2001, 4). Predicting monetary policy with federal funds futures prices. *Journal of Futures Markets* 21, 377–391.
- Stephens, M. (2002, 7). Worker displacement and the added worker effect. *Journal of Labor Economics* 20, 504–537.
- Swanson, E. T. (2021, 3). Measuring the effects of federal reserve forward guidance and asset purchases on financial markets. *Journal of Monetary Economics* 118, 32–53.

- Thapar, A. (2008, 5). Using private forecasts to estimate the effects of monetary policy. *Journal of Monetary Economics* 55, 806–824.
- Wang, H. (2019, 10). Intra-household risk sharing and job search over the business cycle. *Review of Economic Dynamics* 34, 165–182.
- White, N. (2018). Gross Worker Flows, Job Loss, and Monetary Policy. *Working Paper*.
- Wieland, J. F. and M. J. Yang (2020, 2). Financial dampening. *Journal of Money, Credit and Banking* 52, 79–113.
- Woytinsky, W. S. (1942). *Three aspects of labor dynamics*. Washington D.C: Social Science Research Council.
- Wu, C. and D. Krueger (2021, 1). Consumption insurance against wage risk: Family labor supply and optimal progressive income taxation. *American Economic Journal: Macroeconomics* 13, 79–113.

Appendix

A Data

State employment comes from three sources: the LEHD, BLS payrolls and the CPS. LEHD employment is beginning-of-quarter private sector employment, all industries, in the Quarterly Workforce Indicators of the U.S. Census Bureau, downloaded in September 2026. These are administrative data. Employment is counted from the unemployment insurance wage record of each individual worker, not reported by employers as a headcount and not estimated from a survey. A job counts at the start of a quarter if the worker has earnings from that employer in that quarter and the previous one. The LEHD covers over 95 percent of U.S. private sector jobs. We seasonally adjust it with X-13ARIMA-SEATS. States enter the program at different dates, listed in Table A.1. The extended series fills the missing quarters with the growth of BLS payroll employment less federal government. BLS payroll employment is CES total nonfarm employment from FRED, downloaded in June and September 2026. It is estimated from a monthly survey in which establishments report how many workers were on their payroll in the pay period including the 12th. The BLS publishes no seasonally adjusted state series before 1990, so for earlier years we adjust it ourselves. The other state outcomes are the LAUS unemployment rate from FRED, QCEW total wages from the BLS (downloaded in March 2025), QWI earnings times employment, and BEA wages and salaries (SQINC4, downloaded in June 2026). Monthly series are averaged within the quarter.

Household data come from the IPUMS CPS basic monthly files (Flood et al., 2024), downloaded in June 2025 and July 2026, and from all 20 SIPP panels between 1984 and 2023, downloaded in June 2026. CPS counts use the final weights. Flows follow the same person across adjacent months with the IPUMS person identifier and use the link weights. The 1994 to 1995 CPS redesign breaks these links for a few months in 1995. We drop those months for flows and interpolate them for marginality. Wages come from the outgoing rotation groups. They exclude the self-employed, who are not asked about earnings. In the SIPP, spouses are linked by the spouse pointer up to the 2008 panel. From the 2014 panel on, the couple is the two lowest-numbered married adults of opposite sex in the household.

The baseline shock is the federal funds rate factor of Swanson (2021), from 1991Q4 to 2019Q2. We rescale it to percentage points of the federal funds rate, so that one standard deviation equals 8.39 basis points. The shock for the sample that ends before the zero lower bound, from 1990Q2 to 2008Q4, is the change in the current-month federal funds futures rate around FOMC announcements. It comes from Gurkaynak et al. (2005) up to 1993 and from Gorodnichenko and Weber (2016) after. Each announcement enters its own quarter with the share of the quarter still to come and the next quarter with the rest. The alternative shocks are the Romer and Romer (2004) shocks updated by Wieland and Yang (2020) as provided by Ramey (2016), the forward guidance and asset purchase factors of Swanson (2021), and the shocks of Miranda-Agrippino and Ricco (2021) and Bauer and Swanson (2023).

The state characteristics used as controls come from the QWI (industry and finance employment shares, downloaded in November 2025), from FRED (the BLS CES private

employment share, BEA personal income per capita and the Census Housing Vacancy Survey homeownership rate, downloaded in July 2026), from the CPS (population age shares and shares working), from [Boone et al. \(2021\)](#) (unemployment insurance generosity, downloaded in November 2024) and from [Fleck et al. \(2025\)](#) (tax progressivity, downloaded in May 2026). The predictors of S^{MW} in Table B.3 come from IPUMS USA decennial Census and American Community Survey microdata. For all of them except the education gap in couples, we use the IPUMS USA files in the replication package of [Fukui et al. \(2023\)](#), downloaded in June 2026. The education gap comes from our own IPUMS USA extract, downloaded in July 2026. Mean commute time is from IPUMS NHGIS (downloaded in June 2026), the urban share from the 2010 Census, and the remaining predictors from the CPS. The Great Recession house-price drop uses the FHFA purchase-only house price index. The national series are private employment, the CPI and the federal funds rate from FRED, and the excess bond premium of [Favara, Gilchrist, Lewis, and Zakrajšek \(2016\)](#). All were downloaded in September 2026.

Table A.1: LEHD employment: coverage by state

State	First	Last	State	First	Last	State	First	Last
AK	2000Q2	2016Q2	KY	2001Q2	2025Q4	NY	2000Q2	2025Q4
AL	2001Q2	2025Q4	LA	1995Q2	2025Q4	OH	2000Q2	2025Q4
AR	2002Q4	2025Q4	MA	2010Q2	2025Q4	OK	2000Q2	2025Q4
AZ	2004Q2	2025Q4	MD	1990Q2	2025Q4	OR	1991Q2	2025Q4
CA	1991Q4	2025Q4	ME	1996Q3	2025Q4	PA	1997Q2	2025Q4
CO	1993Q3	2025Q4	MI	2000Q4	2021Q4	RI	1995Q2	2025Q4
CT	1996Q2	2025Q4	MN	1994Q4	2025Q4	SC	1998Q2	2025Q4
DC	2005Q3	2025Q4	MO	1995Q2	2025Q4	SD	1998Q2	2025Q4
DE	1998Q4	2025Q4	MS	2003Q4	2025Q4	TN	1998Q2	2025Q4
FL	1998Q1	2025Q4	MT	1993Q2	2025Q4	TX	1995Q2	2025Q4
GA	1998Q2	2025Q4	NC	1993Q1	2025Q4	UT	1999Q4	2025Q4
HI	1996Q1	2025Q4	ND	1998Q2	2025Q4	VA	1998Q4	2025Q4
IA	1999Q1	2025Q4	NE	1999Q2	2025Q4	VT	2000Q2	2025Q4
ID	1991Q2	2025Q4	NH	2003Q2	2025Q4	WA	1990Q2	2025Q4
IL	1993Q3	2025Q4	NJ	1996Q2	2025Q4	WI	1990Q2	2025Q4
IN	1998Q2	2025Q4	NM	1995Q4	2025Q4	WV	1997Q2	2025Q4
KS	1993Q2	2025Q4	NV	1998Q2	2025Q4	WY	2001Q2	2025Q4

Notes: Quarterly Workforce Indicators (LEHD), private sector employment, seasonally adjusted with X-13ARIMA-SEATS. First and last quarter with data in each state; the panel is unbalanced because states entered the program at different dates.

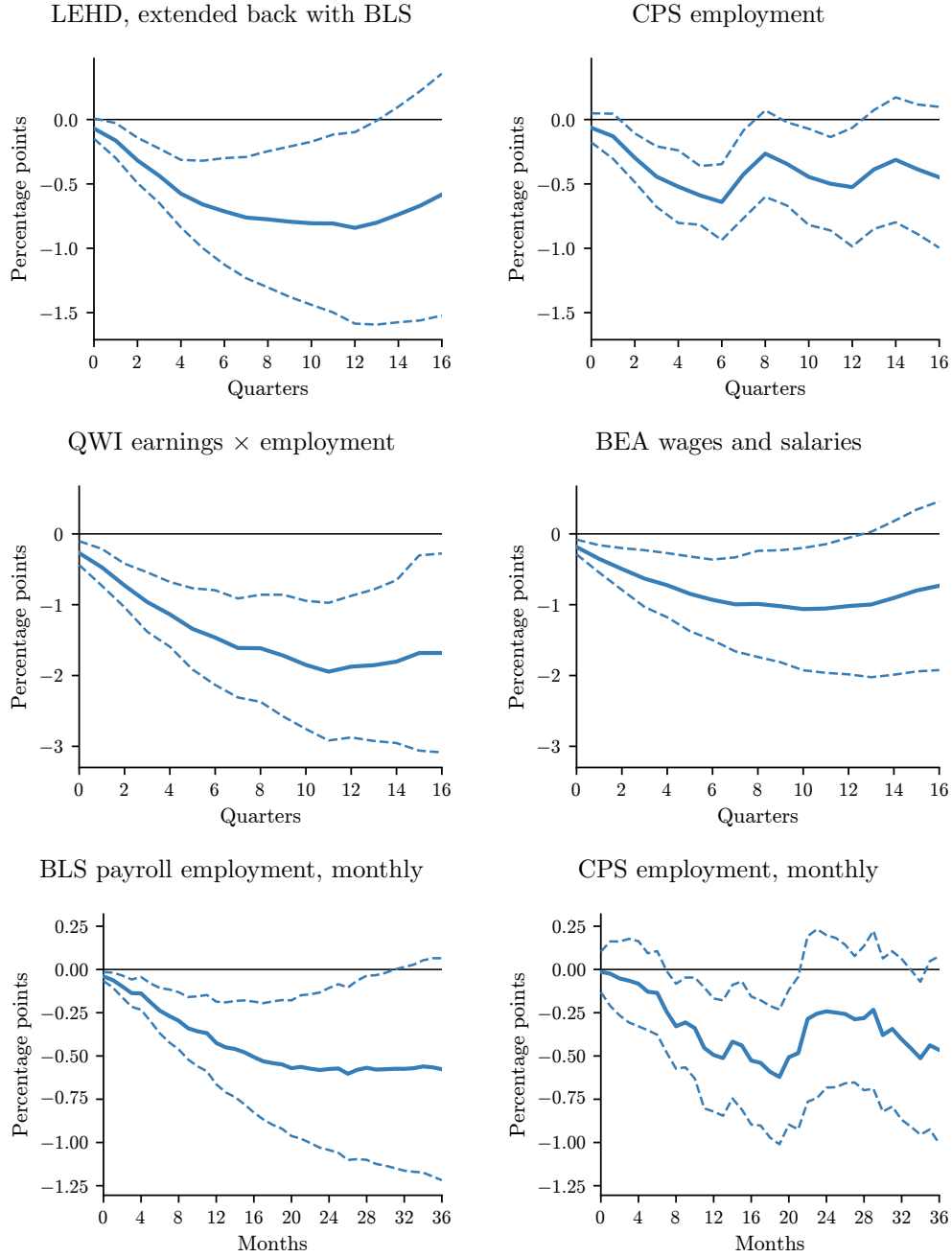
B Robustness of the main result

Table B.1: Effect of an increase in the share of married women working on the response of employment to a monetary policy shock

Quarter	LEHD employment				BLS payroll employment			
	β^k	(s.e.)	p	N	β^k	(s.e.)	p	N
0	-0.088	(0.054)	0.104	4,381	-0.097	(0.030)	0.002	5,661
1	-0.206	(0.102)	0.046	4,380	-0.194	(0.057)	0.001	5,661
2	-0.407	(0.142)	0.005	4,379	-0.317	(0.078)	0.000	5,661
3	-0.588	(0.165)	0.001	4,378	-0.416	(0.102)	0.000	5,661
4	-0.773	(0.195)	0.000	4,377	-0.506	(0.132)	0.000	5,661
5	-0.906	(0.261)	0.001	4,376	-0.576	(0.166)	0.001	5,661
6	-0.995	(0.328)	0.003	4,375	-0.630	(0.201)	0.002	5,661
7	-1.092	(0.356)	0.003	4,374	-0.645	(0.233)	0.007	5,661
8	-1.139	(0.368)	0.003	4,373	-0.667	(0.263)	0.013	5,661
9	-1.221	(0.383)	0.002	4,372	-0.657	(0.293)	0.027	5,661
10	-1.265	(0.395)	0.002	4,371	-0.657	(0.322)	0.044	5,661
11	-1.310	(0.435)	0.003	4,369	-0.646	(0.352)	0.069	5,661
12	-1.379	(0.464)	0.004	4,367	-0.647	(0.377)	0.089	5,661
13	-1.352	(0.486)	0.006	4,365	-0.607	(0.400)	0.133	5,661
14	-1.336	(0.502)	0.009	4,363	-0.533	(0.425)	0.212	5,661
15	-1.280	(0.527)	0.017	4,361	-0.472	(0.450)	0.297	5,661
16	-1.209	(0.556)	0.032	4,359	-0.413	(0.479)	0.390	5,661

Notes: Coefficient on shock $\times S_{s,t-1}^{MW}$ in the local projection of the log outcome, $\times 100$, so the units are the extra percent change in employment k quarters after a one percentage point surprise, in a state where S^{MW} is one percentage point higher. Driscoll–Kraay standard errors. 1991Q4–2019Q2.

Figure B.1: Alternative outcomes



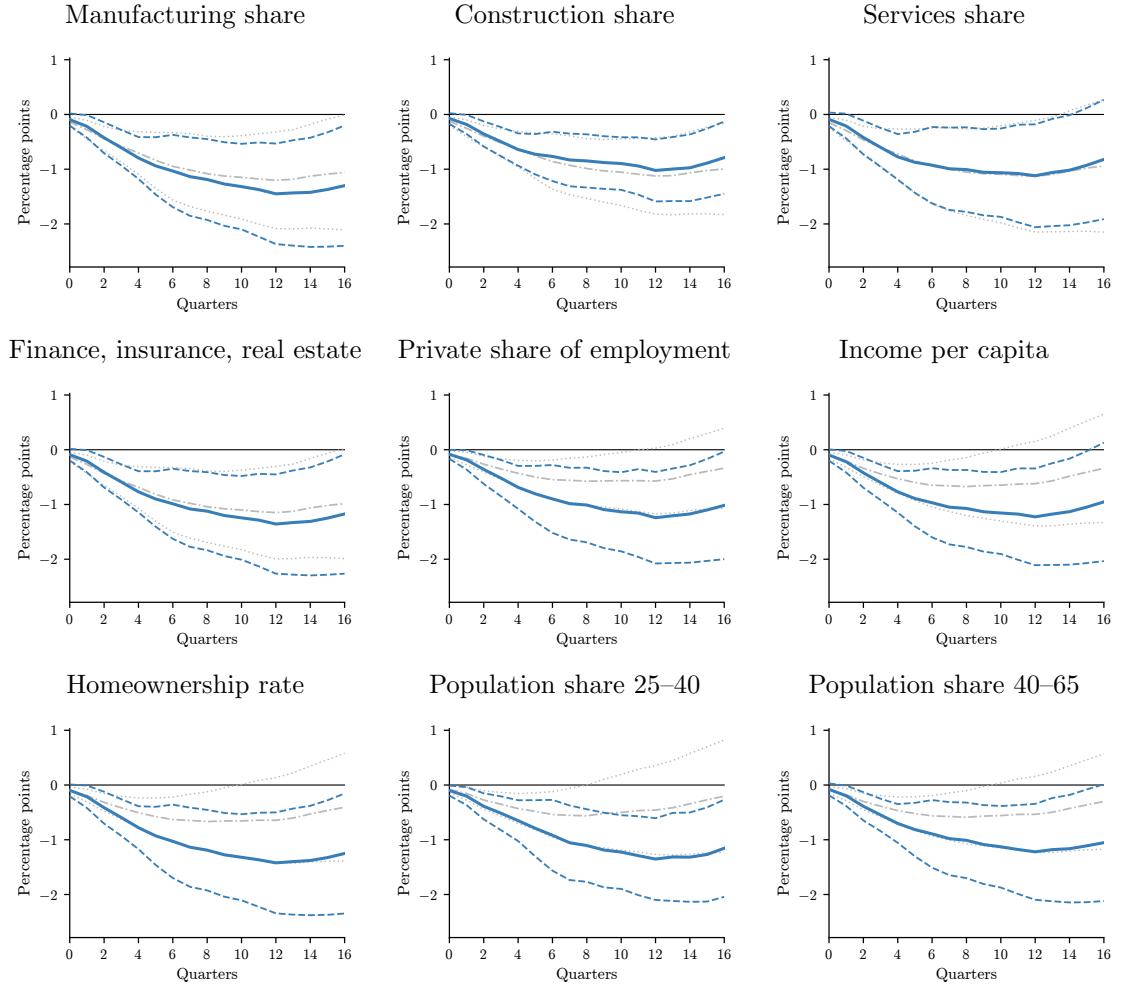
Notes: Logs. Top row: employment; LEHD extended back with the growth of BLS employment (nonfarm less federal government) before a state enters the LEHD. Middle row: total payroll, seasonally adjusted. Bottom row: monthly, horizons in months. CPS panels population-weighted (2010). Plotted against the horizon k : the coefficient on shock $\times S_{s,t-1}^{MW}$ in the local projection of the outcome from $t-1$ to $t+k$, with state and time fixed effects, $\times 100$, so the effect of a one percentage point higher S^{MW} on the response to a one percentage point surprise. 95 percent confidence intervals, Driscoll–Kraay.

Table B.2: Correlation of S^{MW} with other state characteristics, 1990Q1–2019Q2

	S^{MW}
Share of women 16+ working	0.22 (0.01)
Share of men 16+ working	−0.28 (0.01)
Share of all 16+ working	−0.03 (0.01)
Participation of married women 16+	0.53 (0.01)
Participation of women 16+	0.26 (0.01)
Participation of men 16+	−0.27 (0.01)
Participation of all 16+	−0.00 (0.01)
Population share 25–40	−0.45 (0.01)
Population share 40–65	0.39 (0.01)
Population share 65+	0.32 (0.01)
Personal income per capita	0.32 (0.01)
Manufacturing share of employment	0.01 (0.01)
Construction share of employment	−0.36 (0.01)
Services share of employment	0.13 (0.02)
Finance, insurance and real estate share	−0.06 (0.02)
Private share of employment	−0.16 (0.01)
Homeownership rate	0.03 (0.01)
Tax progressivity, state	0.25 (0.01)
Tax progressivity, state and federal	0.36 (0.01)
Maximum UI weekly benefit	0.28 (0.01)
Maximum weeks of UI	0.01 (0.01)
Maximum UI weeks $\times$ amount	0.28 (0.01)

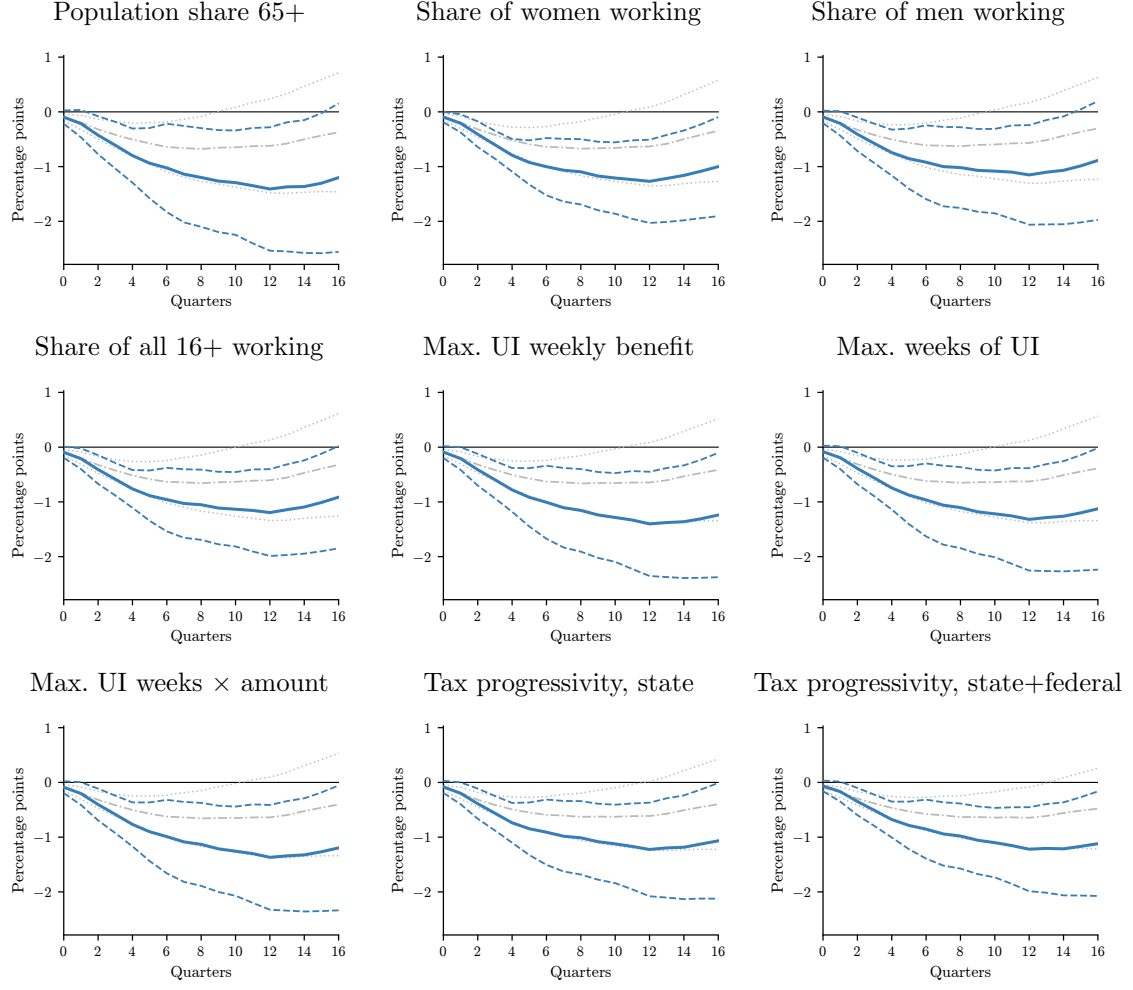
Notes: Correlations across state-quarters, levels; standard errors in parentheses. Sources: employment and participation, CPS; industry and finance shares, QWI; private employment share, BLS CES; income per capita, BEA; homeownership, Census Housing Vacancy Survey; tax progressivity, [Fleck et al. \(2025\)](#), 2005, 2010 and 2015 estimates carried forward; UI generosity, [Boone et al. \(2021\)](#), carried forward after 2014Q4; population age shares, CPS.

Figure B.2: Controlling for other state characteristics (I)



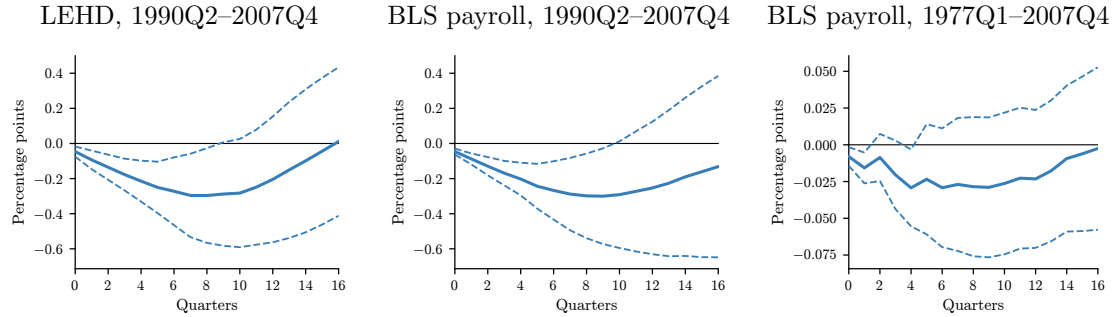
Notes: Each panel adds the named $X_{s,t-1}$ and its interaction with the shock. Sources: industry and finance shares, QWI; private employment share, BLS CES; income per capita, BEA; homeownership, Census Housing Vacancy Survey; working shares, CPS; UI generosity, [Boone et al. \(2021\)](#), through 2014Q4 and held at that value after; population age shares, CPS. Plotted: Plotted against the horizon k in quarters: the coefficient on shock $\times S_{s,t-1}^{MW}$ in the local projection of log state employment (LEHD solid, BLS payroll grey dash-dotted) from $t-1$ to $t+k$, with state and time fixed effects, $\times 100$, so the effect of a one percentage point higher S^{MW} on the response to a one percentage point surprise. 95 percent confidence intervals, Driscoll-Kraay.

Figure B.3: Controlling for other state characteristics (II)



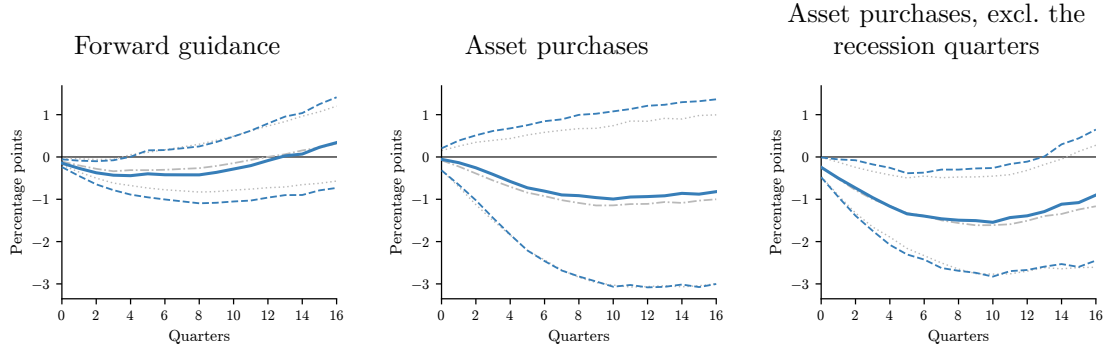
Notes: As in Figure B.2. Tax progressivity: [Fleck et al. \(2025\)](#).

Figure B.4: Romer and Romer (2004) shocks



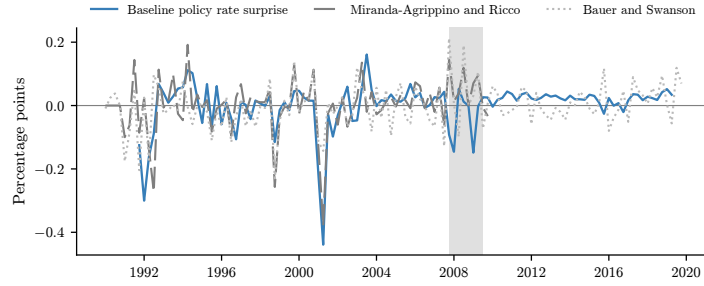
Notes: [Romer and Romer \(2004\)](#) narrative shocks, updated by [Wieland and Yang \(2020\)](#), from [Ramey \(2016\)](#). Shock $\times S_{s,t-1}^{MW}$; state and time fixed effects; $\times 100$. 95 percent confidence intervals, Driscoll–Kraay.

Figure B.5: Forward guidance and asset purchase factors



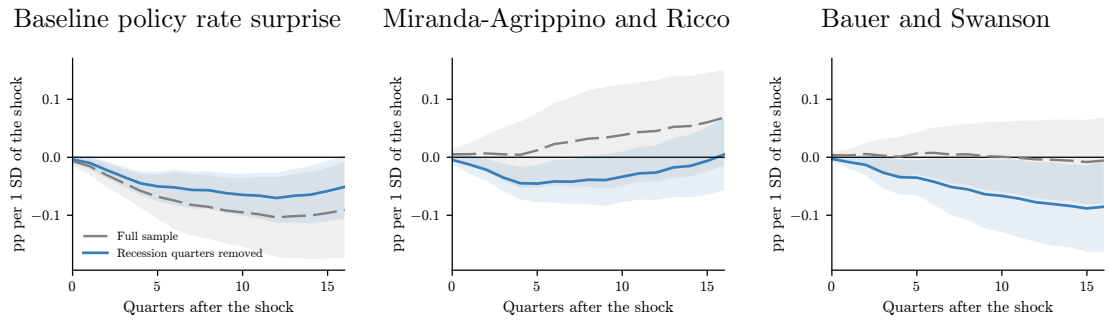
Notes: Swanson (2021) forward guidance and asset purchase factors, 1991Q4–2019Q2. Right: the factor set to its non-recession mean in 2007Q4–2009Q2 (NBER recession quarters). Factor $\times S_{s,t-1}^{MW}$ in the local projection of log state employment (LEHD solid, BLS payroll grey dash-dotted); $\times 100$. 95 percent confidence intervals, Driscoll–Kraay.

Figure B.6: Monetary policy shock series



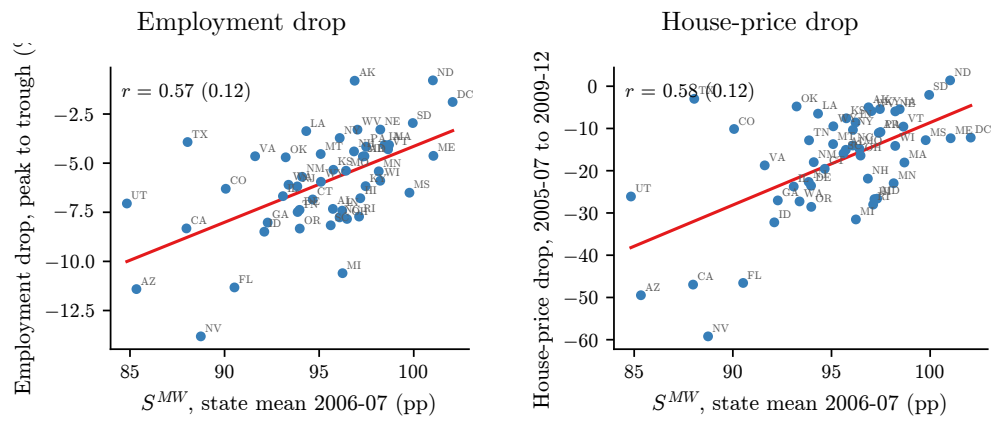
Notes: Quarterly series of the baseline policy rate surprise, Miranda-Agrippino and Ricco (2021), and Bauer and Swanson (2023). NBER recession quarters shaded (2007Q4–2009Q2). In percentage points.

Figure B.7: Miranda-Agrippino and Ricco (2021) and Bauer and Swanson (2023) shocks



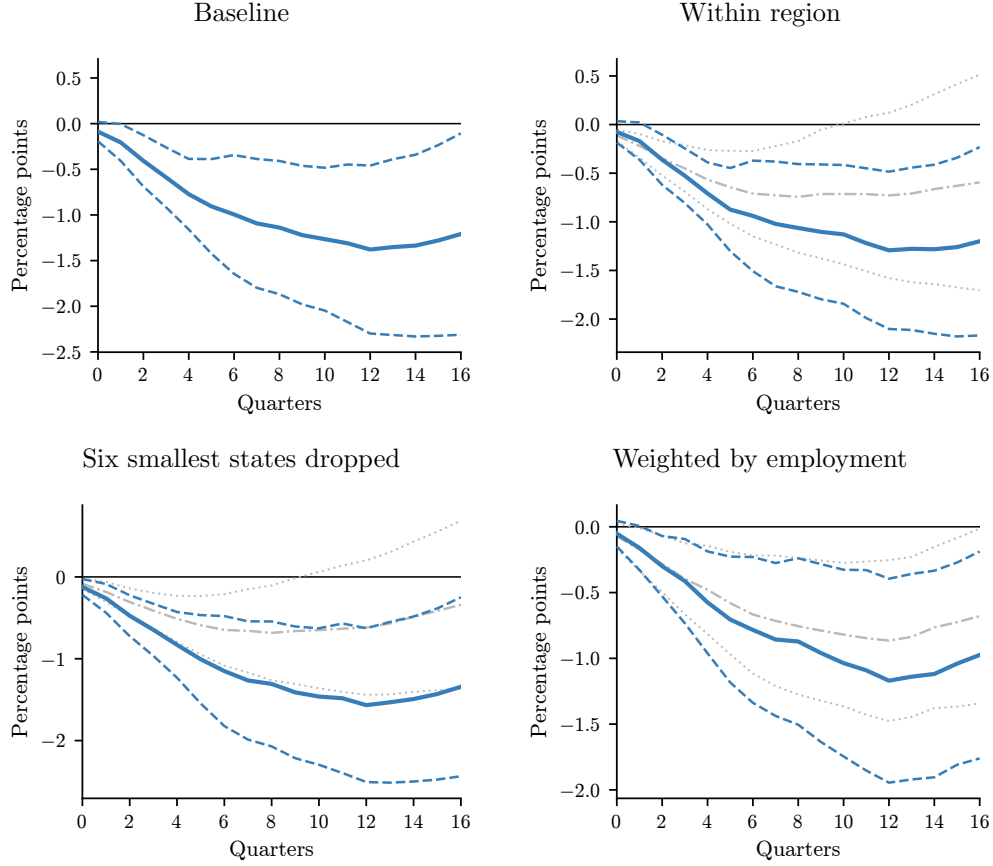
Notes: Shock $\times S_{s,t-1}^{MW}$ in the local projection of log state employment (LEHD solid, BLS payroll grey dash-dotted), per one standard deviation of the shock over its estimation sample. Solid: the shock set to its non-recession mean in 2007Q4–2009Q2 (NBER recession quarters). Grey, long-dashed: full sample. Shaded: 95 percent confidence intervals, Driscoll–Kraay.

Figure B.8: Share of married women working and the severity of the Great Recession across states



Notes: Dots: states. Horizontal axis: S^{MW} , state mean 2006-07. Left: employment drop, peak to trough. Right: house-price drop, 2005-07 to 2009-12. Red: OLS fit.

Figure B.9: Alternative specifications of the baseline



Notes: Within region: the shock interacted with BEA region dummies is added. Six smallest states: ranked by mean employment over 1990–2008. Weighted: states weighted by population (2010). Plotted: Plotted against the horizon k in quarters: the coefficient on $\text{shock} \times S_{s,t-1}^{MW}$ in the local projection of log state employment (LEHD solid, BLS payroll grey dash-dotted) from $t - 1$ to $t + k$, with state and time fixed effects, $\times 100$, so the effect of a one percentage point higher S^{MW} on the response to a one percentage point surprise. 95 percent confidence intervals, Driscoll–Kraay.

B.1 The national response

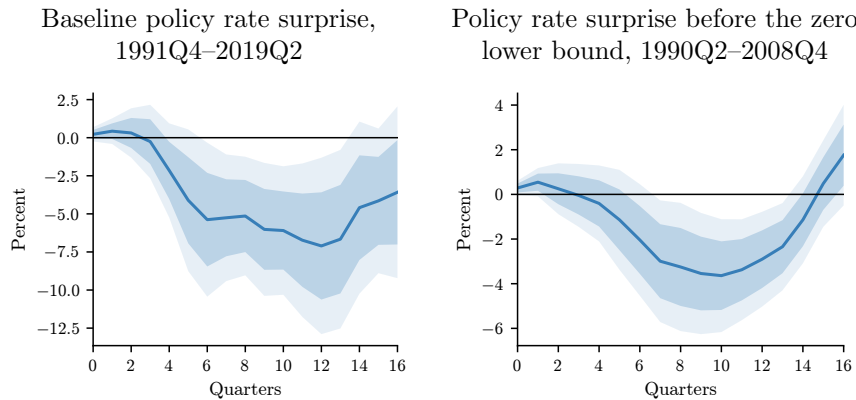
Our estimates are differences across states, since the time fixed effects absorb whatever the shock does to the country as a whole. The national response is what those differences are set against, so we estimate it here. We follow the aggregate specification of [Leahy and Thapar \(2022\)](#) and run the local projection of [Jorda \(2005\)](#) on national quarterly data,

$$100 \Delta \ln EMP_{t+k} = \phi^k \epsilon_t + \sum_{j=1}^2 \Gamma_j^k Z_{t-j} + c^k + v_{t+k},$$

where EMP is private employment from FRED and Z collects two lags each of employment, the shock, the federal funds rate, the consumer price index and the excess bond premium of [Favara et al. \(2016\)](#). Figure B.10 plots ϕ^k with 68 and 90 percent bands based on Newey–West standard errors, for our shock and for the one that ends before the zero lower bound. Employment falls for about three years and recovers, which is the standard picture. Figure B.11 asks the same question of the flows behind that employment response. We take the monthly CPS, link each respondent to the following month, and count the moves from outside the labor force into it and back out, and from non-employment into employment and back out, for people aged 25 to 55, summing the states into national counts. Each flow is cumulated from t to $t+k$ and divided by the group’s population before the shock, so the outcome is a share and the coefficient is in percentage points. The controls are as above, with log private employment as the activity term, and the bands are 90 percent Newey–West.

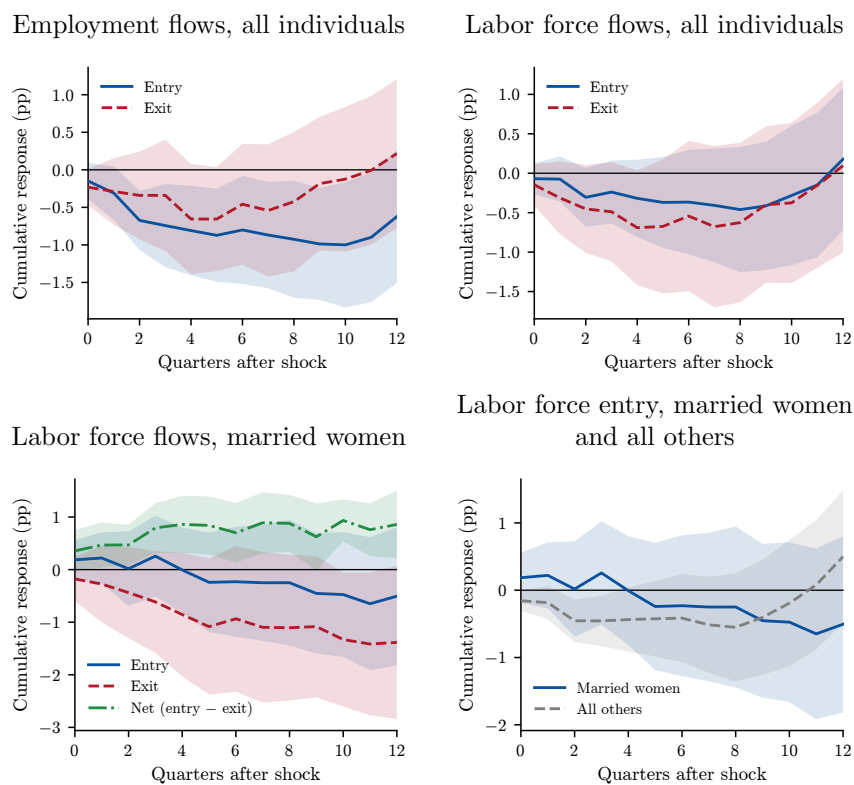
The panel for married women is worth a look. Their entry into the labor force is flat, if anything weakly positive in the first year. Their exits fall. On net more married women are in the labor force after a tightening, and this net response is significant at most horizons. This is suggestive evidence for an added-worker-effect-like response on the aggregate.

Figure B.10: Aggregate employment response to the monetary policy shock



Notes: National local projection of $100 \times \log$ private employment (FRED USPRIV) on the shock; two lags each of employment, the shock, the federal funds rate, log CPI, the excess bond premium. Per unit of the shock. Shaded: 68 and 90 percent confidence intervals, Newey–West.

Figure B.11: Aggregate labor market flows after a monetary policy shock



Notes: National local projections. CPS 25–55, seasonally adjusted. Outcome: flows over t to $t + k$, share of the group's base-period population; $\times 100$. Two lags each of the shock, the federal funds rate, the excess bond premium, log CPI, log private employment (FRED USPRIV). Shaded: 90 percent confidence intervals, Newey–West. Shock: the baseline policy rate surprise, 1991Q4–2019Q2, leads through 2019Q4.

B.2 Predictors of S^{MW}

Our measure of the employment of married women is potentially endogenous, and we lack an instrument. We therefore look for cross-state variation that predicts S^{MW} and is more structural, slow moving, the result of long-term decisions and not explicitly tied to the current state of the economy. We then ask whether that kind of variation also moves the response to monetary policy. This second step can be interpreted as a reduced form exercise with the predictor serving as an instrument. Table B.3 lists the predictors and reports how each of them correlates with S^{MW} , across states and pooled over state-quarters. We find two of them particularly compelling. The education gap in couples is close to a structural characteristic of a state, as education and marriage decisions are made over a long horizon and are not tied to the current state of the labor market. The pre-sample levels of S^{MW} use variation that is determined before any of our shocks.

The top row of Figure B.12 puts each predictor in the place of S^{MW} in equation (1), for LEHD employment on the left and BLS payroll employment on the right. Each panel carries one line per predictor, with its confidence band shaded, so the bands darken where they overlap. The education gap is drawn in blue and the remaining predictors in grey. Every predictor and S^{MW} are standardized, and predictors that correlate negatively with S^{MW} are sign-flipped, so the lines are comparable and point the same way. Each of them tells the same story. Employment falls further where the predictor says married women work more.

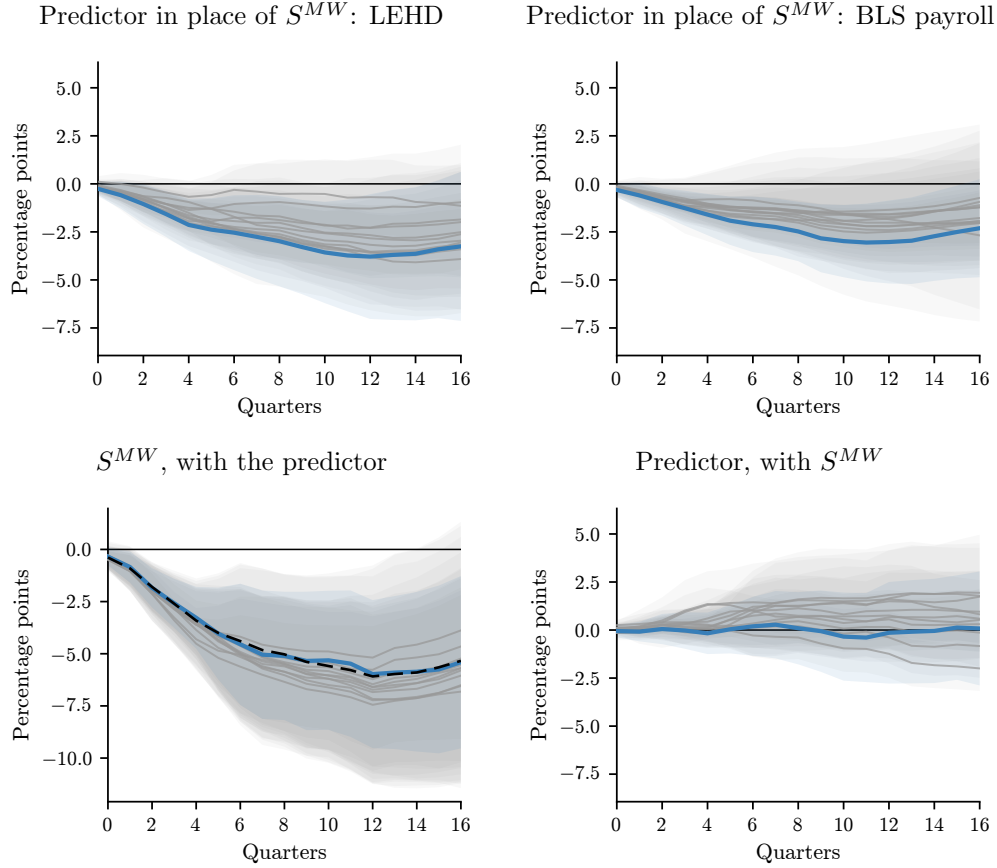
The bottom row reports the horse race of equation (2), with the coefficient on S^{MW} on the left and the coefficient on the predictor on the right. The black dashed line is the baseline, the estimate of S^{MW} on its own. The effect of S^{MW} is virtually unchanged when a predictor is added, while the effect of the predictor weakens and typically drops out altogether. This is also reassuring. We conclude that it is S^{MW} itself that matters.

Table B.3: Cross-state predictors of S^{MW}

	State means	Pooled
Education gap in couples (Census/ACS)	−0.69 (0.10)	−0.54 (0.07)
Couples where the wife is more educated (CPS)	0.63 (0.11)	0.49 (0.08)
Married women, youngest child < 1 (Census)	−0.63 (0.11)	−0.49 (0.07)
Married women, youngest child 1–2 (Census)	−0.62 (0.11)	−0.49 (0.08)
Population share under 5 (Census)	−0.62 (0.11)	−0.49 (0.09)
Household size (Census)	−0.60 (0.11)	−0.47 (0.10)
Hispanic share (Census)	−0.63 (0.11)	−0.49 (0.11)
Foreign-born share (Census)	−0.40 (0.13)	−0.31 (0.10)
Urban share (Census)	−0.46 (0.13)	−0.35 (0.12)
excluding DC	−0.56 (0.12)	−0.43 (0.10)
Mean commute (NHGIS)	−0.17 (0.14)	−0.14 (0.10)
excluding DC	−0.26 (0.14)	−0.20 (0.09)
S^{MW} level, 1990 (CPS)	0.86 (0.07)	0.67 (0.06)
S^{MW} mean, 1980–89 (CPS)	0.70 (0.10)	0.55 (0.09)

Notes: Correlation between the predictor and S^{MW} over the sample: across states, using each state’s mean, and pooled over all state-quarters (the variation the local projections use). Standard errors in parentheses; pooled: clustered by state. In brackets, where it differs by 0.10 or more: the correlation excluding the District of Columbia. S^{MW} is the moderator of the regressions. Census and Census/ACS: IPUMS USA microdata, from the replication package of Fukui, Nakamura and Steinsson (2023) except the education gap (own IPUMS USA extract). Education gap: state mean over married couples of the husband’s years of schooling minus the wife’s. Wife more educated: share of married couples in which the wife’s years of schooling exceed the husband’s. Youngest-child shares: married women whose youngest child is in that age band. Population share under 5, Hispanic share, foreign-born share and urban share: shares of the state population; household size: state mean. Mean commute: state mean travel time to work, in minutes. S^{MW} level 1990 and mean 1980–89: the moderator itself, measured before the sample.

Figure B.12: Predictors of S^{MW} as moderators



Notes: One line per predictor of Table B.3: education gap blue, the others grey. Predictor and S^{MW} standardized; predictors correlating negatively with S^{MW} sign-flipped. Top row: shock $\times$ predictor, in place of S^{MW} , for LEHD and for BLS payroll employment. Bottom left: the same for LEHD, with shock $\times S^{MW}$ in the projection. Bottom right: shock $\times S^{MW}$ in those projections; black dashed: S^{MW} alone. $\times 100$. Shaded: 95 percent confidence intervals, Driscoll–Kraay.

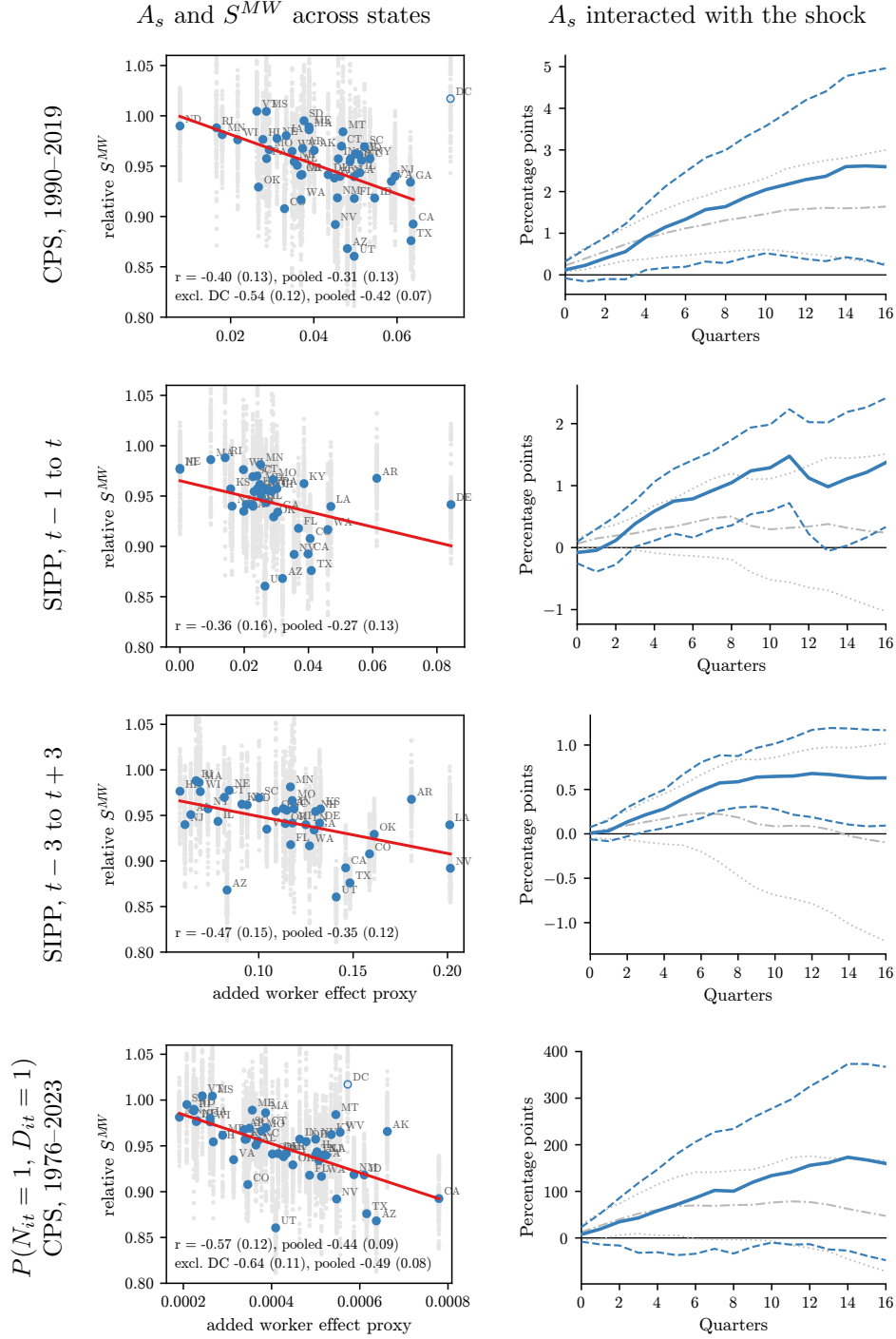
C Supplemental results for Section 5

Table C.1: Added worker effect proxies across states

Measure	Mean	Std. dev.	r with S^{MW}	
			All	Excl. DC
S^{MW}	95.13	3.47		
$P(N = 1 \mid D = 1)$	4.35	1.03	−0.55 (0.12)	−0.68 (0.11)
$P(N = 1 \mid D = 1)$, CPS 1990–2019	4.20	1.31	−0.40 (0.13)	−0.54 (0.12)
$P(N = 1 \mid D = 1) - P(N = 1 \mid D = 0)$	1.94	0.91	−0.47 (0.13)	−0.61 (0.11)
$P(N = 1 \mid D = 1, LF_{t-1}^w = 0)$ − $P(N = 1 \mid D = 0, LF_{t-1}^w = 0)$	6.75	3.45	−0.26 (0.14)	−0.41 (0.13)
$P(N = 1, D = 1)$	0.04	0.01	−0.57 (0.12)	−0.64 (0.11)
$P(N = 1 \mid D = 1)$, SIPP $t - 1$ to t	2.86	1.51	−0.36 (0.16)	−0.36 (0.16)
$P(N = 1 \mid D = 1)$, SIPP $t - 3$ to $t + 3$	11.40	3.68	−0.47 (0.15)	−0.47 (0.15)
M^{MW}	82.70	4.42	−0.67 (0.11)	−0.70 (0.10)

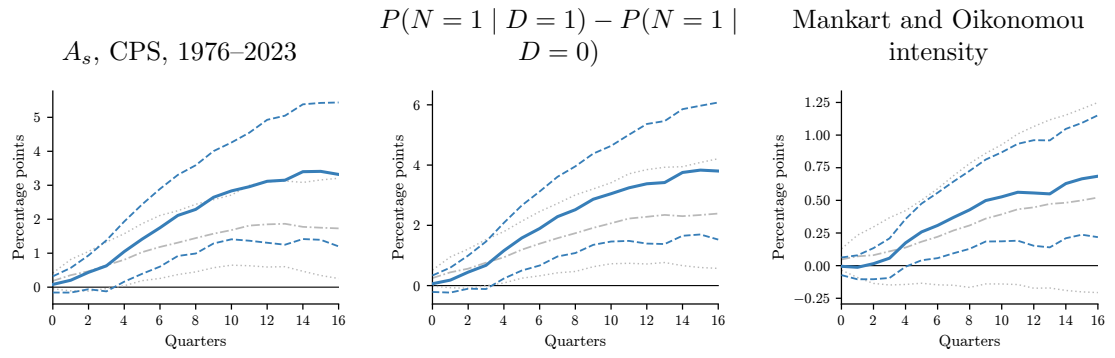
Notes: One value per state, in percentage points. Standard errors of r in parentheses. $D = 1$: husband employed in $t - 1$, unemployed in t . $N = 1$: wife out of the labor force in $t - 1$, in it in t . $LF_{t-1}^w = 0$: wife out of the labor force in $t - 1$; the fourth row follows the specification of [Mankart and Oikonomou \(2016\)](#). CPS: married couples, wife 25–55, monthly. SIPP: wife 20–60, husband 18–64, 1984–2023, 38 states.

Figure C.1: Alternative added worker effect proxies



Notes: As in Figure 3. Bottom row: $P(N_{it} = 1, D_{it} = 1) = A_s \cdot P(D_{it} = 1)$, DC excluded. SIPP: wife 20–60, husband 18–64, 38 states; entry within $t - 1$ to t or $t - 3$ to $t + 3$.

Figure C.2: Added worker effect proxies with the District of Columbia included



Notes: As in Figures 3 and 4, DC included.

Figure C.3: Horse races between S^{MW} and the moderators

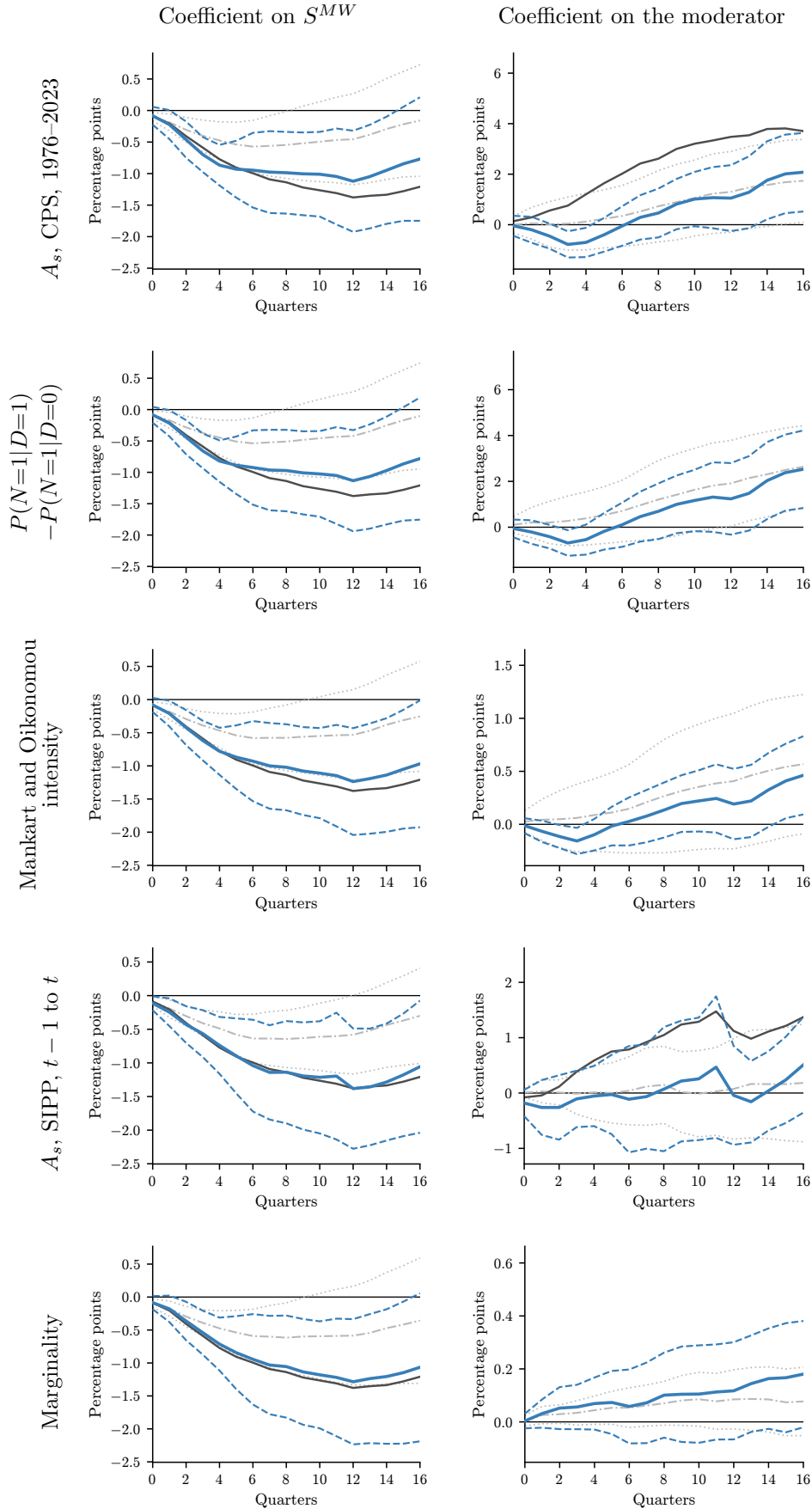
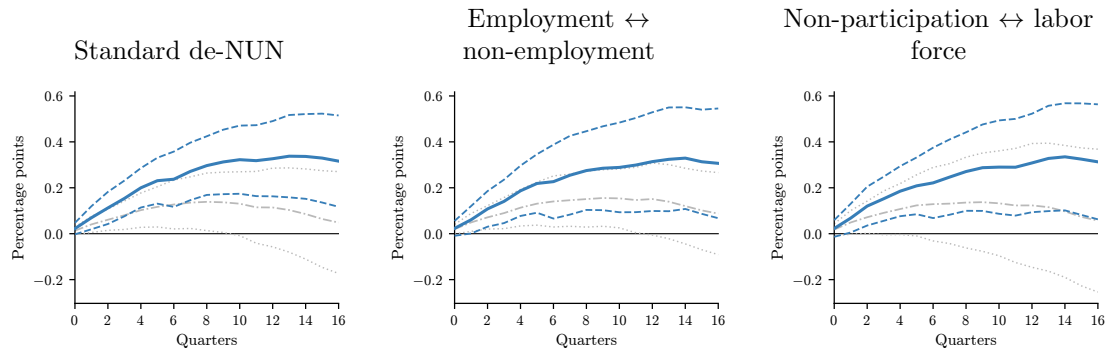


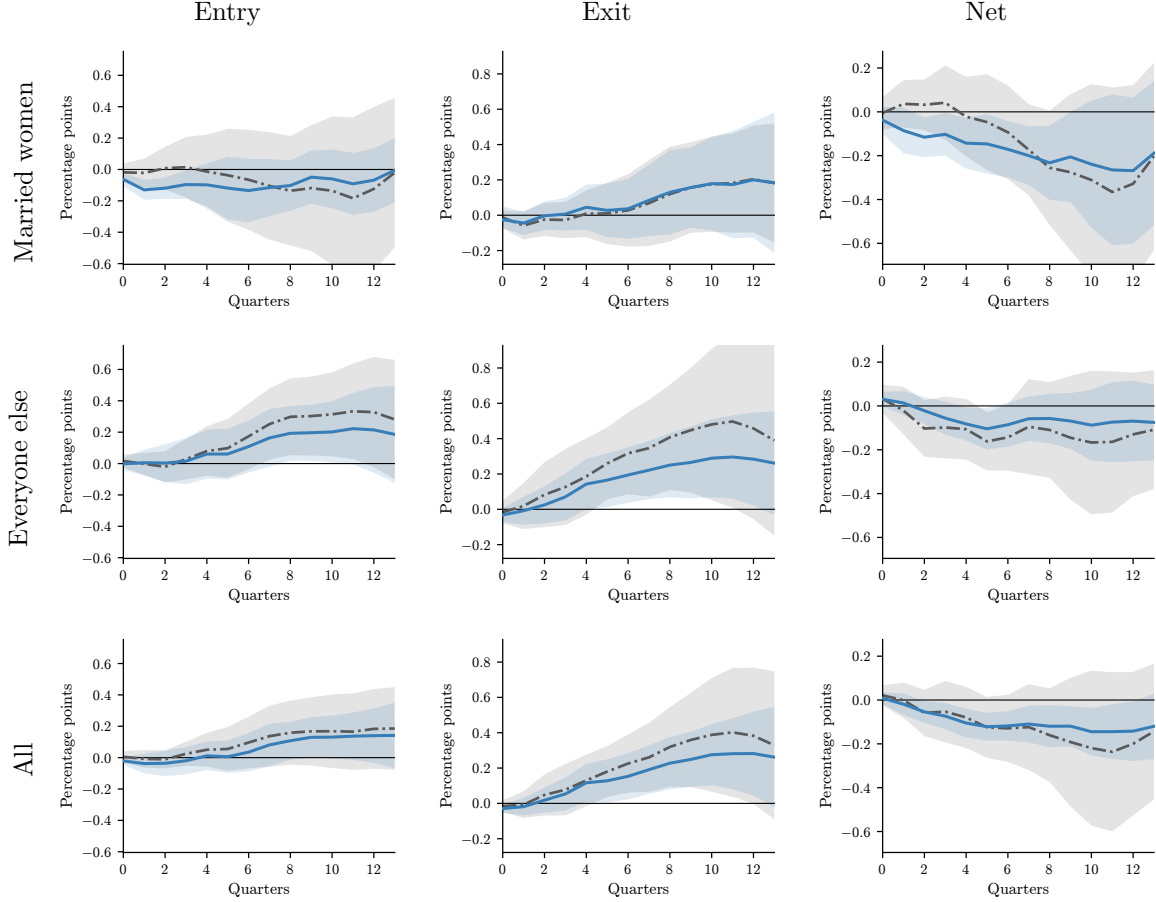
Figure C.4: Alternative marginality measures



Notes: The effect of a one percentage point higher value of each measure on the response of state employment to a one percentage point monetary policy surprise, k quarters after the shock, in percent. LEHD solid, BLS payroll grey dash-dotted. Left: N-U-N and U-N-U reversals recoded (Elsby et al., 2015). Middle, right: built from the transitions in the heading. 95 percent confidence intervals, Driscoll-Kraay.

Figure C.5 supplements the results in Figure 7. Entries, exits and the net of the two are in the three columns, each cumulated over t to $t + k$ as a share of the group's population, and the rows are married women, everyone else and the whole population. The blue lines are flows into and out of the labor force and the grey lines repeat the exercise for flows into and out of employment.

Figure C.5: Cumulative labor force and employment flows



Notes: CPS 25–55, one-month linked transitions, population-weighted (2010), as in Figure 7; exit and net flows defined analogously. Blue: flows into and out of the labor force. Grey, dash-dotted: flows into and out of employment; shaded: the two 95 percent confidence intervals.

Figure C.6 examines the intensive margin. We take hours worked last week from the monthly CPS, for everyone at work, the self-employed included. The top left panel is the state mean among people at work, seasonally adjusted. The bottom left panel takes the difference in hours between two adjacent months for each respondent, sums these differences from t to $t + k$ and divides by the average number of linked respondents, which can be read as an average cumulative change in hours. Wages are the hourly wage of the earner study, reported directly by those paid by the hour and computed as usual weekly earnings over usual hours for everyone else, deflated to real terms (available for employees only).¹⁸ The top right panel is the state median among workers. The bottom right panel

¹⁸A wage change requires a respondent to be linked between the fourth and the eighth month in the survey. This leaves about 10 percent of the observations behind the month to month hours changes.

takes the difference in wages between a respondent's two appearances in the earner study, which are twelve months apart, sums these differences from t to $t + k$ and divides by the average number of linked respondents, which can be read as an average cumulative year on year change in wages. Blue is married women and grey is everyone at work.

Average hours drop more in high S^{MW} states, and for married women at least as much as on average. Average hours are also affected by selection, since who works changes. The measure constructed from within-worker changes agrees, dropping more in high S^{MW} states. This is consistent with a direct intensive margin added worker effect response of married women. Married women in low S^{MW} states may raise their labor supply in response to a negative shock. It is also consistent with high S^{MW} states having a bigger drop in labor demand, the indirect effect. The second column presents the results on wages. The median wage falls more in high S^{MW} states, but not among married women. It would be tempting to read the married women result as weak evidence that the direct effect dominates for married women (labor supply differential dominating a labor demand differential). However, since the extensive margin is moving substantially, selection is likely to play a major role. Indeed, when measured from within-person wage changes we see that wages fall in high S^{MW} states more for both married women and the aggregate.

Figure C.6: Hours and wages among workers

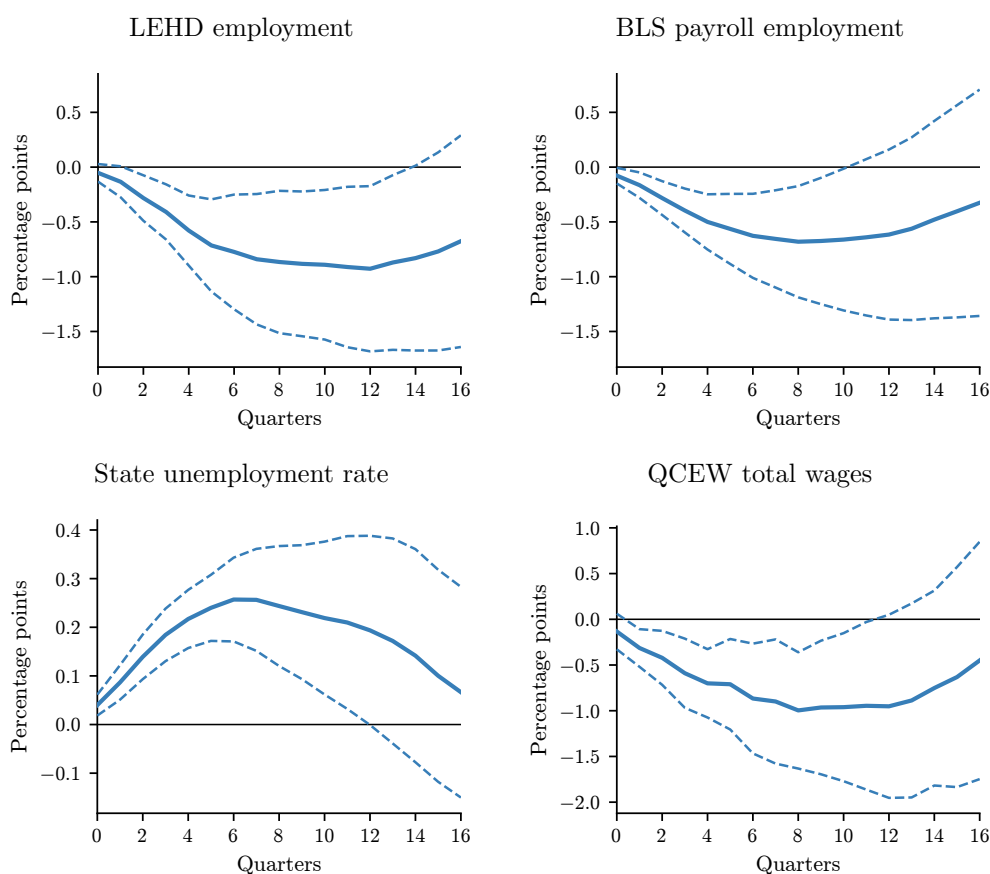


Notes: CPS 25–55, population-weighted (2010). Blue: married women; grey, dash-dotted: all. Top row: mean hours and median real hourly wage among workers; hours seasonally adjusted. Bottom row: within-person changes summed over t to $t + k$ and divided by the average number of linked workers; hours one-month link, wages outgoing rotation groups with a 12-month link. Shock $\times S_{s,t-1}^{MW}$; state and time fixed effects; $\times 100$. 95 percent confidence intervals, Driscoll–Kraay.

D Robustness: the sample before the zero lower bound

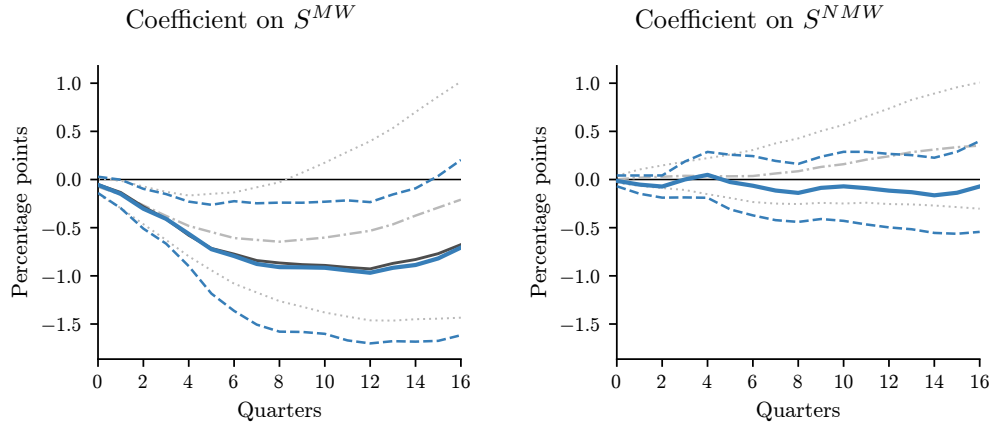
Here we repeat every result of the main text on the shorter sample that ends in 2008Q4, before the federal funds rate hit the zero lower bound. The shock is the change in the current month federal funds futures rate around FOMC announcements, described in Section 3, taken from [Gurkaynak et al. \(2005\)](#) up to 1993 and from [Gorodnichenko and Weber \(2016\)](#) after, and the sample runs from 1990Q2 to 2008Q4. The figures come in the order of the main text and each note names the figure it repeats.

Figure D.1: Effect of an increase in the share of married women working on the response of employment to a monetary policy shock



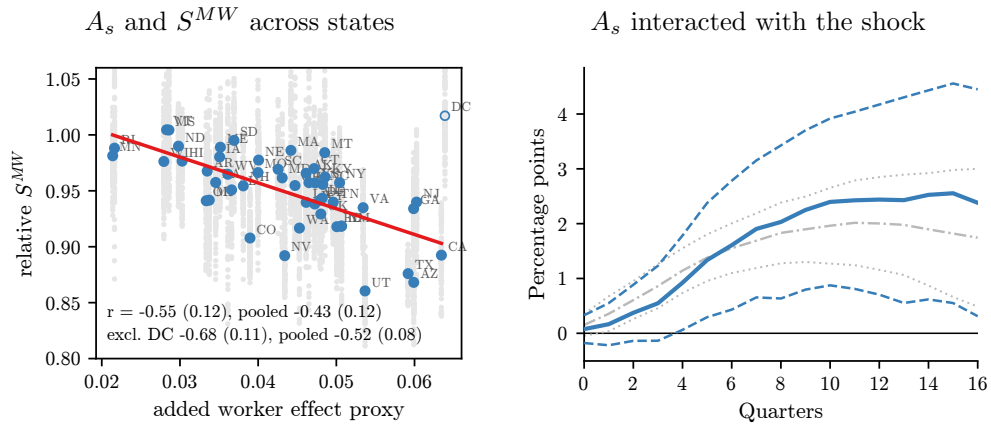
Notes: As in Figure 1, 1990Q2–2008Q4.

Figure D.2: Share of married women working and share of unmarried women working



Notes: As in Figure 2, 1990Q2–2008Q4.

Figure D.3: Added worker effect proxy across states: CPS, 1976–2023



Notes: As in Figure 3, 1990Q2–2008Q4.

Figure D.4: Alternative definitions of the added worker effect proxy

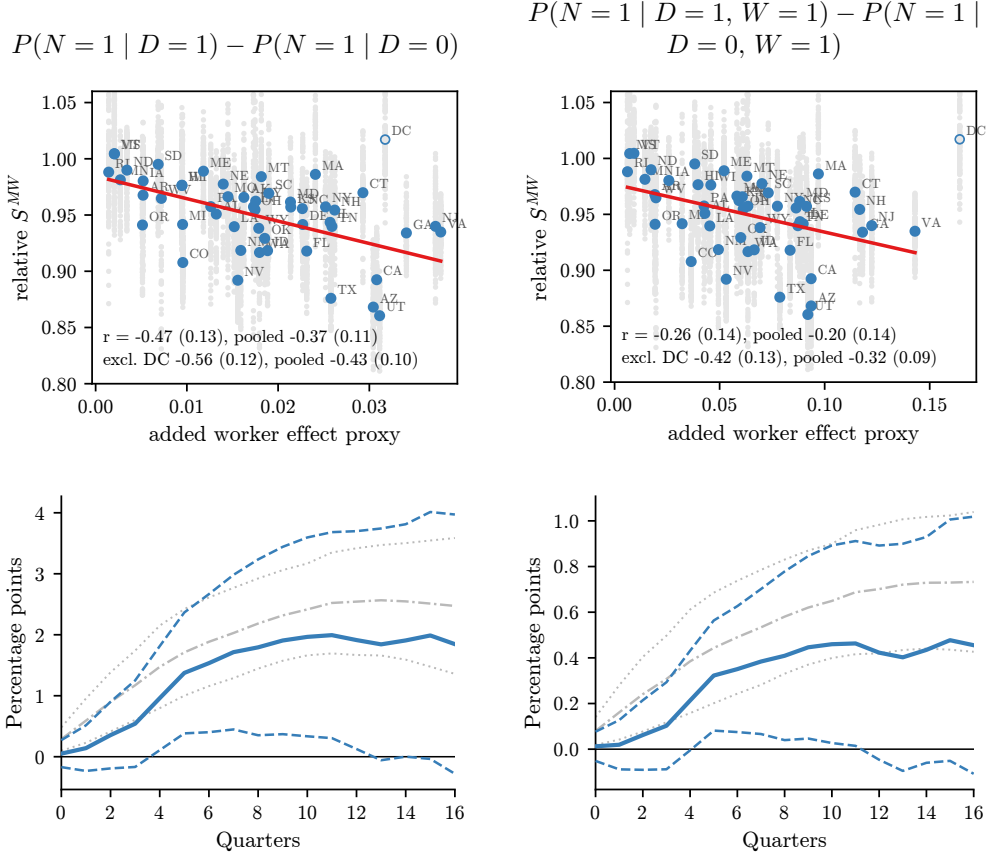


Figure D.5: Marginality of married women

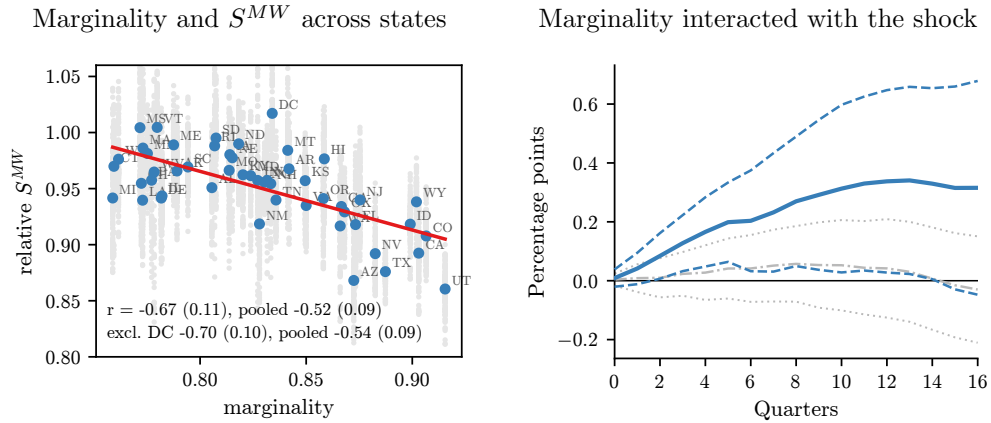
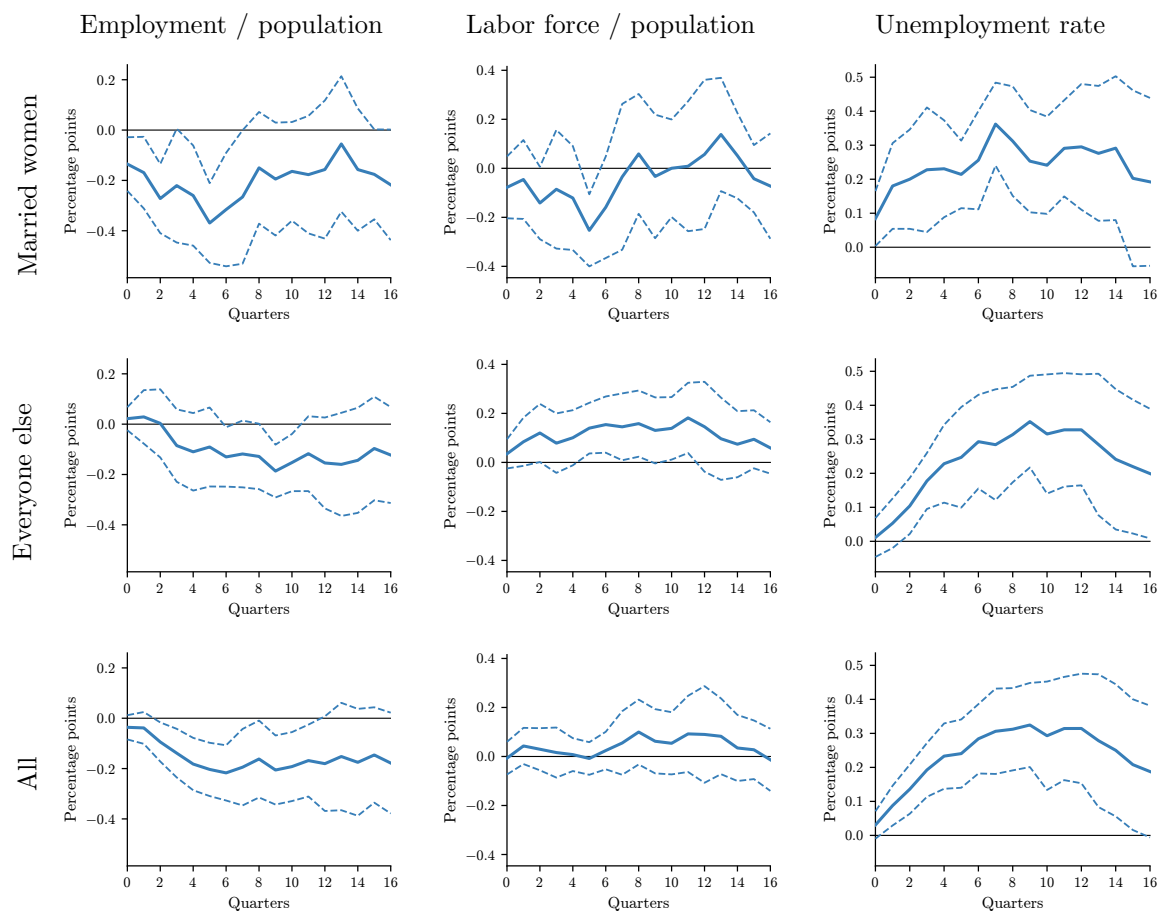
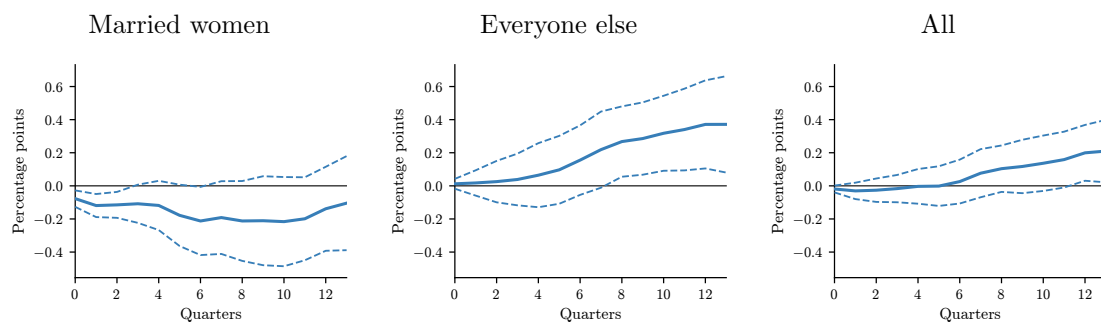


Figure D.6: Employment, labor force participation and unemployment, ages 25–55



Notes: As in Figure 6, 1990Q2–2008Q4.

Figure D.7: Cumulative labor force entry



Notes: As in Figure 7, 1990Q2–2008Q4.