Monetary Policy and Racial Differentials in Labor Market Outcomes

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December 22, 2025

Abstract

This paper examines the empirical relationship between monetary policy and racial and ethnic labor market outcomes. Estimating structural vector autoregressions with external instruments, we find a much larger response of labor market outcome gaps than has been documented previously. These differential outcomes persist for Black workers of both sexes even within the same age group and level of educational attainment, while Hispanic and white responses are similar within such groupings. Estimates based on individual-level data show that distributional differences in demographic characteristics, as well as occupation and industry of employment, account for a large portion of the Hispanic-white unemployment gap response to economic downturns. Most of the response of the Black-white unemployment gap, however, is not explained by differences in demographic observables, occupation, or industry.

Keywords: monetary policy, race, inequality

JEL classifications: E52, J15

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Author contributions: Both authors contributed equally to this work.

Competing interests: The authors declare no competing interests.

Data/code availability: Replication materials are available here.

1. Introduction

Do monetary policy contractions have stronger effects on labor market outcomes for Black and Hispanic workers compared to white workers? Early attempts at answering this question document small but statistically significant disparate effects for Black workers and mixed evidence for Hispanic workers (see Zavodny and Zha, 2000; Thorbecke, 2001; Carpenter and Rodgers, 2004). The issue gained new relevance in 2020 when the Federal Reserve updated its "longer-run goals and monetary policy strategy" to stress policymakers' interpretation of maximum employment as a "broad and inclusive" goal. Vice Chair Lael Brainard and other Fed officials argued that historically disadvantaged groups are often the first to feel the impact of monetary contractions and the last to enjoy the benefit of expansions (Brainard, 2020). Consequently, as President Raphael Bostic of the Federal Reserve Bank of Atlanta put it, the Fed needs to use monetary policy to promote more vigorous growth in order to "bring those on the economy's margins into the mainstream" (Bostic, 2021).

In this paper, we revisit earlier work that assesses the disparate effects of monetary policy on Black and Hispanic workers against white workers. We begin by estimating the effects of monetary policy on labor market outcomes for male and female non-Hispanic white, non-Hispanic Black, and Hispanic workers. Like the earlier research on this topic, we derive our estimates from vector autoregressions (VARs). In addition to taking advantage of around 20 more years of monthly data, we estimate structural VARs with external instruments (SVAR-IV) to identify monetary policy shocks. This methodology produces more plausible estimates of the exogenous component of monetary policy than a Cholesky decomposition with a recursive ordering assumption used in older research. We find that monetary contractions have a much greater effect on racial and ethnic differences in labor market outcomes than has been previously documented. Whereas other authors find larger responses of Black-white labor market outcome gaps than Hispanic-white gaps, we find effects that are statistically significant and comparable in magnitude for both pairs, as well as within male- and female-only groupings.

Next, we examine the extent to which observable demographic factors, such as age and educational attainment, account for the widening of the aggregate gaps we report. We do this in three different ways. First, remaining within the SVAR-IV framework, we analyze the effect of monetary policy shocks on labor market outcome gaps for narrow sex-age-education groupings. We find that much of the response of Hispanic-white labor market gaps disappears when the data is broken down this way, suggesting that differences in the age distribution and levels of educational attainment between the groups account for a large fraction of the aggregate disparate effect of monetary policy. In contrast, certain Black-white labor market outcome gaps continue to widen significantly in response to contractionary monetary policy shocks even when comparing workers within the same age bracket and level of education.

We then use linear probability models (LPMs) on individual-level data from the Current Population Survey (CPS), obtained from IPUMS (Flood et al., 2024), to estimate race-specific marginal effects on the probability of being unemployed following a contraction in economic activity. Using microdata in this way allows us to broaden our list of candidate demographic explanators to include occupation, industry, and marital, veteran, and residential status, while also taking advantage of variation in economic conditions across states. The results generally confirm those from SVAR-IVs. Hispanic-white labor market outcome differentials mostly cease to exist when comparing worker groups of similar age and level of education (and controlling for an expanded set of demographic observables), while such differentials persist in Black-white labor market outcomes, growing with each successively younger age cohort.

Lastly, we use the Oaxaca-Blinder decomposition to quantify the contribution of demographic factors to the movement in racial and ethnic unemployment gaps following economic contractions. We show that compositional differences in demographic observables between racial and ethnic groups help explain large portions of the responses of Hispanic-white unemployment gaps, particularly when comparing male workers, with differences in the distribution of educational attainment being the most important contributor. Although demographic observables also play a significant role in explaining the differential effect on Black-white unemployment gaps, large portions remain unexplained.

In the following section (2), we review the related literature. In Section 3, we discuss our VAR approach and present estimates for the responses of racial and ethnic labor market outcome gaps to monetary policy shocks. In Section 4, we describe our individual-level methodology and report these results. Lastly, in Section 5, we discuss the importance of our findings and conclude.

2. Related Literature

This paper draws on three related streams of research. First, we build on studies from the early 2000s that estimate the effect of monetary policy on racial and ethnic disparities in labor market outcomes. Three of these studies – Zavodny and Zha (2000), Thorbecke (2001), and Carpenter and Rodgers (2004), estimate vector autoregressions using similar samples of monthly U.S. macroeconomic data from the 1970s to the early 2000s. All find small (though significant) effects on Black-white labor market outcome gaps relative to the magnitude of the monetary policy shock. Zavodny and Zha (2000) find that a 43 basis point monetary contraction leads to a 0.05 percentage point increase in the gap between Black and white unemployment; Thorbecke (2001) finds that a 54 basis point increase in the federal funds rate widens the Black-white unemployment rate gap by 0.05 percentage points; and Carpenter and Rodgers (2004) show that a 1.24 percentage point increase in the federal funds rate widens the Black-white unemployment gap by 0.15 percentage points. While the size of the effect on the Black-white gap is similar across the three studies, the evidence presented on Hispanic-white labor market outcome gaps is mixed. Thorbecke (2001) finds that the Hispanic-white unemployment gap responds by about the same amount as the Black-white unemployment gap to a monetary shock, while Carpenter and Rodgers (2004) do not find a statistically significant effect. Zavodny and Zha (2000) do not address the effect of monetary policy on the Hispanic-white unemployment gap.

In the first part of our paper, we extend the work of these researchers in several ways. First, they estimate VARs over a period up to the late 1990s or early 2000s. We update these estimates by including the subsequent two decades of pre-pandemic data. Second, we avoid potential problems of parameter instability posed by the turbulent macroeconomic environment of the 1970s and early 1980s (and the start of the pandemic in 2020) by limiting the sample to encompass data from January 1992 to February 2020. When we break the data down by level of education, limiting the sample in this way also avoids problems involving the Census Bureau's change in the definition of education categories in 1992 (Jaeger, 1997). Most importantly, while the early papers in this literature use the Cholesky decomposition to identify monetary policy shocks, we use the SVAR-IV method popularized by Stock and Watson (2012, 2018), Gertler and Karadi (2015), and others. This approach produces responses to monetary policy shocks that are more in line with conventional macroeconomic theory compared to those produced using the Cholesky decomposition.

The second stream of research relevant to our paper includes recent work that has also used more modern empirical methods to estimate the disparate effects of monetary policy by race and ethnicity. Coibion et al. (2017) estimate the effect of monetary policy shocks on racial income and consumption inequality in a local projections framework, identifying monetary shocks with an exogenous instrument. They find that contractionary monetary policy shocks are associated with a subsequent rise in income and consumption inequality, but they do not estimate the effect on labor market outcomes. Bartscher et al. (2022) follow a similar empirical methodology. Their paper focuses on the effects of monetary policy on racial disparities in household income and wealth, but they also provide an estimate of the effect on the Black-white unemployment rate gap. They find results slightly larger in magnitude than Carpenter and Rodgers (2004) do – a one percentage point monetary expansion reduces the Black-white unemployment rate gap by about 0.18 to 0.34 percentage points, depending on the choice of external instrument.

Several papers focus directly on the disparate effects of monetary policy on labor market outcomes rather than income, consumption, or wealth inequality, tying their work more closely to ours. Rodgers (2008) provides an analysis of the unemployment duration differences between Black and white workers following a monetary contraction, finding that Black workers incur a disproportionate share of both shorter- and longer-term unemployment lengths. Couch and Fairlie (2010) study the claim that Black workers are fired first when the business cycle contracts and hired last when it expands. They find strong evidence for the former claim, but not for the latter. Studying the dynamics of state-specific Black-white population and employment differences, Seguino and Heintz (2012) find that monetary contractions raise the Black-white unemployment rate ratio in males and that this effect is stronger the greater the Black share of the population.

More recently, Bergman et al. (2022) use panel data to estimate the effect of monetary policy on employment growth by sex, race, and level of education, factoring industry variation, and interacting their monetary policy variable with a measure of local labor market tightness. The authors find that expansionary monetary policy has stronger effects on employment growth for Black workers at times when labor markets are tight than it does for white workers. They do not, however, focus on the standalone effect of monetary policy as we do. De et al. (2021) estimate the effect of monetary policy shocks (as well as demand and supply shocks) on labor market differentials using a factor-augmented VAR model with shocks defined via sign restrictions. In addition to using a different identification approach, their model is more restrictive in that they identify not only monetary but nonmonetary demand and supply shocks. The authors find that a 20 basis point increase in the interest rate is associated with a 0.1 percentage point increase in the Black-white unemployment gap. Lastly, Elder and Payne (2023) show that, similarly to a monetary policy shock, oil price uncertainty is another type of macroeconomic shock which disproportionately affects Black and Hispanic unemployment rates.

An important objective of our paper is to examine the role that demographic characteristics at the individual level play in generating the aggregate racial and ethnic differences in the response to monetary policy that we find. A third stream of recent research is relevant to this objective. Three papers estimate the differential effects of monetary policy on different demographic groups without focusing specifically on race and ethnicity. Amir-Ahmadi et al. (2022) estimate the effects of monetary policy on labor outcomes for workers in different educational and marital categories similarly to our disaggregated VARs methodology. They find that monetary policy has particularly large effects on the labor market outcomes of less educated individuals and single men. Dolado et al. (2021) find that expansionary monetary policy shocks increase employment more among high-skilled than less-skilled workers. Zens et al. (2020) estimate heterogeneous effects of monetary policy across 32 occupation categories, finding considerable worsening of labor market conditions in fields such as construction, transportation, and service following a monetary contraction.

One paper that explicitly makes the link between demographic variation and racial disparities is Cajner et al. (2017). The authors break down the average Black-white and Hispanic-white gaps in unemployment rates, labor force participation rates, and employment-population ratios by education, age, marital status, and state of residence. They find that Hispanic-white labor market gaps are driven largely by differences in educational attainment, while these demographic observables explain only a small portion of the Black-white labor market gaps. We extend their work by decomposing the dynamic response of labor market gaps to monetary policy shocks – not just the average gaps – to test whether demographic variables other than race and ethnicity can account for the observed disparate effects of monetary policy shocks.

The papers cited above motivate our selection of demographic indicators to include in individual-level regressions presented in Section 4. We do not include income variables directly due to data availability and the fact that individuals' earnings are likely endogenous with respect to their labor market outcomes. However, occupation, industry, age, and education are likely to be correlated with income. Furthermore, given the evidence that age, education, occupation, and marital status are important drivers of the differences in labor market outcomes, we consider these characteristics as potential determinants of racial and ethnic disparities in response to monetary shocks. We also ask whether industry, veteran status, and urban residence can account for the racial and ethnic disparities we find.

3. Unemployment Gaps and Monetary Shocks

In this section, we update and extend the empirical work of Thorbecke (2001), Carpenter and Rodgers (2004), and others that estimate the impact of monetary policy shocks on racial and ethnic labor market outcome gaps using VARs. First, we set up and estimate a baseline SVAR-IV which delivers plausible macroeconomic responses to a contractionary monetary policy shock. Then we augment the model to estimate the effects of monetary policy shocks on racial and ethnic unemployment gaps. We focus on unemployment rate gaps and report analogous results for employment-population ratio gaps in Appendix A.

3.1. The Baseline SVAR-IV

Our baseline model is a vector autoregression (VAR) of the form

$$Y_t = A(L)Y_{t-1} + u_t \tag{1}$$

where Y_t is a k-dimensional vector of macroeconomic and financial variables, $A(L)$ is a matrix polynomial of order p in the lag operator, and u_t is a vector of residuals with a positive semi-definite variance-covariance matrix Σ. The residuals from the reduced form VAR in Equation 1 are related to structural shocks by

$$u_t = \Theta v_t \tag{2}$$

where Θ is a nonsingular $k \times k$ matrix and the variance-covariance matrix of v_t is diagonal. Invertibility of $A(L)$ implies that the impulse response function (IRF) relating the endogenous variables to realizations of the structural shocks is

$$Y_t = C(L)\Theta v_t \tag{3}$$

where $C(L) = A(L)^{-1}$.

Let the first variable $Y_{1,t}$ in the VAR be a monetary policy variable and $u_{1,t}$ be the corresponding reduced-form residuals. Identification of the monetary shock is accomplished by finding an "external instrument" for the monetary policy variable. Since we are only interested in the effect of monetary shocks, we need only identify the first column of θ. Let Z_t be an instrumental variable that satisfies the standard conditions for relevance and exogeneity. Then identification is achieved by running two-stage least squares (2SLS) regressions of each residual in u_{it} on $u_{1,t}$ using Z_t as an instrument. This normalizes the response of the monetary policy variable to the instrument to equal one, while the second through k-th elements of the first column of Θ are the contemporaneous effects of the shock on all the other variables in Y_t. In our figures, we scale the responses to correspond to a 25 basis point increase in the monetary policy rate.

We estimate the model on monthly data for the U.S. from January 1992 to February 2020. We choose 1992 as the start date for two reasons. The first is to avoid the possibility of instability in the coefficient estimates due to the disinflation of the 1980s and accompanying monetary policy framework revisions. The second reason is that the Census Bureau changed its definitions of educational attainment in 1992 (Jaeger, 1997), which becomes relevant when we break the data down by level of education in the following subsections. We end the sample just before the onset of the COVID-19 pandemic to avoid potential skewing of estimates due to the presence of extreme values for variables such as GDP and unemployment. We set the lag length in the VAR to 12 months.

Consistent with recent applications of the SVAR-IV model to the estimation of the effects of monetary policy shocks (see e.g., Gertler and Karadi, 2015; Jarociński and Karadi, 2020; Miranda-Agrippino and Ricco, 2021, 2023; Bauer and Swanson, 2023), we include five macroeconomic and financial variables in the VAR and one proxy for monetary policy in the subsequent 2SLS procedure. The macroeconomic variables are: a monthly measure of real GDP from Brave et al. (2019); the civilian unemployment rate from the Bureau of Labor Statistics (BLS); the Consumer Price index (CPI), also from the BLS; the Commodity Research Bureau's commodity spot price index; and the Gilchrist and Zakrajšek (2012) excess bond premium. GDP, CPI, and the commodity price index are in logs.

Our monetary policy variable is the Wu and Xia (2016) shadow federal funds rate. The Wu-Xia rate is derived from the shadow rate term structure model of Black (1995), incorporating information from forward rates on U.S. Treasury securities. It provides a measure of the Federal Reserve's monetary policy stance that tracks the federal funds rate for most of our sample, but provides more information about the monetary policy stance during the zero lower bound period of 2008-15 by translating unconventional monetary policy actions into equivalent movements of the federal funds rate.

Our external instrument is the change in the three-month federal funds futures rate in a 30-minute window around Federal Open Market Committee (FOMC) announcements used in Jarociński and Karadi (2020), which is an updated version of the data in Gürkaynak et al. (2005). Such monetary policy "surprises" likely represent a pure financial market response to Federal Reserve actions. They are unlikely to be attributed to changes in publicly available information on the economy given the short window used.

Figure 1 shows the IRFs that correspond to a 25 basis point increase in the Wu-Xia rate from our baseline SVAR-IV. Lightly shaded areas are 90 percent confidence bands around the point estimates; dark shaded areas are 68 percent confidence bands. Confidence bands are constructed using methods in Montiel Olea et al. (2021). They are robust to the presence of weak instrument, which the authors argue is a common problem in the SVAR-IV literature. We use these confidence bands as a precaution because the F statistics from our first-stage regressions are close to the cutoff of 10, traditionally used to identify weak instruments.

The figure shows that following a 25 basis point increase in the Wu-Xia rate, GDP falls and unemployment rises immediately, with the effects building up over time and exhibiting a strong degree of persistence. GDP falls by as much as 0.45 percent while the unemployment rate peaks at around 0.2 percentage points above its initial level. Consumer prices fall immediately as well, by as much as 0.2 percent within the first six months, with persistent deflationary effects throughout the forecast horizon. The excess bond premium rises by about 0.1 percentage points on impact, and commodity prices fall insignificantly. In theory, commodity prices fall with economic contraction and rise with lower interest rates. The response suggests that the former effect dominates in the first year of the monetary contraction and the latter effect dominates after that. Overall, the responses are plausible and in line with other estimates of the impact of monetary policy shocks such as Bauer and Swanson's (2023) "best practice" VAR model.

3.2. Robustness Tests

Specification of VARs inevitably involves contestable decisions about variable selection and econometric techniques. In this subsection, we test whether our baseline estimates are materially affected by various specification changes to the model. The alternative results are documented in the appendices referenced below.

Responses are unchanged if we use Bayesian methods as in Giannone et al. (2015) and Miranda-Agrippino and Ricco (2021, 2023) instead of the frequentist methods discussed in the previous subsection (3.1). The point estimates and confidence bands from Bayesian estimation are essentially indistinguishable from those in the previous subsection (3.1). We used the frequentist methods because of the similarity to the Bayesian estimates and due to the lack, to our knowledge, of weak-instrument-robust confidence bands for Bayesian estimates (see Appendix B).

Use of external instruments generally resolves the "price puzzle" and other issues that originally motivated the inclusion of variables like commodity prices and credit spreads in VARs (see discussion in Sims, 1992; Ramey, 2016; Caldara and Herbst, 2019). Appendix C reports IRFs from the baseline SVAR-IV omitting one or both of these variables. IRFs are very similar whether we include or exclude commodity prices. First-stage F statistics are lower when commodity prices are omitted, however, increasing the risk of instrument irrelevance. Excluding the excess bond premium does have a substantial effect on the IRFs. In the model that excludes the excess bond premium, monetary shocks are more persistent, yet have a much smaller effect on real GDP, the unemployment rate, and prices. This model also produces a counterintuitive response of prices, which fall in the first six months following the shock, but rise substantially at a horizon of 12 to 36 months. The excess bond premium seems to represent an important channel of transmission of monetary policy shocks, which merits keeping it in the baseline model.

Appendix C also shows that reducing the lag length to six or nine months has little effect on the IRFs, though, we stick to the literature standard of imposing 12 lags when dealing with monthly data.

The industrial production index is another commonly used measure of output when VARs are estimated on monthly data. Appendix D shows that while the response of output to monetary policy shocks is considerably different when industrial production is used instead of the Brave et al. (2019) monthly GDP series, other responses are very similar. Since industrial production is a small and declining fraction of total output, we argue that the monthly GDP measure is a better proxy for output.

Gertler and Karadi (2015), Bauer and Swanson (2023), and others address the zero-bound problem differently than we do by using one- or two-year Treasury yields as monetary policy variables instead of the Wu-Xia rate used by us. Appendix E shows that the estimated IRFs are virtually the same when the one-year Treasury yield is used instead of the Wu-Xia rate. By contrast, using the two-year Treasury yield produces much different and implausible IRFs, indicative of instability in the estimated VAR coefficients. In addition, each of these measures produce a weaker identification of monetary policy shocks than our baseline model judging from first-stage F statistics.

Several other measures have been proposed as external instruments, including those in Miranda-Agrippino and Ricco (2021), and Bauer and Swanson (2023). Bauer and Swanson propose two measures – an unorthogonalized instrument series and a series orthogonalized with respect to predating macroeconomic and financial data. Appendix F compares our estimates of the IRFs from the baseline model using the Jarociński-Karadi instrument and these alternatives. The IRFs shown in Figure F.1 generally coincide, but the orthogonalized Bauer-Swanson instrument shows a much larger response of unemployment, output, and prices. Only the Jarociński-Karadi series, however, produces an F statistic (14.91) safely above the threshold of 10 for instrument irrelevance. The Jarociński-Karadi measure has the additional virtue of being available for almost our entire sample, whereas the Miranda-Agrippino-Ricco instrument is only available from 1991 to 2015. The Bauer-Swanson data we compare with are also available until 2019, although the authors do not make their preferred measure publicly available.

To further compare our instrument choice with the others, we estimate the VAR using Bauer and Swanson's (2023) data. The instruments produce widely varying IRFs, reported in Figure F.2. Again, the orthogonalized Bauer-Swanson instrument produces results that are considerably larger than the other instruments. The Jarociński-Karadi instrument produces IRFs that track the IRFs for the other instruments closely, while producing a better fit in the first-stage regression.

Finally, Figures F.1 and F.2 show the IRFs from a Cholesky decomposition with the monetary policy variable entered first (most exogenous). These IRFs generally produce implausible estimates of the effect of monetary policy shocks. Overall, we find that the Jarociński-Karadi series is the strongest of the alternatives in terms of instrument relevance, while producing plausible IRFs.

One possible drawback to using the Jarociński-Karadi instrument is that it does not filter out "information effects." That is, an increase in the federal funds futures rate following an FOMC decision could be evidence of a monetary policy decision that is more contractionary than expected, which should increase the interest rate while depressing asset prices. Alternatively, the rise in fed funds futures could be due to information revealed in the Fed's decision that suggests a stronger-than-anticipated economy. This information effect would normally be followed by an increase in both interest rate futures and asset prices. Jarociński and Karadi (2020) account for this by factoring in changes in stock prices around FOMC announcements together with federal funds futures rate changes. We do not replicate their main procedure imposing sign restrictions, but we estimate our model using their "poor man's" shock series, taking a value of zero whenever an FOMC announcement produces changes in the fed funds futures rate and stock prices in the same direction, indicative of information effects (see Figure 4). The figure reports IRFs from our baseline model including racial and ethnic unemployment gaps. These comparisons speak directly to the possibility that our main results concerning the effect of monetary policy on racial and ethnic unemployment gaps are due to not controlling for information effects. The figure shows that the responses for all six aggregate and sex-specific Black-white and Hispanic-white unemployment gaps to monetary policy shocks are even stronger using the "poor man's" version of the instrument than using the instrument drawn just from the federal funds futures. The "poor man's" instrument, however, produces low first-stage F statistics, indicating that it is a much weaker instrument than the unadjusted surprise series.

In summary, the Jarociński-Karadi instrument without correction for information effects is a stronger instrument than the alternatives and gives us plausible IRFs to monetary policy shocks. Thus, our baseline model provides a reasonable framework that can serve as the core of a model to estimate the response of racial and ethnic unemployment rate gaps to monetary policy shocks.

3.3. Aggregate Unemployment Gaps

Our objective is to estimate the differential effect of monetary shocks between racial and ethnic groups and to understand the role that demographic variables play in generating these differential responses. To this end, we augment the baseline VAR estimated above with an "unemployment gap" variable representing the difference between unemployment rates for Black or Hispanic and white individuals.

Our data source is the monthly Current Population Survey from January 1992 to February 2020. Using individual data with person-level weights, we construct separate unemployment rates by race/ethnicity (non-Hispanic white; non-Hispanic Black; Hispanic), sex (male; female), age group (16-24; 25-44; 45-64; 65+), and level of education (less than a high school diploma; high school diploma only; some college or associate's degree; bachelor's degree or higher). This results in unemployment rates for 96 distinct demographic populations. We also construct unemployment rates for aggregations of these groups, for example, all white female workers, all Black college graduates. We seasonally adjust all unemployment rates using the X-13 ARIMA-SEATS software provided by the Census Bureau.

Such a fine division results in a relatively small number of observations for particular demographic groups and correspondingly large standard errors. Appendix G reports the average sample size per month for each demographic group, along with the minimum and maximum size. Most demographic groups have large enough sample sizes that the unemployment rates can be estimated precisely. But some groups have quite small sample sizes – for example, we have an average of 60 (unweighted) observations per month for Hispanic females over the age of 65 and only 11 in one of the months. Our results for these demographic groups should therefore be taken with a grain of salt.

We define the Black-white unemployment gap for a particular demographic group as the difference between unemployment rates for non-Hispanic Black and non-Hispanic white individuals in that group. Likewise, the Hispanic-white unemployment gap is the difference between unemployment rates for Hispanic and non-Hispanic white individuals in a particular demographic group. Figure 2 shows the aggregate and sex-specific Black-white and Hispanic-white unemployment gaps over our sample period. The unemployment gap for both groups is positive, meaning that the Black and Hispanic unemployment rates are uniformly higher than those for white workers. Interestingly, while the Black-white unemployment gap is larger for males than for females, the reverse is true for the Hispanic-white gap. Close inspection of the figure shows that the gap is cyclical: for both sexes and both racial/ethnic pairings, the unemployment gap widens around recessions and falls during expansions.

We begin by estimating the effect of monetary policy shocks on Black-white and Hispanic-white unemployment gaps for all individuals and for males and females separately. We do this by appending these gap variables to the baseline SVAR-IV one at a time. Figure 3 shows the response to a 25 basis point contractionary monetary policy shock. The responses of the remaining variables in the VAR, which are not shown, resemble those in Figure 1. Positive values of point estimates indicate a widening of the racial/ethnic unemployment gap, i.e. an increase in Black or Hispanic unemployment relative to white unemployment. The shaded areas represent the 68 and 90 percent confidence bands around the point estimates. Portions of IRFs in red are estimates that are statistically significant at the 10 percent level in the direction signifying worsening of labor market conditions for Black or Hispanic workers relative to white workers.

The aggregate and sex-specific Black-white unemployment gaps respond similarly to monetary shocks. There is little impact in the first year and a half following the shock, but after that the response becomes positive and significant. The aggregate Black-white unemployment gap reaches a peak of about 0.2 percentage points. The maximum response for males is stronger (0.26 percentage points) than that for females (0.16 percentage points). The aggregate and sex-specific Hispanic-white gaps increase more quickly but with a smaller magnitude, peaking at about 0.13 percentage points. The magnitude of the gap response is similar for both sexes. In all cases, the response is very persistent.

Our estimates of the effect of monetary policy shocks on racial and ethnic unemployment gaps are considerably larger than those documented previously. Compared to the older literature, our estimate of the peak response of the Black-white unemployment gap is 8.6 times larger, and the response of the Hispanic-white gap is 7.2 times larger than for respective gap increases reported in Thorbecke (2001). Our estimate of the peak response of the Black-white gap is 6.6 times larger than in Carpenter and Rodgers (2004). We find a response of the Hispanic-white gap that is slightly smaller than of the Black-white gap, whereas Carpenter and Rodgers did not find a significant response for the Hispanic-white gap.

To put these magnitudes in perspective, note that the average Black-white unemployment gap in our sample is 5.9 percent and the average Hispanic-white gap is 3.2 percent. In Figure 3, the average increase in the unemployment gap over the statistically significant range is approximately 0.17 percentage points for the Black-white gap and 0.12 percentage points for the Hispanic-white gap. In the last four significant tightening episodes (1993-1995, 1999-2000, 2004-2006, 2015-2019, and 2022-2023), the Fed has increased the federal funds rate cumulatively by an average of about 3.3 percentage points. Abstracting from the dynamics of sequential rate increases, a 3.3 percentage point tightening episode would be associated with an increase in the Black-white unemployment gap of about 2.2 percentage points and 1.6 percentage points in the Hispanic-white gap. This implies a potential increase in both aggregate unemployment gaps equal to about half of their respective mean sample values.

The main reason for the larger estimated effect of monetary policy on unemployment gaps in our paper is the better identification of monetary shocks achieved by the SVAR-IV approach. Figure 4 compares our estimates of the response to monetary policy shocks against those implied by the Cholesky decomposition method using the same data and sample period. When shocks are identified using the Cholesky decomposition, the point estimate of the response of the Black-white unemployment gap overall and for males is barely above zero at the 24-month horizon. For female workers, the point estimate is below zero. The response of the Hispanic-white unemployment gap is a bit larger but would not be statistically different from zero at conventional significance levels. The response is even larger when we control for information effects using the Jarociński and Karadi (2020) "poor man's" instrument, although as discussed above, we are skeptical of the instrument validity of this measure due to the low first-stage F statistics.[1] Our results are consistent with other papers that use external instruments to identify monetary policy shocks, including Bartscher et al. (2022) and De et al. (2021), who also find a larger response of labor market gaps to monetary policy shocks than the earlier literature reports.

3.4. Disaggregated Unemployment Gaps

Cajner et al. (2017) find that differences in Hispanic and white unemployment rates, employment-population ratios, and labor force participation rates are largely explained by differences in the distribution of educational attainment between the two groups, while differences in Black-white labor market outcomes are largely not explained by such demographic observables. We ask a similar question about the response of unemployment gaps to monetary policy shocks: Would the differences we observe in the responses of unemployment across racial and ethnic groups to monetary policy shocks persist if we take account of age and educational attainment?

We first examine the effect of age by computing Black-white and Hispanic-white unemployment gaps (separately for males and females) by four age brackets: 16-24, 25-44, 45-64, and 65+. Figure 5 shows the Black-white unemployment gaps responses, and Figure 6 shows the Hispanic-white unemployment gaps responses. Generally, we find that the responses of the three lower age groups are similar to the aggregate ones. In the statistically significant range, they range from 0.08 to 0.30 percentage points for Black-white gaps and from 0.11 to 0.22 percentage points for Hispanic-white gaps. The unemployment gaps for workers 65 years or older are mostly insignificant and less persistent. For workers under the age of 65, then, age

[1] The range of first-stage F statistics (heteroskedasticity-consistent versions in brackets) associated with the Jarociński-Karadi "poor man's" instrument is 5.23-6.39 (5.68-7.15) and 12.71-15.49 (7.48-8.51) for the unadjusted Jarociński-Karadi instrument.

does not appear to account for the differential response of unemployment to monetary shocks.

Next, we disaggregate workers by level of educational attainment. We focus on the 25-44 and 45-64 age brackets given the lack of differential in the responses for the 65+ age bracket and the ambiguous interpretation of education-level groupings for school-aged workers. Figures 7 and 8 show the results for the 25-44 age bracket, and Figures 9 and 10, for the 45-64 age bracket. We consider four levels of educational attainment: less than a high school diploma, high school diploma only, some college or associate's degree, and bachelor's degree or higher.

Figure 8 shows that there is no significant movement in the Hispanic-white unemployment gaps for the 25-44 age group within each education category, for male and female workers alike. This suggests that the disparate effect of monetary contractions on Hispanic versus white males and females may be due to the different distribution of educational attainment for these workers. That is, if younger or less educated workers are generally more likely to lose their jobs following a monetary policy contraction than older or highly educated workers, this could explain the larger response of unemployment to worsening labor market conditions for Hispanic workers. This is in line with the finding from Cajner et al. (2017) on the level of the Hispanic-white unemployment rate gap. Figure 10 shows similar results for the 45-64 age group, although there is some evidence of an increase in some Hispanic-white gaps (e.g., male workers with a high school diploma or some college or associate's degree).

In contrast, Figure 7 shows that the Black-white unemployment gaps for males and females of ages between 25 and 44 respond similarly for each education-level grouping to the aggregate Black-white gap. The main exception is for female workers without a high school degree. The responses of Black-white gaps for the 45-64 age group (see Figure 9) appear similar to the aggregate response, but are rarely statistically significant. These results suggest that the disparate effect of monetary policy between Black and white unemployment does not disappear when comparing workers of similar age and level of education, especially for the 25-44 age cohort. This is again consistent with the findings of Cajner et al. (2017).

4. Evidence from Individual-Level Regressions

In this section, we use person-weighted individual-level data from the CPS to further examine the role that demographic variables play in driving the differential effects of monetary policy on unemployment across race and ethnicity. In Subsection 4.1, we use a linear probability model (LPM) to estimate the differential effect of an economic contraction (measured as an unemployment rate increase at the state level) on the probability that a person is unemployed, controlling for a number of demographic observables. We derive our estimates via weighted least squares instead of logit or probit due to computational constraints. Validation of the LPM approach is provided in Appendix H, which shows that across different specifications, 90 percent or more of the fitted values from the weighted LPM regressions fall within the expected unit interval. In Subsection 4.2, we use a similar setup to estimate the contribution of observable demographic characteristics to the differential effects of an economic contraction using the Oaxaca-Blinder decomposition.

Our model relies on the plausible assumption that monetary policy actions affect each person's unemployment likelihood solely through the labor market conditions in their state of residence. A drawback to this approach, however, is that we do not distinguish between the effects of economic contractions solely due to monetary policy versus other causes. Using a measure of monetary policy in place of the state unemployment rate would isolate the impact of monetary policy and would also tie the individual-level regressions more closely to the VARs in the previous section (3). On the other hand, using a single measure of monetary policy that is (necessarily) the same across states throws out information about the geographic variation in the effects of monetary policy. We experimented with using the monetary policy stance and external shock series from the previous section (3) either directly or as instruments for the state unemployment rates in our regressions, but these regressions did not produce sensible results.

4.1. Differentials in the Effect of an Economic Contraction on the Probability of Unemployment

Our data is monthly CPS data from January 1992 to February 2020 for the civilian noninstitutionalized population 16 years of age or older, excluding those not in the labor force. We estimate our models separately for 48 sex (male; female), age (16-24; 25-34; 35-44; 45-54; 55-64; 65+) and education level (less than a high school diploma; high school diploma only; some college or associate's degree; bachelor's degree or higher) bins. For each bin, we estimate the model on one sample that includes only Black and white individuals and another that includes only Hispanic and white individuals. The dependent variable $u_{i,s,t}$ is an indicator variable equal to one if the individual is unemployed and zero otherwise. For each sample, we estimate the unemployment probability for an individual i residing in state s in period t using the following linear probability model:

$$
\begin{aligned}
P(u_{i,s,t} = 1 | \overline{U}_{s,t}, X_{i,s,t}) = \alpha_0 &+ \alpha_1 \cdot Black|Hispanic_i + \alpha_2 \cdot \overline{U}_{s,t} \\
&+ \alpha_3 \cdot Black|Hispanic_i \cdot \overline{U}_{s,t} \\
&+ A_1 \cdot X_{i,s,t} + A_2 \cdot Black|Hispanic_i \cdot X_{i,s,t} + \varepsilon_{i,s,t}
\end{aligned} \tag{4}
$$

The macroeconomic variable, $\overline{U}_{s,t}$, is the 12-month average (lags 1 through 12) of the aggregate state-level unemployment rate. $X_{i,s,t}$ is a vector of control variables that includes occupation and industry groupings, marital status (one if married and spouse is present; zero otherwise), veteran status (one if a veteran; zero otherwise), and central city residence (one if living in a central city area; zero otherwise), as well as month- and state-specific indicators to respectively adjust for seasonal and geographic variation.[2][3] Both $\overline{U}_{s,t}$ and all the variables in $X_{i,s,t}$ are also interacted with being Black or Hispanic.

Figure 11 plots the α_3 coefficients – estimates of the interaction term between being Black or Hispanic and $\overline{U}_{s,t}$, along with 99 percent heteroskedasticity-consistent confidence intervals – for each sex-age-education grouping. These estimates represent the racial/ethnic differential in the marginal effect of a one percentage point increase in $\overline{U}_{s,t}$ on the probability of being unemployed. The corresponding numerical estimates are presented in Appendix I.

The upper left graph shows the response of the probability of unemployment for Black versus white male workers by age and education after controlling for occupation, industry, and the other demographic variables. A one percentage point increase in a state's unemployment rate increases the probability of unemployment for Black workers more than for white workers for all age-education groupings with only a few exceptions. For example, the left-most bar shows that a one percentage point increase in the unemployment rate increases the probability of a 16-24 Black male worker without a high school diploma by 0.47 percentage points more than a white male worker in the same grouping. The estimated difference in the marginal effect of

[2] We refer to the major occupation and industry categories suggested by IPUMS. Our occupation indicators are "technical, sales, and administrative support", "service", "farming, forestry, and fishing", "precision production, craft, and repair", "operators, fabricators, and laborers" ("managerial and professional specialty" used as reference). Our industry indicators are "agriculture, forestry, and fisheries", "mining", "construction", "manufacturing", "transportation, communications, and other public utilities", "wholesale trade", "retail trade", "finance, insurance, and real estate", "business and repair services", "personal services", "entertainment and recreation services", "professional and related services", and "public administration." Note that about 8 percent of surveyed unemployed individuals are first-time civilian job seekers and do not have a record of occupation or industry. These observations are also used as a reference category for the occupation and industry indicators in our regression analysis.

[3] Inclusion of these variables is prudent since they are correlated with labor market outcomes and race/ethnicity. For example, the unemployment rate for veterans is slightly lower than for nonveterans, while white males are more likely to be veterans and Hispanic males – less likely.

unemployment is statistically significant at the one percent significance level. The estimated differentials are somewhat larger for less educated male workers in the 25-34 and 35-44 age brackets, but otherwise the differentials are around half a percentage point. Estimates are also statistically significant for all other age-education groupings, except for 65 and older workers without a high school diploma.

We find similar results for Black versus white females in the graph on the bottom left. Black female unemployment rises more than white unemployment after an increase in the state unemployment rate for all but the oldest age group. The estimated differential effects are highest for female workers in the first two age brackets with some college education and the 35-44 age bracket without a high school diploma. Otherwise, the estimates are fairly uniform and similar to those of Black male workers for the first four age groups. Estimates are smaller and in three of four cases statistically insignificant for the 55-64 age bracket and statistically indistinguishable from zero in the 65+ age bracket.

The results are considerably different when comparing Hispanic and white workers, as shown in the two graphs to the right in Figure 11. Here, the differential effect of an economic contraction on the probability of unemployment is considerably smaller for both males and females than it is for Black versus white workers. In many cases, we estimate a near-zero and, in a few cases, even a negative differential. Most of the estimates are not statistically significant at the one percent significance level. Results are similar (not presented here) when we do not include occupation and industry controls, indicating that age and education-level segmentation adequately addresses the Hispanic-white unemployment differentials observed in aggregation.

These results are broadly consistent with those in the VAR section (3). Estimated differential responses of unemployment to economic contractions between Hispanic and white workers, which are present in aggregated data analysis, largely (but not completely) disappear when the population is broken down by age and educational attainment, even without factoring in occupation and industry, as VAR results suggest. These same differentials persist between Black and white workers even though we control for a wider array of observables.

4.2. Decomposing Unemployment Gaps

In this subsection, we again estimate LPMs, this time to decompose the differential change in racial and ethnic unemployment probability gaps following an economic contraction. We utilize the Oaxaca-Blinder decomposition to split these estimated effects into components that are "explained" and "unexplained" by relevant demographic observables. This approach is motivated by the work of Cajner et al. (2017), although, rather than focusing on decomposing macroeconomic labor market gaps, we focus on decomposing the differential effect of an economic contraction on unemployment gaps estimated at the individual level.

We use the same time frame as before and estimate separate models for each race/ethnicity and sex combination for noninstitutionalized civilian individuals in the labor force using the regression equation:

$$\begin{aligned} P(u_{i,s,t} = 1 | \overline{U}_{s,t}, X_{i,s,t}) = \beta_0 + \beta_1 \cdot \overline{U}_{s,t} + B_1 \cdot X_{i,s,t} \\ + B_2 \cdot \overline{U}_{s,t} \cdot X_{i,s,t} + B_3 \cdot F_{s,t} + \omega_{i,s,t} \end{aligned} \tag{5}$$

where $\overline{U}_{s,t}$ is defined as above and $X_{i,s,t}$ contains all the variables that we assume contribute to the existence of gaps in employment outcomes. This includes the age and education-level groupings used in the previous subsection (4.1), as well as occupation and industry controls, marital and veteran status, and central city residence. $F_{s,t}$ represents month and state fixed effects. These LPMs provide us with estimates of the marginal effect of a one percentage point increase in $\overline{U}_{s,t}$ for each of the six groups.

The estimated effect of a one percentage point increase in the unemployment rate on an individual's probability of being unemployed is $\hat{\beta}_1 + \hat{B}_2 \cdot X_{i,s,t}$. It follows that the effect of an increase in unemployment on the percentage of unemployed in a particular sex-race grouping, calculated across all states and periods, is $\hat{\beta}_1 + \hat{B}_2 \cdot \bar{X}$, where $\bar{X}$ is the person-weighted mean of the demographic indicators for that population. The Oaxaca-Blinder decomposition breaks down the difference between these estimated marginal effects on unemployment outcomes for Black or Hispanic (k) versus white (j) individuals by:

$$\hat{u}^{k-j} = \{(\bar{X}^k - \bar{X}^j) \cdot \hat{B}_2^j\} + \{(\hat{\beta}_1^k - \hat{\beta}_1^j) + (\hat{B}_2^k - \hat{B}_2^j) \cdot \bar{X}^k\} \tag{6}$$

The first term in brackets on the right-hand side is the estimated difference in the response of unemployment arising from different demographic proportions, assuming the effect of demographics is given by the coefficients for white individuals. This term is commonly referred to as the "explained" portion. The second term in brackets is the "unexplained" portion, i.e., the estimated difference in the response of unemployment that is due to differences in the response coefficients.

Table 1 presents the results of the decomposition. Following a one percentage point increase in the state unemployment rate, the probability of unemployment rises by 0.8 percentage points more for Black males than for white males, 0.59 percentage points more for Black females than for white females, 0.29 percentage points more for Hispanic males than for white males, and 0.35 percentage points more for Hispanic females than for white females.

For both male and female Black-white gaps, distributional differences in education account for between 6 and 7 percent of the gap. This is driven by the smaller proportion of Black workers with at least a college degree, contributing over 5 percent of the gap for each sex. Furthermore, marriage rates are lower for both male and female Black individuals than for white individuals, and this difference accounts for 10.3 percent of the differential response to unemployment for Black male workers and 8.8 percent for Black female workers. Differences in the distributions across occupations account for 5.7 percent of the gap for males and 4.3 percent for females. The "operators, fabricators, and laborers" occupation category is an important indicator. The higher proportion of Black workers in this occupation group accounts for 7.4 percent of the male gap and 2.7 percent of the female gap. In contrast, distributions across industries tend to reduce the differential response to changes in unemployment. In particular, the lower proportion of Black workers in the construction industry (an especially cyclically sensitive industry) insulates Black workers from worsening labor market conditions relative to white workers.

The most important result from the decompositions of Black-white unemployment gaps is that 84.2 percent of the male and 83.6 percent of the female gaps are not explained by the demographic indicators in our regressions. This is consistent with what Cajner et al. (2017) found for the average unemployment gaps. We can think of at least three broad possible explanations for this finding. One is that we do not account for unmeasured variations within demographic or occupation/industry groupings. For example, these unexplained factors could include differences in access to and quality of education or in occupation below the broad categories we have measured. Another possibility is that we have not explicitly accounted for years of work experience for which age is only a rough proxy. Finally, the differences may reflect various types of racial discrimination in the labor force.

The decomposition results for the Hispanic-white unemployment gaps are quite different. Here, our demographic, occupation, and industry controls account for almost half (48.4 percent) of the female gap, while "overexplaining" the male gap. Distributional differences in educational attainment account for the largest fraction in each gap – 56 percent for male workers and 24.8 percent for female workers. Differences in occupation explain 28.3 percent of the male gap and 11.5 percent of the female gap. The largest single occupation contributor for both sexes is “operators, fabricators, and laborers”, explaining 18.1 percent of the male gap and 7.3 percent of the female gap. Distributional differences by industry account for 28.3 percent of the male gap but only 5.2 percent of the female gap. Within industry, the large proportion of male workers in construction adds 41.4 percent to the gap. Finally, lower marriage rates contribute 12.7 percent of male and 5.9 percent of female gaps.

In summary, the results of the decomposition offer distinct insights. First, the magnitude of unemployment gaps with white workers is largest for Black male workers, followed by Black female workers, Hispanic female workers, and finally, Hispanic male workers. Second, differences in the distribution of educational attainment, marriage rates, some occupations, and industries appear to be the greatest contributors to the differential effect of economic contractions on Hispanic-white unemployment, helping explain the entire male gap and more than half of the female gap. On the other hand, the Black-white male and female unemployment gaps are only somewhat explained primarily by distributional differences in the highest level of educational attainment and lower marriage rates. For both males and females, the vast portion of the response of the Black-white gap in unemployment to economic contractions remains unexplained even when accounting for an extensive array of observables. Thus, the main finding by Cajner et al. (2017) that demographic factors can explain much of Hispanic-white unemployment gaps but little of Black-white unemployment gaps carries over to the marginal effect of economic contractions on those gaps.

5. Conclusion

In this paper, we provide updated estimates of the effect of monetary policy shocks on racial and ethnic gaps in labor market outcomes using recent data and improved monetary policy identification techniques. We find much larger responses of racial and ethnic labor market outcome gaps than has been documented in the previous literature. For example, the effect of a monetary contraction on the Black-white unemployment rate gap is 6.6 to 8.6 times larger than that reported in older studies. Furthermore, we show that differential unemployment responses persist for Black males and females even when compared to white males and females of a similar age and education level, while Hispanic and white unemployment responses are comparable within age-education groupings. Differences in educational attainment and marital status can explain only a portion of the differential response of Black-white unemployment to economic contractions, but a large portion of the gap is not accounted for by a variety of demographic observables.

Our results suggest that monetary policy contractions may have a substantially disparate effect on Black and Hispanic workers, creating tension between the Fed's goals of price stability and maximum employment that is "broad and inclusive." For Hispanic-versus-white workers, these differential labor market outcomes largely reflect the existence of distributional differences in relevant demographic factors, whereas demographics play a small role in driving the differential outcomes we document for Black-versus-white workers. Understanding the role that demographics play can help inform the policy response to racial and ethnic differentials in the effects of monetary policy.

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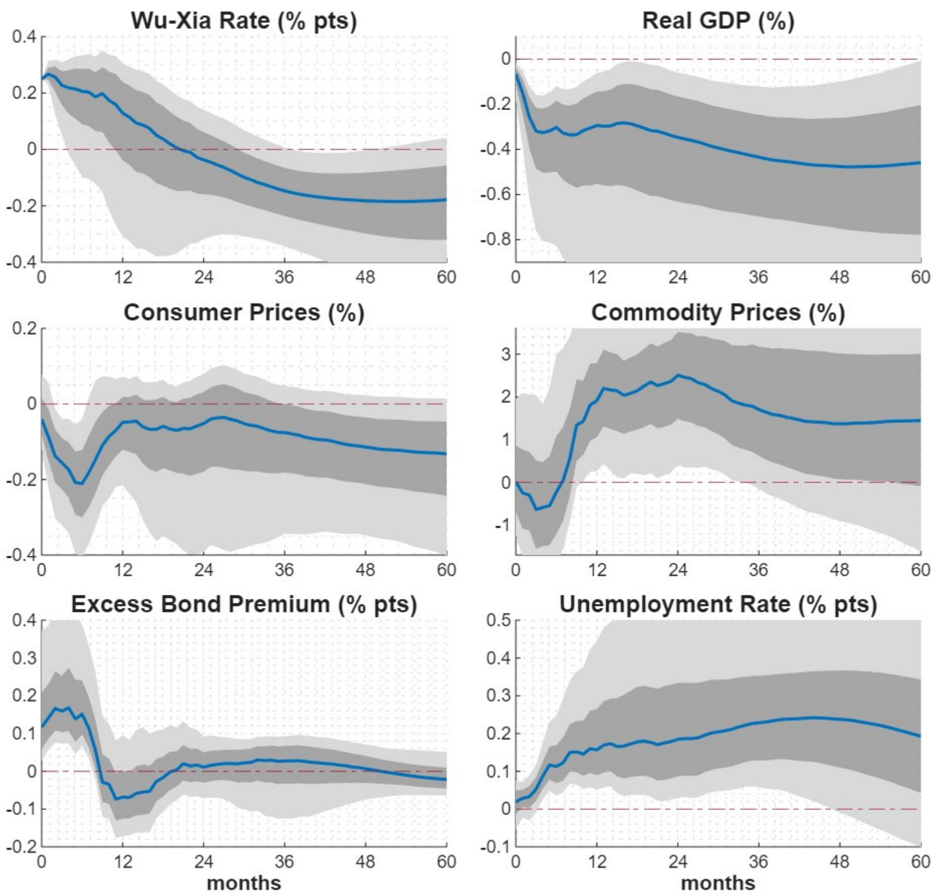
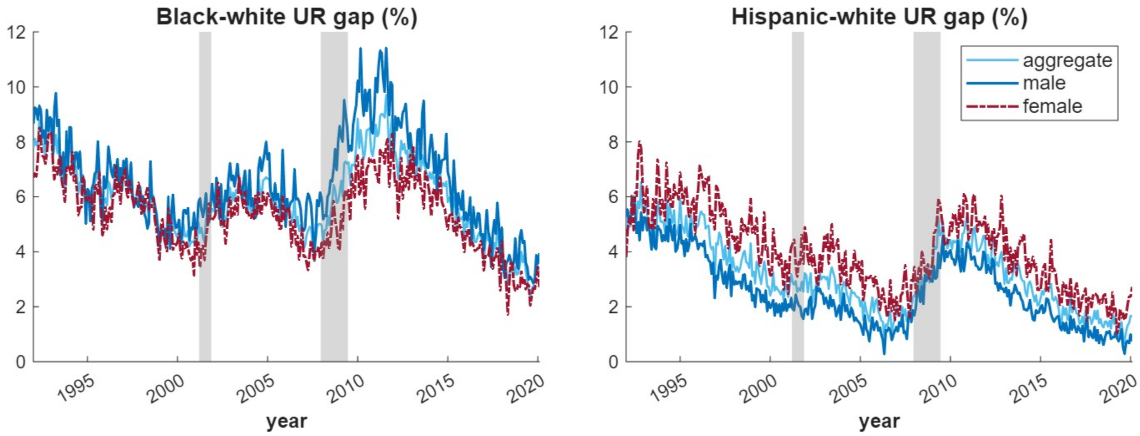
Figures and Tables

Figure 1. Baseline SVAR-IV Monetary Policy Shock Responses



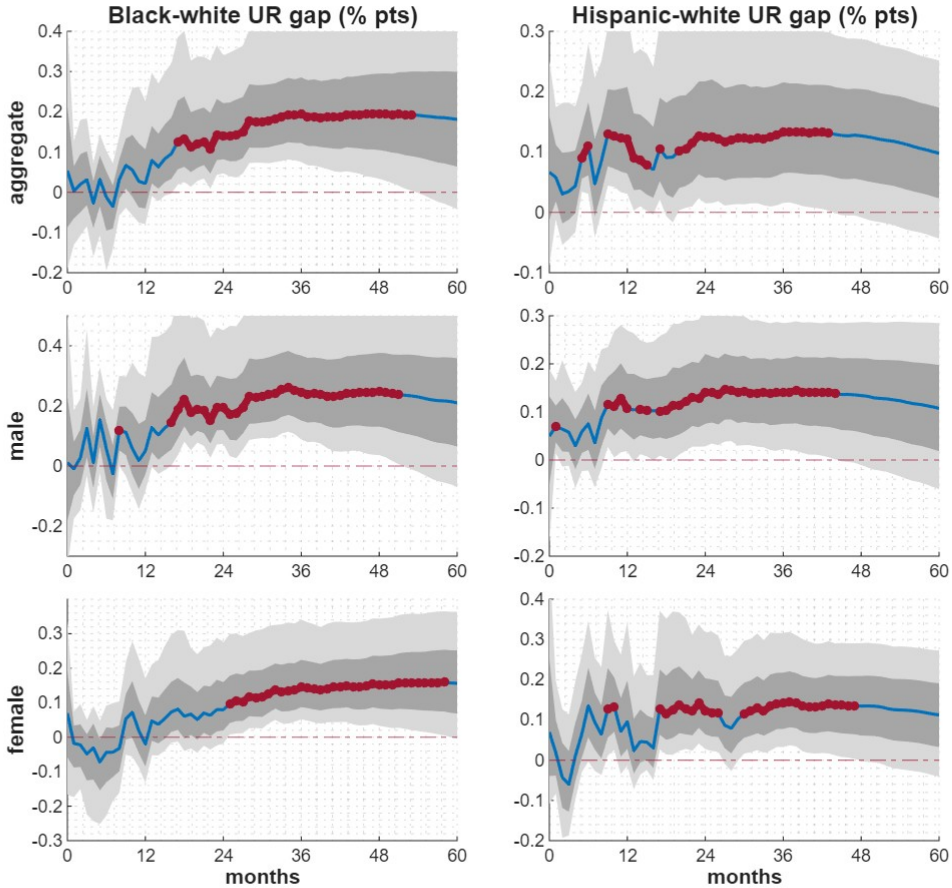
Notes: Responses to a 25 b.p. contractionary monetary policy shock. VAR estimated over 1992:01-2020:02 with 12 lags. Monetary policy instrument as in Jarociński and Karadi (2020). The first-stage F statistic is 14.91 and the heteroskedasticity-consistent F statistic is 8.15. Shaded areas are 90% (light) and 68% (dark) confidence bands computed as in Montiel Olea et al. (2021).

Figure 2. Aggregate and Sex-Specific Unemployment Gaps



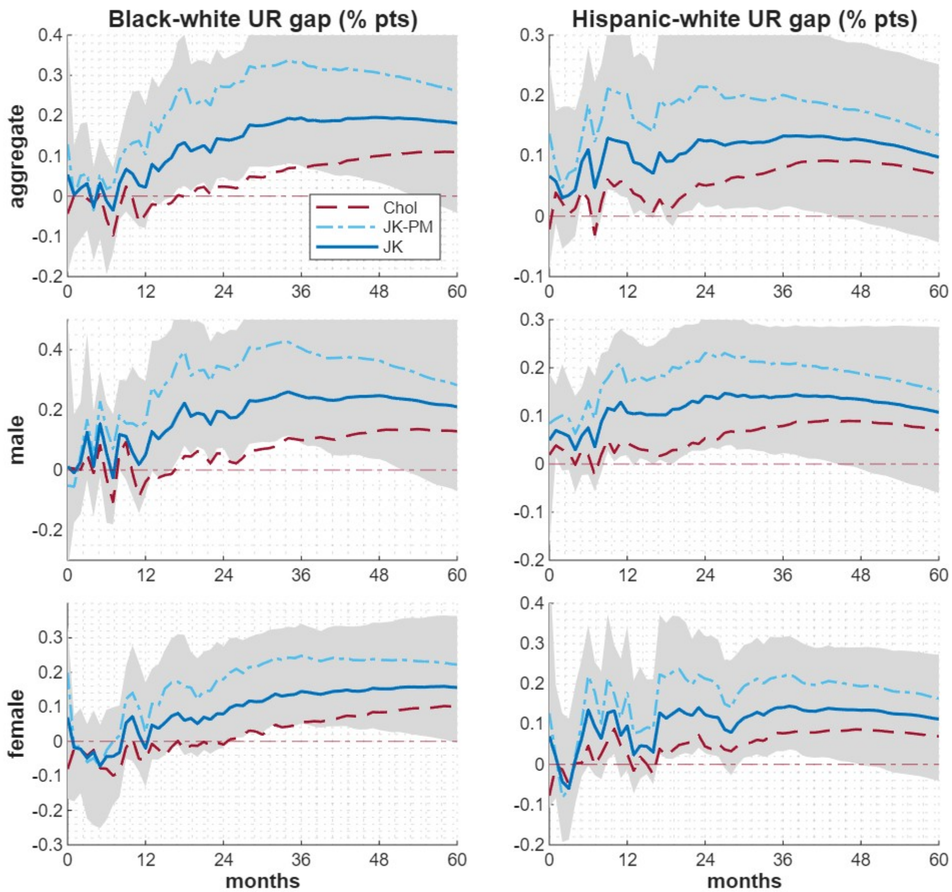
Notes: UR (unemployment rate) gaps from Jan 1992 to Feb 2020, representing the difference between Black/Hispanic and white seasonally-adjusted unemployment rates.

Figure 3. Aggregate and Sex-Specific Unemployment Gap Responses



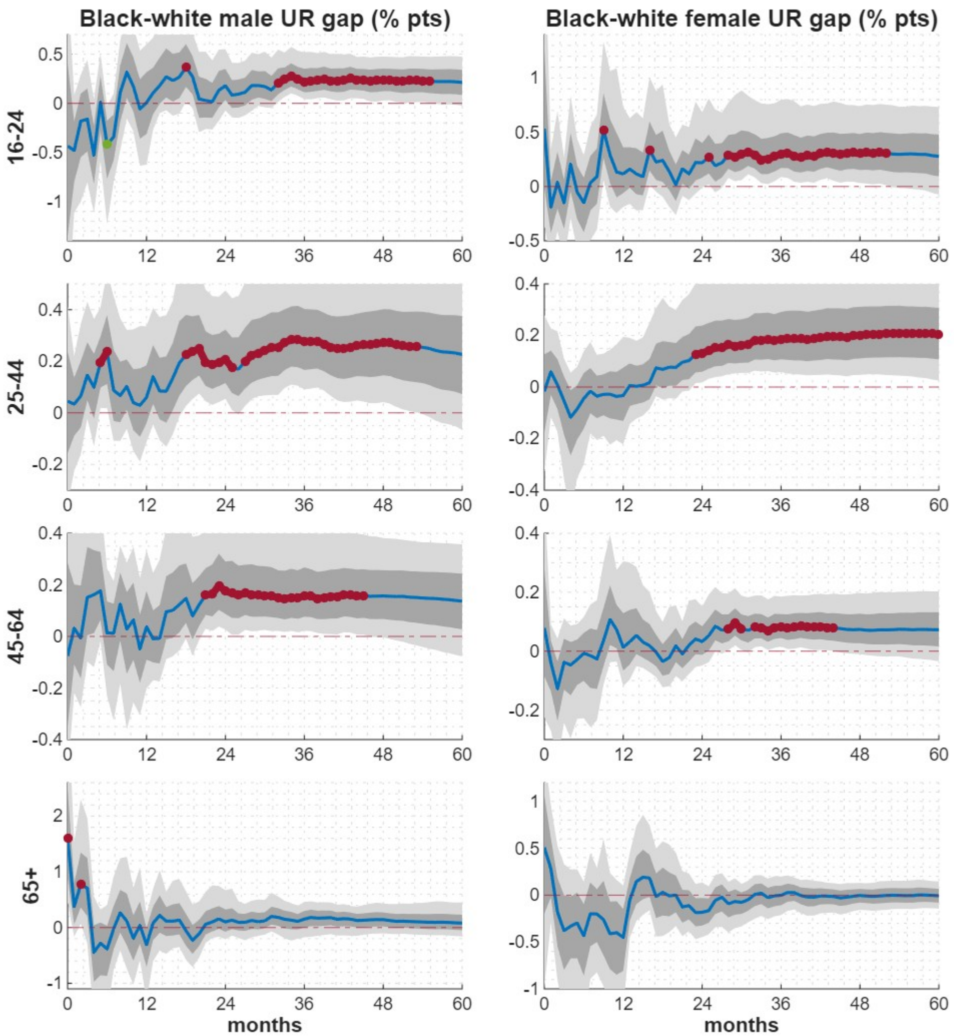
Notes: Responses of UR (unemployment rate) gaps to a 25 b.p. contractionary monetary policy shock. Separate seven-variable VARs estimated over 1992:01-2020:02 with 12 lags. Monetary policy instrument as in Jarociński and Karadi (2020). Shaded areas are 90% (light) and 68% (dark) confidence bands computed as in Montiel Olea et al. (2021). Larger dots on point estimates indicate statistical significance at the 10% level.

Figure 4. Aggregate and Sex-Specific Unemployment Gap Responses Under Different Identifications



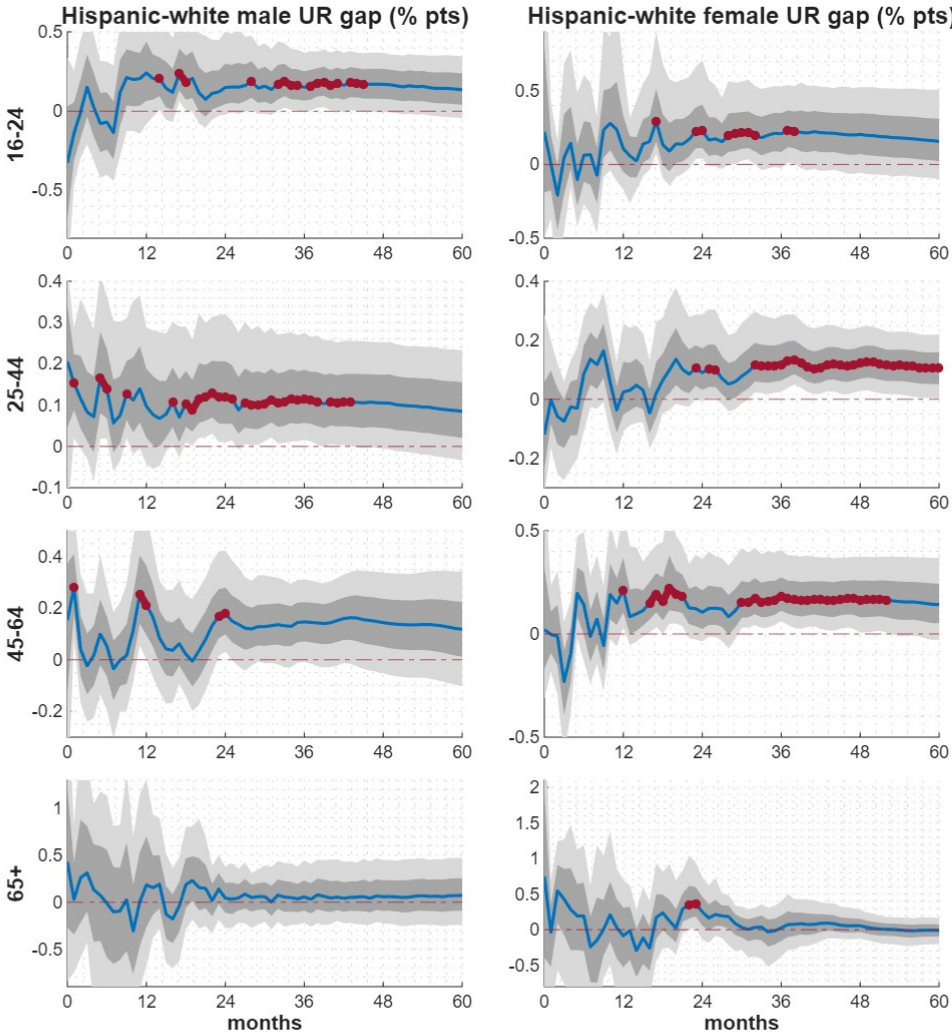
Notes: Responses of UR (unemployment rate) gaps to a 25 b.p. contractionary monetary policy shock. Separate seven-variable VARs estimated over 1992:01-2020:02 with 12 lags. Identification: 1) solid line – monetary policy instrument as in Jarociński and Karadi (2020); 2) dashed-dotted line – "poor man's" monetary policy instrument as in Jarociński and Karadi (2020); 3) dashed line – Cholesky decomposition. Shaded areas are the 90% confidence bands computed as in Montiel Olea et al. (2021), associated with the solid line estimates.

Figure 5. Black-White Sex-Specific Unemployment Gap Responses by Age



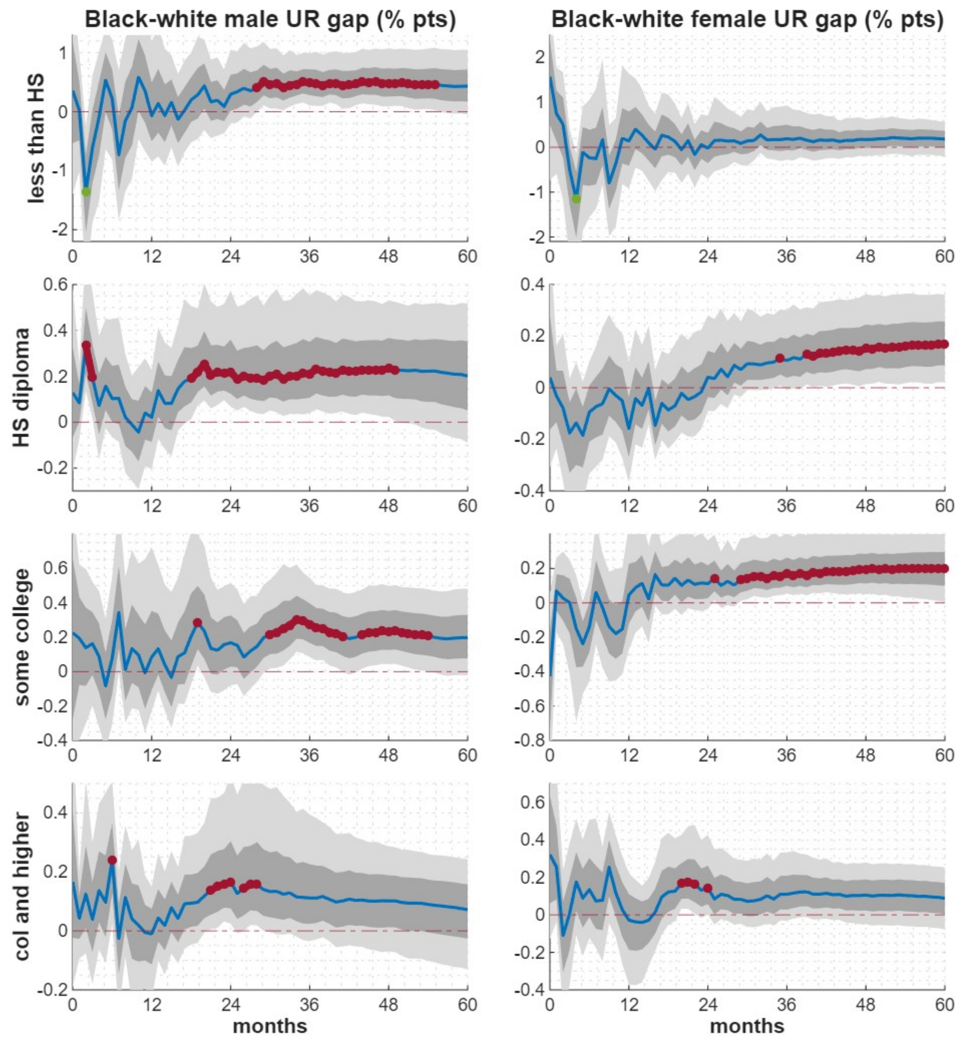
Notes: Responses of UR (unemployment rate) gaps to a 25 b.p. contractionary monetary policy shock. Separate seven-variable VARs estimated over 1992:01-2020:02 with 12 lags. Monetary policy instrument as in Jarociński and Karadi (2020). Shaded areas are 90% (light) and 68% (dark) confidence bands computed as in Montiel Olea et al. (2021). Larger dots on point estimates indicate statistical significance at the 10% level.

Figure 6. Hispanic-White Sex-Specific Unemployment Gap Responses by Age



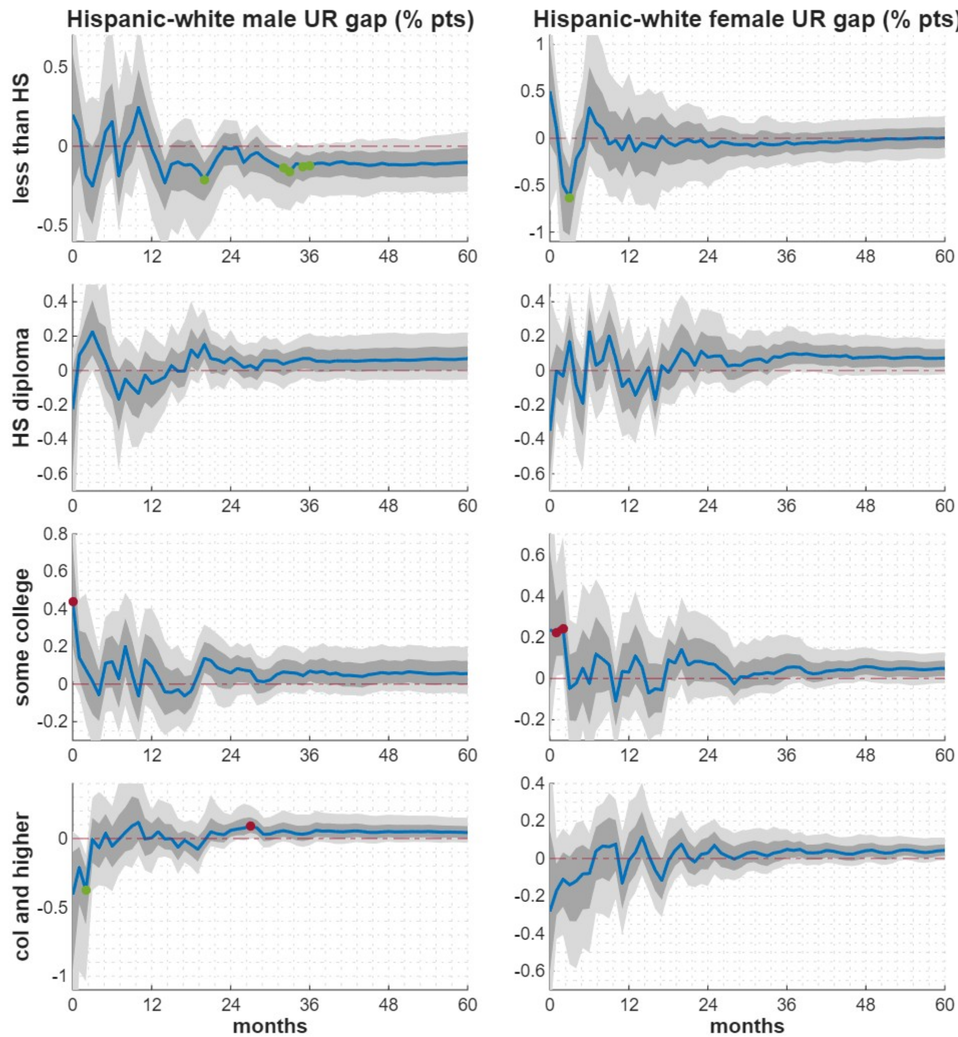
Notes: Responses of UR (unemployment rate) gaps to a 25 b.p. contractionary monetary policy shock. Separate seven-variable VARs estimated over 1992:01-2020:02 with 12 lags. Monetary policy instrument as in Jarociński and Karadi (2020). Shaded areas are 90% (light) and 68% (dark) confidence bands computed as in Montiel Olea et al. (2021). Larger dots on point estimates indicate statistical significance at the 10% level.

Figure 7. Black-White Sex-Specific Unemployment Gap Responses by Education Level, Ages 25-44



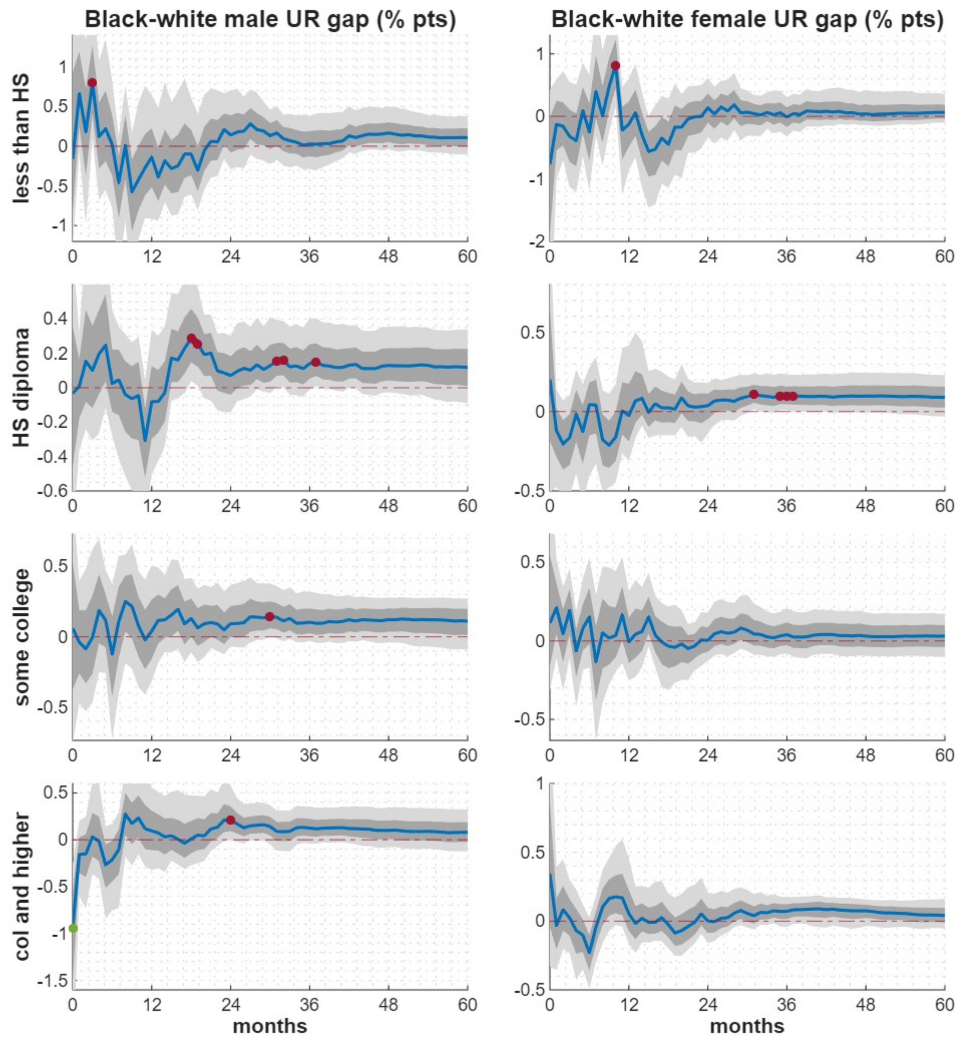
Notes: Responses of UR (unemployment rate) gaps to a 25 b.p. contractionary monetary policy shock. Separate seven-variable VARs estimated over 1992:01-2020:02 with 12 lags. Monetary policy instrument as in Jarociński and Karadi (2020). Shaded areas are 90% (light) and 68% (dark) confidence bands computed as in Montiel Olea et al. (2021). Larger dots on point estimates indicate statistical significance at the 10% level.

Figure 8. Hispanic-White Sex-Specific Unemployment Gap Responses by Education Level, Ages 25-44



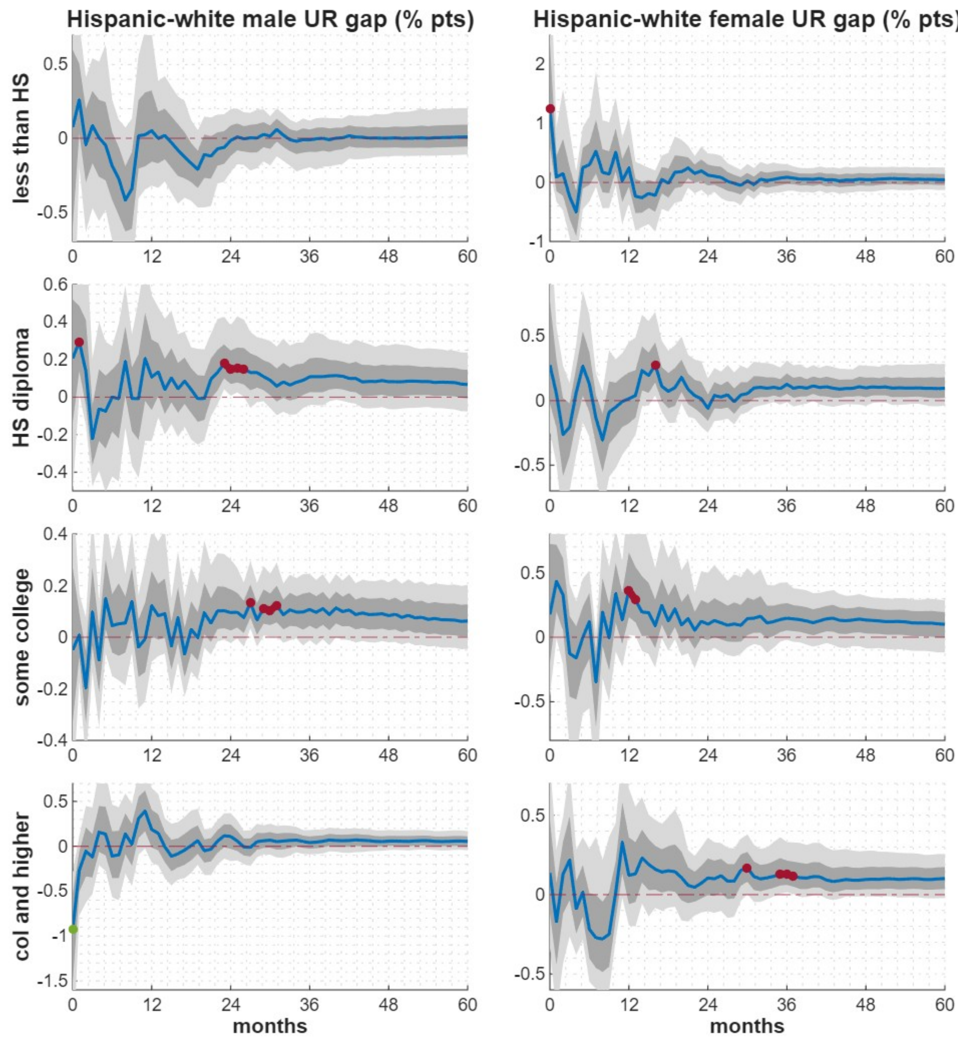
Notes: Responses of UR (unemployment rate) gaps to a 25 b.p. contractionary monetary policy shock. Separate seven-variable VARs estimated over 1992:01-2020:02 with 12 lags. Monetary policy instrument as in Jarociński and Karadi (2020). Shaded areas are 90% (light) and 68% (dark) confidence bands computed as in Montiel Olea et al. (2021). Larger dots on point estimates indicate statistical significance at the 10% level.

Figure 9. Black-White Sex-Specific Unemployment Gap Responses by Education Level, Ages 45-64



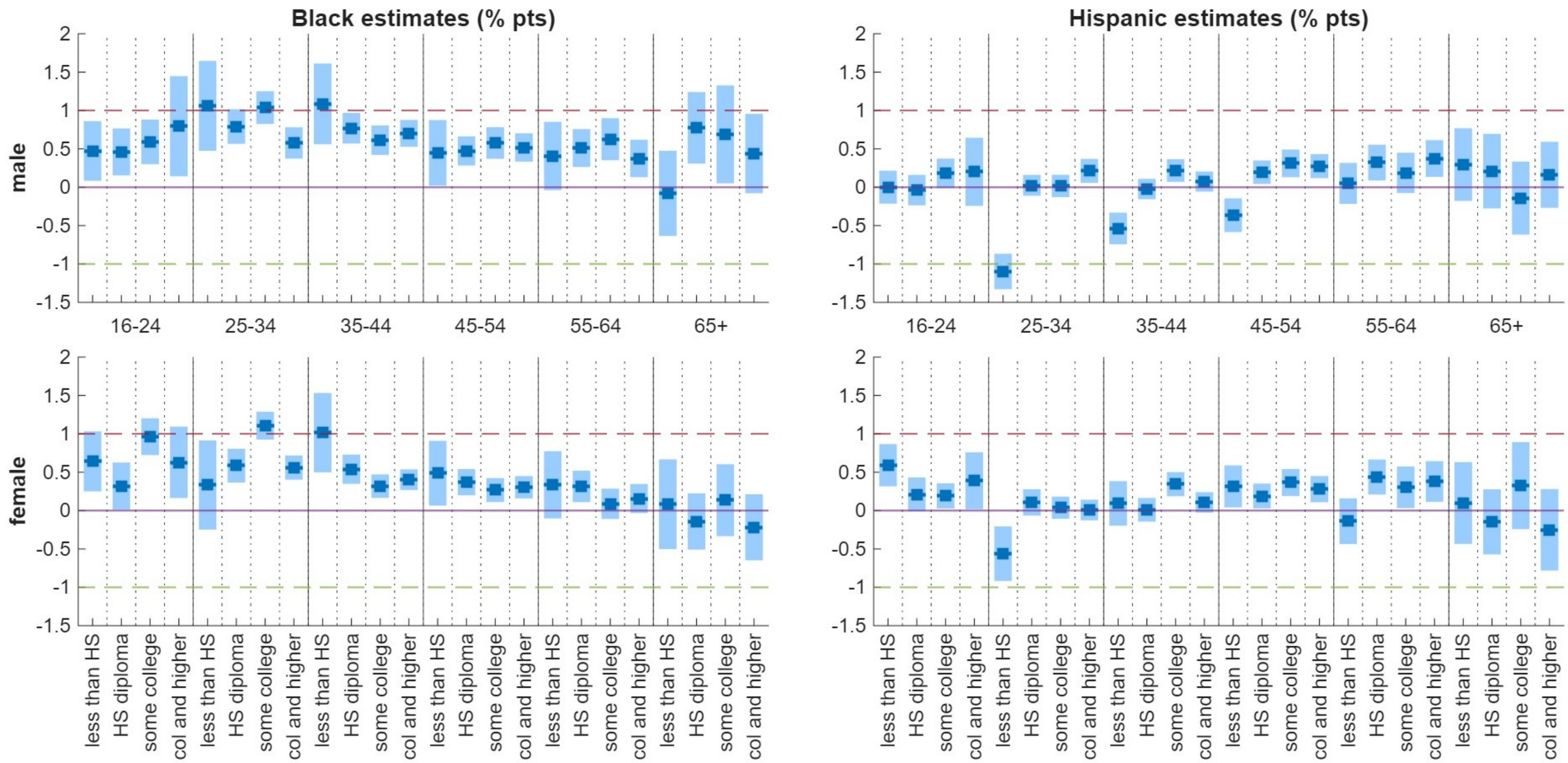
Notes: Responses of UR (unemployment rate) gaps to a 25 b.p. contractionary monetary policy shock. Separate seven-variable VARs estimated over 1992:01-2020:02 with 12 lags. Monetary policy instrument as in Jarociński and Karadi (2020). Shaded areas are 90% (light) and 68% (dark) confidence bands computed as in Montiel Olea et al. (2021). Larger dots on point estimates indicate statistical significance at the 10% level.

Figure 10. Hispanic-White Sex-Specific Unemployment Gap Responses by Education Level, Ages 45-64



Notes: Responses of UR (unemployment rate) gaps to a 25 b.p. contractionary monetary policy shock. Separate seven-variable VARs estimated over 1992:01-2020:02 with 12 lags. Monetary policy instrument as in Jarociński and Karadi (2020). Shaded areas are 90% (light) and 68% (dark) confidence bands computed as in Montiel Olea et al. (2021). Larger dots on point estimates indicate statistical significance at the 10% level.

Figure 11. Racial and Ethnic Differentials in the Marginal Effects of an Economic Contraction on the Probability of Being Unemployed



Notes: Racial and ethnic differentials in the marginal effects of unemployment probability following a one percentage point increase in the 12-month average state-level unemployment rate. Estimates represent the percentage point difference between Black/Hispanic and white groups. Shaded areas around point estimates represent the 99% heteroskedasticity-consistent confidence intervals.

Table 1. Decomposition of the Responses of Sex-Specific Racial and Ethnic Unemployment Gaps to an Economic Contraction

	Black-white				Hispanic-white			
	male		female		male		female	
	% pts	% of gap	% pts	% of gap	% pts	% of gap	% pts	% of gap
Total	0.8026	100.00	0.5919	100.00	0.2861	100.00	0.3459	100.00
Explained	0.1272	15.85	0.0973	16.43	0.3577	125.00	0.1674	48.40
age	-0.0002	-0.03	0.0041	0.69	0.0006	0.20	0.0026	0.76
16-24	-0.0014	-0.18	-0.0024	-0.41	-0.0022	-0.76	-0.0074	-2.14
25-34	0.0019	0.23	0.0019	0.32	0.0038	1.33	0.0027	0.78
45-54	0.0004	0.04	0.0014	0.23	0.0009	0.32	0.0025	0.73
55-64	-0.0023	-0.28	0.0017	0.29	-0.0036	-1.25	0.0027	0.77
65+	0.0012	0.15	0.0015	0.25	0.0016	0.56	0.0021	0.62
education	0.0552	6.88	0.0376	6.36	0.1603	56.02	0.0860	24.85
less than HS	0.0150	1.87	0.0083	1.41	0.0839	29.33	0.0350	10.13
some college	-0.0030	-0.38	-0.0019	-0.33	0.0117	4.08	0.0031	0.89
col and higher	0.0432	5.38	0.0312	5.28	0.0647	22.60	0.0479	13.84
occupation	0.0460	5.74	0.0252	4.26	0.0809	28.27	0.0399	11.53
tech/sales support	-0.0020	-0.25	-0.0028	-0.47	-0.0095	-3.31	-0.0046	-1.32
service	0.0141	1.75	0.0139	2.35	0.0093	3.26	0.0171	4.95
farming/fishing	-0.0045	-0.56	-0.0019	-0.32	0.0129	4.51	0.0015	0.45
production/craft	-0.0210	-2.62	-0.0000	-0.00	0.0163	5.70	0.0007	0.20
operators/laborers	0.0595	7.42	0.0161	2.72	0.0518	18.11	0.0251	7.26
industry	-0.0665	-8.28	-0.0233	-3.93	0.0810	28.29	0.0180	5.21
agriculture/fishery	-0.0211	-2.63	-0.0085	-1.44	0.0481	16.83	0.0017	0.49
mining	-0.0051	-0.63	-0.0008	-0.14	-0.0012	-0.43	-0.0004	-0.12
construction	-0.0800	-9.97	-0.0134	-2.27	0.1183	41.35	-0.0052	-1.50
manufacturing	-0.0181	-2.26	-0.0023	-0.39	-0.0265	-9.26	0.0192	5.56
transport/comm	0.0353	4.40	0.0108	1.82	-0.0040	-1.38	0.0001	0.04
wholesale trade	-0.0098	-1.22	-0.0066	-1.12	-0.0051	-1.79	0.0012	0.36
retail trade	0.0175	2.18	-0.0142	-2.41	0.0318	11.12	0.0240	6.93
FIRE	-0.0148	-1.84	-0.0107	-1.81	-0.0212	-7.41	-0.0135	-3.90
bus services	0.0134	1.67	0.0064	1.08	0.0052	1.81	0.0187	5.40
pers services	0.0102	1.26	0.0043	0.73	0.0094	3.29	0.0244	7.05
entertainment	-0.0015	-0.19	-0.0069	-1.16	-0.0042	-1.47	-0.0042	-1.20
prof services	0.0014	0.17	0.0069	1.16	-0.0584	-20.42	-0.0454	-13.13
public admin	0.0062	0.77	0.0119	2.02	-0.0114	-3.97	-0.0026	-0.76
other	0.0927	11.55	0.0536	9.05	0.0350	12.22	0.0209	6.04
married	0.0829	10.33	0.0521	8.81	0.0363	12.68	0.0204	5.9
veteran	-0.0016	-0.20	0.0005	0.08	-0.0117	-4.09	-0.0003	-0.10
urban	0.0114	1.42	0.0010	0.17	0.0104	3.63	0.0008	0.24
Unexplained	0.6754	84.15	0.4946	83.57	-0.0715	-25.00	0.1785	51.60

Notes: Estimates computed via weighted least squares Oaxaca-Blinder decomposition. The gap estimates represent the percentage-point differential responses between Black/Hispanic and white unemployment probabilities following a one percentage point increase in the 12-month average state-level unemployment rate.

Appendix A. Responses of Employment Gaps to Monetary Policy Shocks

Macroeconomic labor market outcomes can be measured in several ways. For this reason, following the estimation procedure for the response of unemployment gaps, we provide estimates for the racial and ethnic employment-population ratio (employment) gaps. In doing so, we keep all baseline SVAR-IV specifications as detailed in our paper with one exception – swapping the civilian unemployment rate with the civilian employment-population ratio in the vector of macroeconomic and financial variables. Using this 'revised' baseline SVAR-IV, we append employment gap variables one at a time and estimate several separate models outlined below.

Figure A.1 presents a plot of the monthly aggregate and sex-specific gaps data; Figure A.2 presents the responses of these gaps to contractionary monetary policy shocks. Opposite to the response of unemployment gaps presented earlier, employment gap widenings, i.e., Black or Hispanic employment decreases more than white employment following a monetary contraction, appear as a negative response, colored in red if statistically significant at the 10 percent level.

The employment gaps respond similarly to respective unemployment gaps – significance is not immediate, but after about a year following the monetary policy shock, they widen with a large magnitude relative to the shock size and with a strong degree of persistence. The aggregate Black-white employment gap reaches a trough of about 0.16 percentage points, with the minimum response for males being a little stronger (0.20 percentage points). The negative response for females evolves insignificantly. The aggregate and sex-specific Hispanic-white employment gaps respond similarly.

Carpenter and Rodgers (2004) estimate employment-population ratio gaps also. They report a trough response for the Black-white gap of 0.11 percentage points following a 1.24 percentage point monetary policy shock and an insignificant response for the Hispanic-white gap. After rescaling the shock size, this suggests that our minimum estimate for the widening of the Black-white gap is 7.2 times larger.

Next, we compare our aggregate and sex-specific employment gap estimates to noninstrumented Cholesky and "poor man's sign restrictions" estimates, presented in Figure A.3.

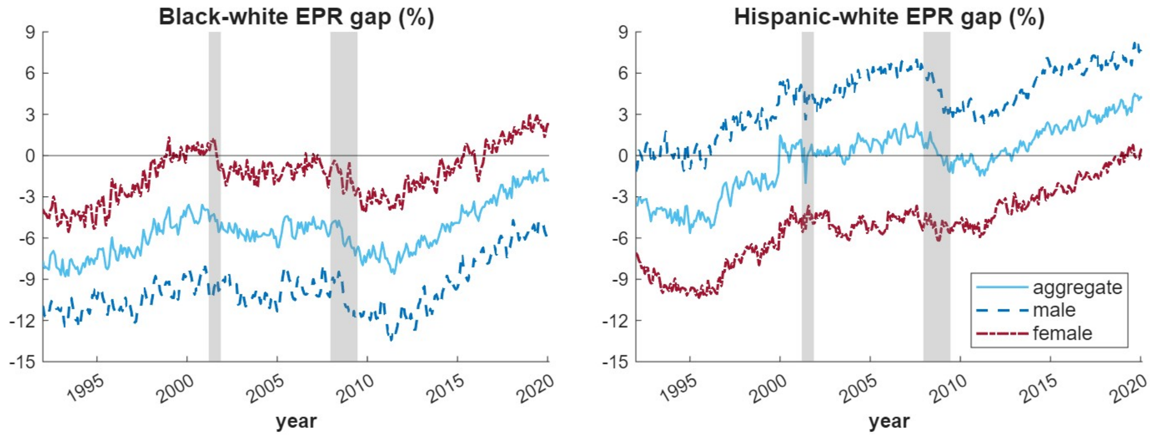
As is the case for the unemployment gaps, Cholesky responses are closer to the zero horizontal line. In contrast, the "poor man's" estimates are more pronounced than when the instrument is not adjusted for financial movements. However, the "poor man's" version of the instrument is questionable with F statistics ranging from 7.79 to 9.58. The nonadjusted instrument that we continue using produces F statistics in the range between 16.81 and 19.58.

Next, we construct the sex-specific employment gaps for each of the four age brackets. Figure A.4 shows the Black-white responses and Figure A.5 shows the Hispanic-white responses. Generally, employment gaps appear to respond less persistently than respective unemployment gaps. Nevertheless, those that do – younger Hispanic males, Black and Hispanic individuals in the 25-44 age group, and Black males of ages 45-to-64 – achieve persistent troughs spanning from 0.15 to 0.26 percentage points in the statistically significant range.

Lastly, we construct the sex-specific employment gaps by level of education for the 25-44 age bracket (see Figures A.6 and A.7) and for the 45-64 age bracket (see Figures A.8 and A.9). The responses of the Hispanic-white gaps generally suggest the same interpretation as that for the responses of respective unemployment gaps – accounting for educational attainment generally eliminates the disparate effect of monetary contractions for Hispanic versus white males and females. The notable exceptions are the Hispanic-white male gap for high school graduates of ages 25-to-44 and the Hispanic-white female gap for the lowest education group of ages 45-to-64. The former widens significantly two years following a monetary policy shock, while the latter widens instantaneously by 0.43 percentage points and remains significant for a year and a half. The Black-white employment gaps also respond less persistently compared to respective unemployment gap estimates. Both the highest and least educated Black-white female employment gaps respond significantly, with the latter realizing an immediate trough of 0.73 percentage points (to a 0.25 percentage points monetary contraction).

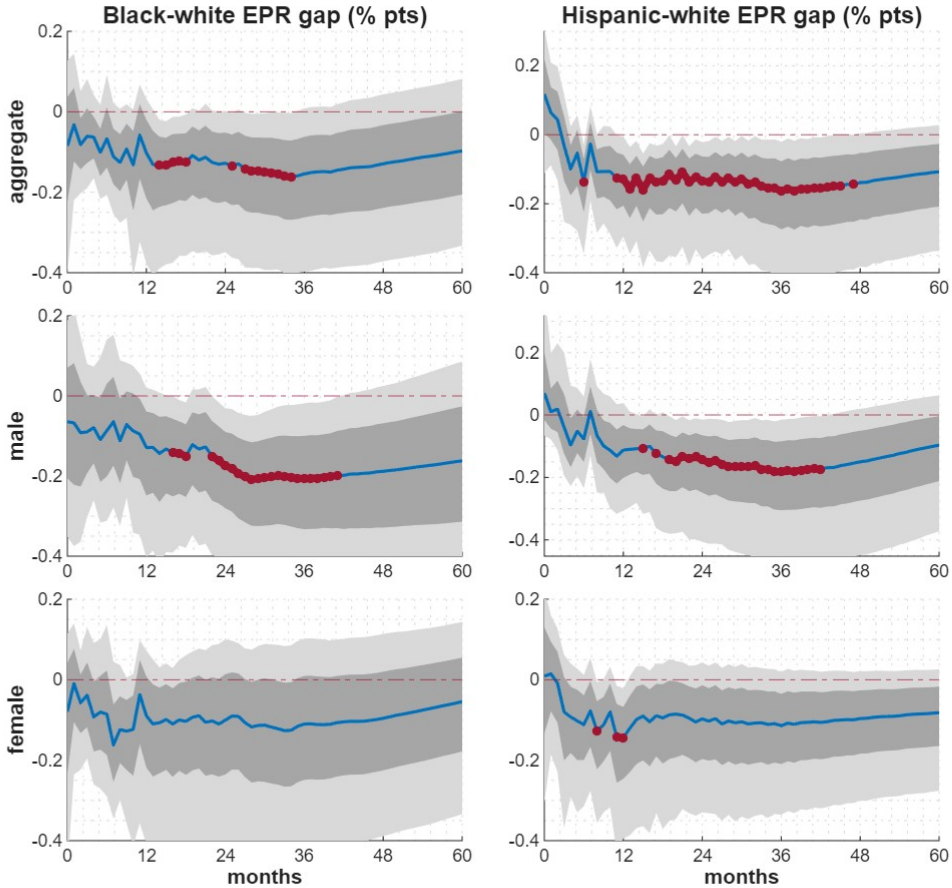
Overall, the employment-population ratio gaps appear less responsive to monetary policy shocks; however, abstracting from significance levels, the employment-population ratio gaps generally exhibit similar behavior when compared to respective unemployment rate gaps.

Figure A.1. Aggregate and Sex-Specific Employment Gaps



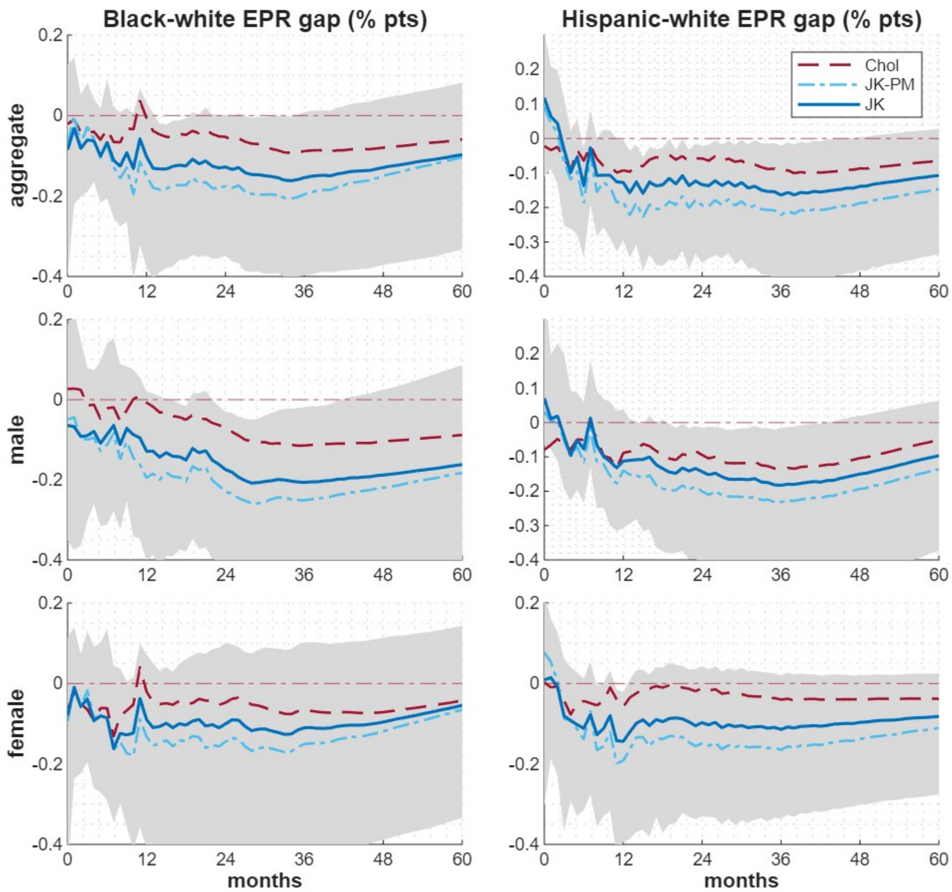
Notes: EPR (employment-population ratio) gaps from Jan 1992 to Feb 2020, representing the difference between Black/Hispanic and white seasonally-adjusted employment-population ratios.

Figure A.2. Aggregate and Sex-Specific Employment Gap Responses



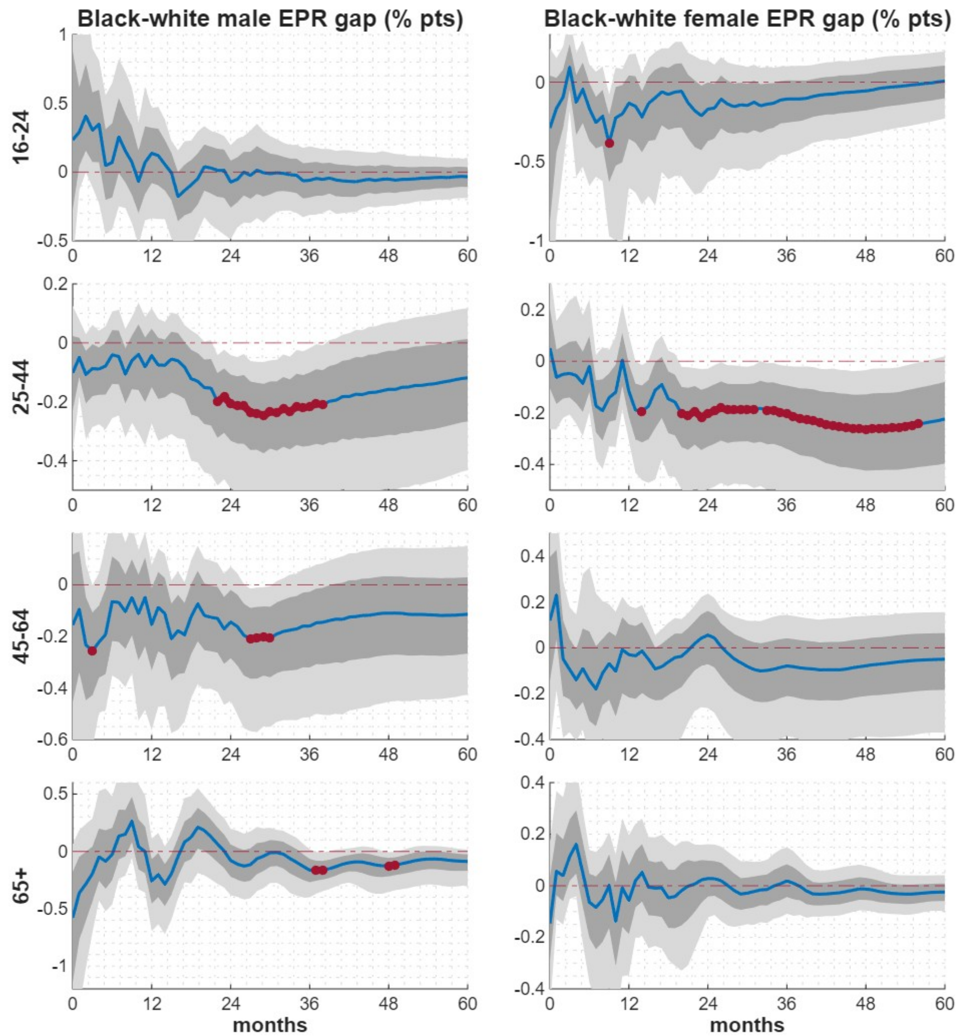
Notes: Responses of EPR (employment-population ratio) gaps to a 25 b.p. contractionary monetary policy shock. Separate seven-variable VARs estimated over 1992:01-2020:02 with 12 lags. Monetary policy instrument as in Jarociński and Karadi (2020). Shaded areas are 90% (light) and 68% (dark) confidence bands computed as in Montiel Olea et al. (2021). Larger dots on point estimates indicate statistical significance at the 10% level.

Figure A.3. Aggregate and Sex-Specific Employment Gap Responses Under Different Identifications



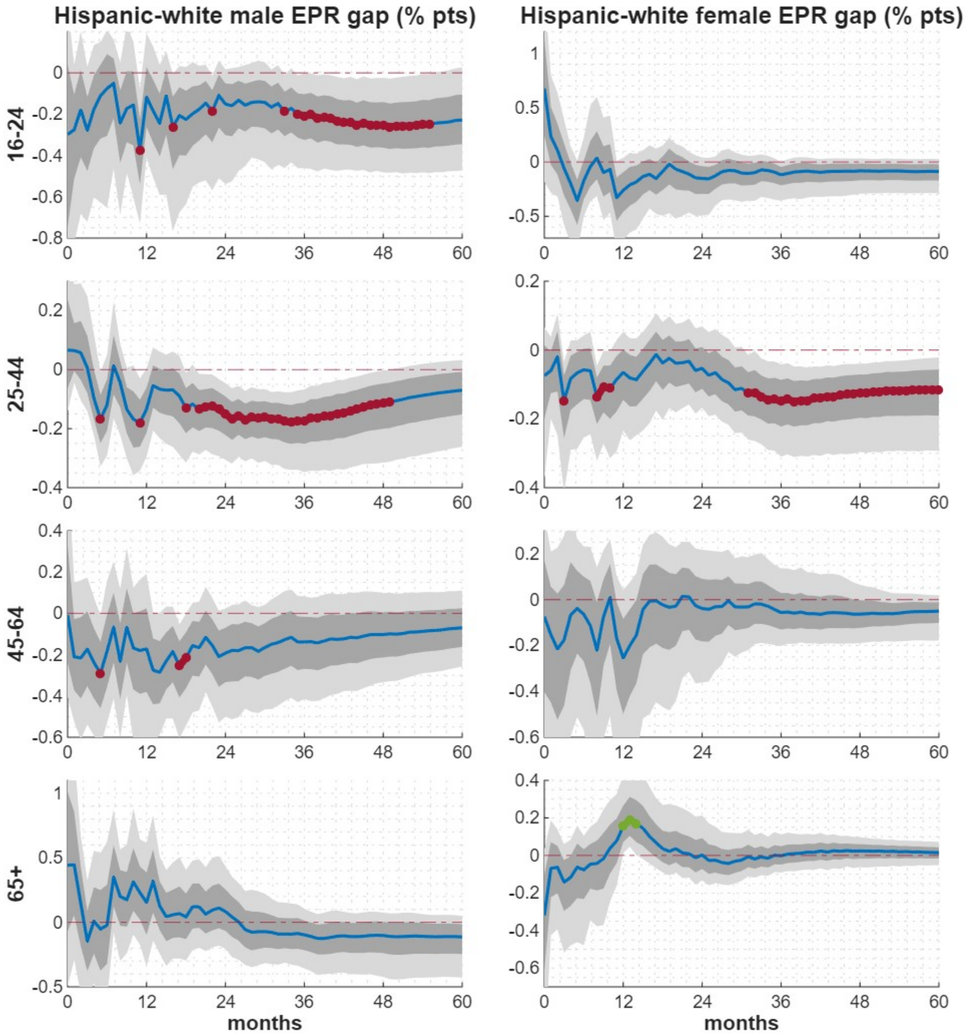
Notes: Responses of EPR (employment-population ratio) gaps to a 25 b.p. contractionary monetary policy shock. Separate seven-variable VARs estimated over 1992:01-2020:02 with 12 lags. Identification: 1) solid line – monetary policy instrument as in Jarociński and Karadi (2020); 2) dashed-dotted line – "poor man's" monetary policy instrument as in Jarociński and Karadi (2020); 3) dashed line – Cholesky decomposition. Shaded areas are the 90% confidence bands computed as in Montiel Olea et al. (2021), associated with the solid line estimates.

Figure A.4. Black-White Sex-Specific Employment Gap Responses by Age



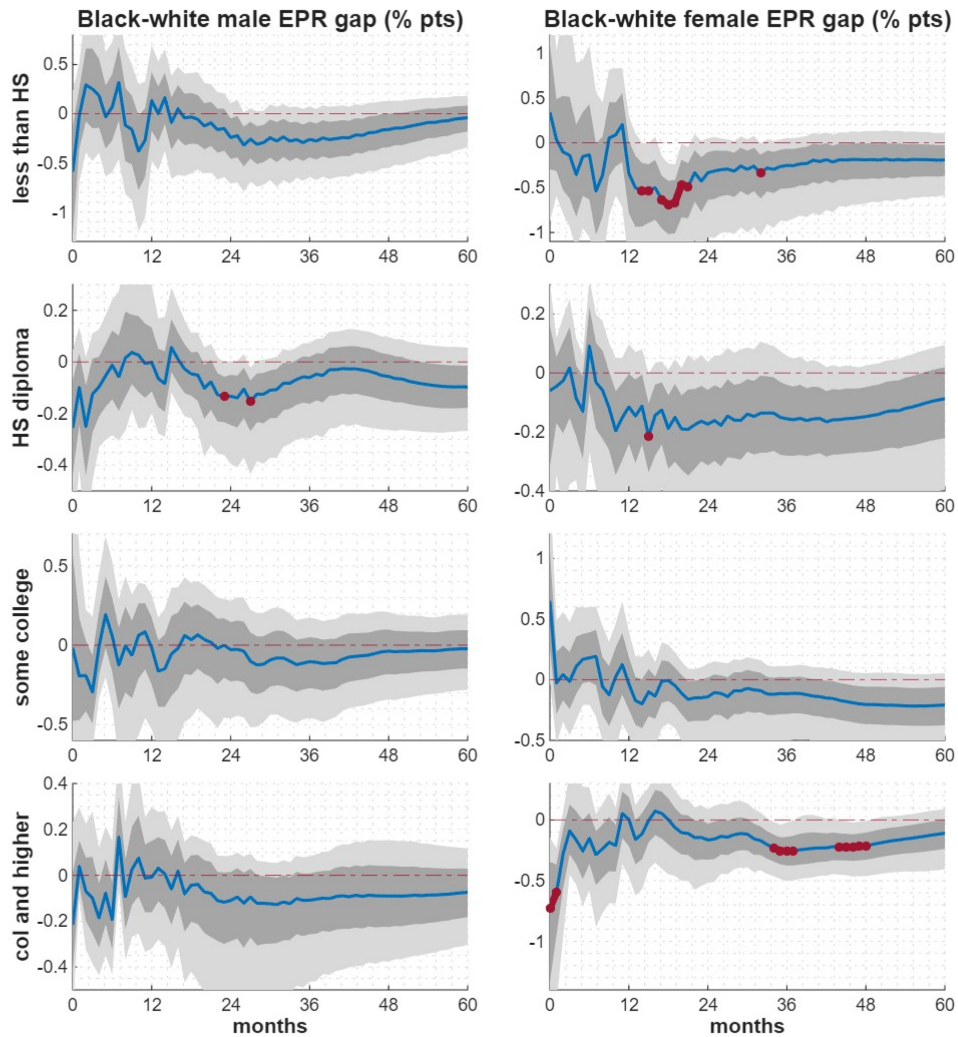
Notes: Responses of EPR (employment-population ratio) gaps to a 25 b.p. contractionary monetary policy shock. Separate seven-variable VARs estimated over 1992:01-2020:02 with 12 lags. Monetary policy instrument as in Jarociński and Karadi (2020). Shaded areas are 90% (light) and 68% (dark) confidence bands computed as in Montiel Olea et al. (2021). Larger dots on point estimates indicate statistical significance at the 10% level.

Figure A.5. Hispanic-White Sex-Specific Employment Gap Responses by Age



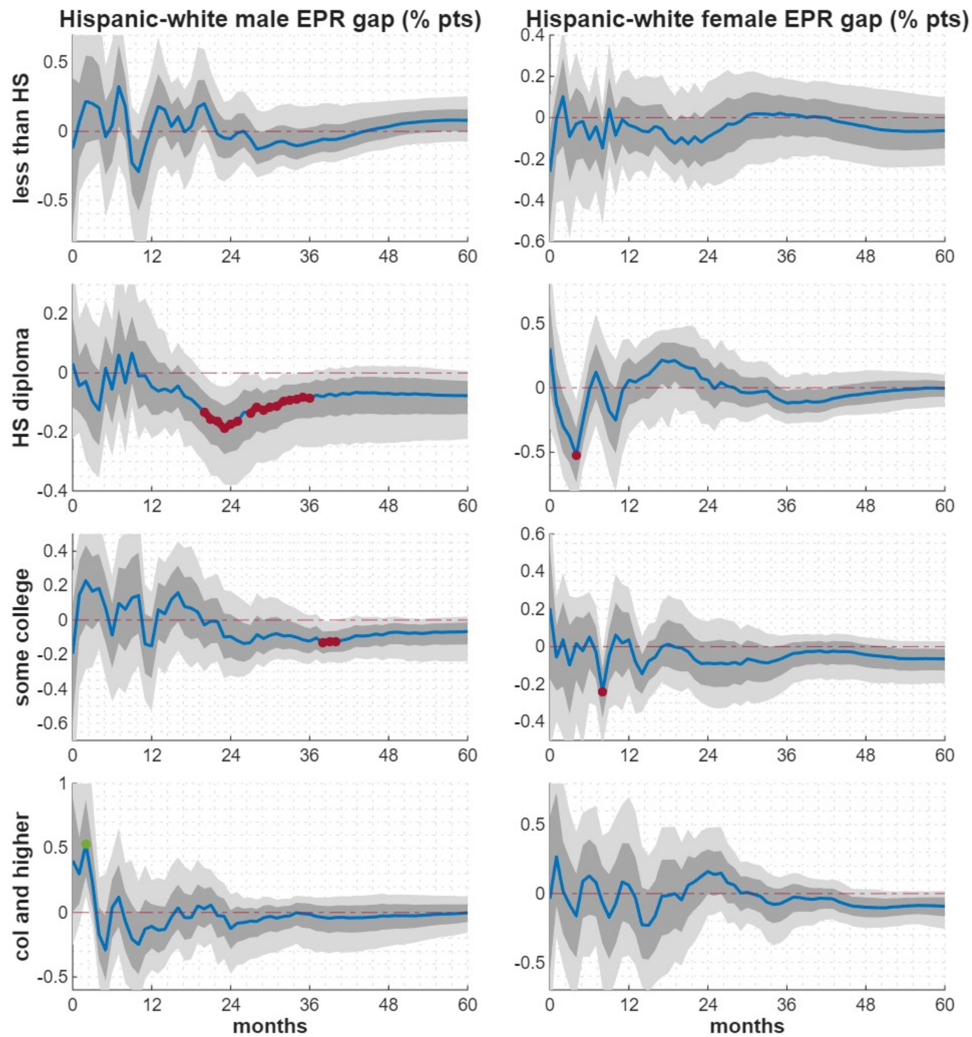
Notes: Responses of EPR (employment-population ratio) gaps to a 25 b.p. contractionary monetary policy shock. Separate seven-variable VARs estimated over 1992:01-2020:02 with 12 lags. Monetary policy instrument as in Jarociński and Karadi (2020). Shaded areas are 90% (light) and 68% (dark) confidence bands computed as in Montiel Olea et al. (2021). Larger dots on point estimates indicate statistical significance at the 10% level.

Figure A.6. Black-White Sex-Specific Employment Gap Responses by Education Level, Ages 25-44



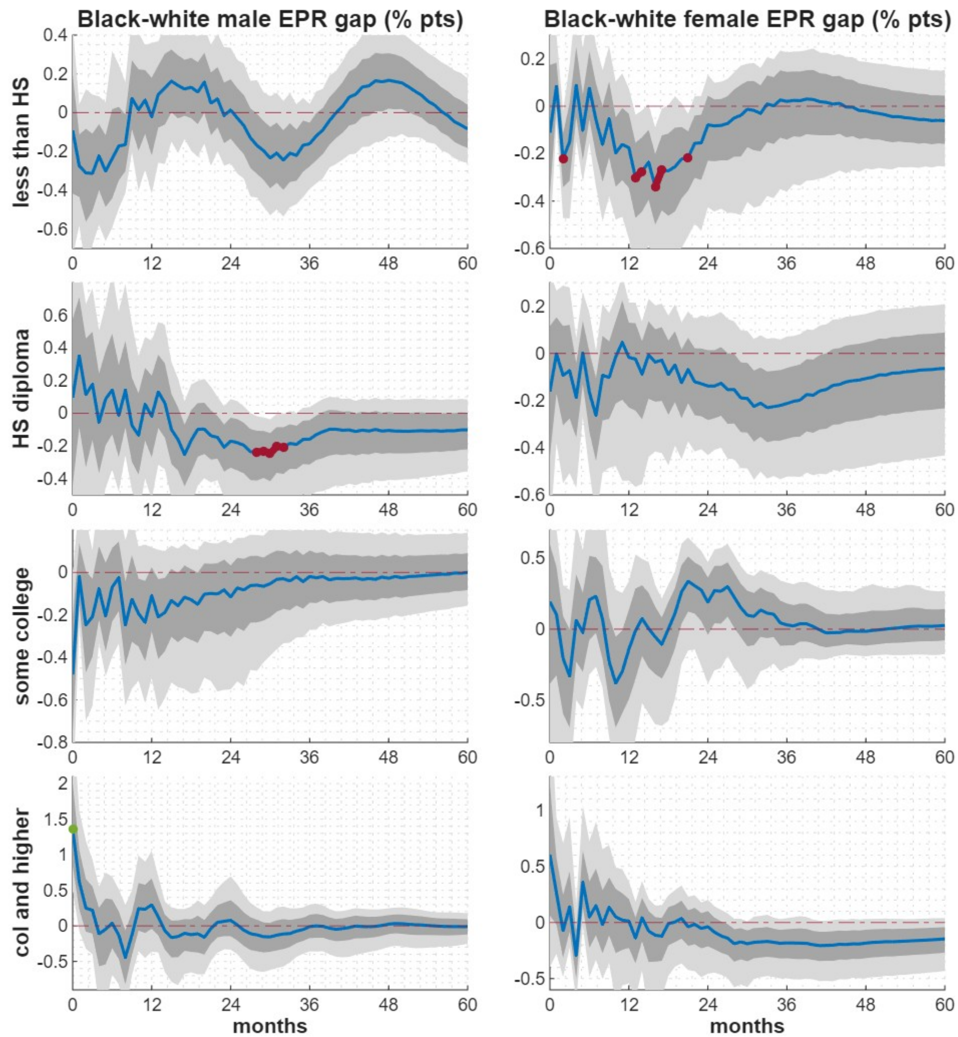
Notes: Responses of EPR (employment-population ratio) gaps to a 25 b.p. contractionary monetary policy shock. Separate seven-variable VARs estimated over 1992:01-2020:02 with 12 lags. Monetary policy instrument as in Jarociński and Karadi (2020). Shaded areas are 90% (light) and 68% (dark) confidence bands computed as in Montiel Olea et al. (2021). Larger dots on point estimates indicate statistical significance at the 10% level.

Figure A.7. Hispanic-White Sex-Specific Employment Gap Responses by Education Level, Ages 25-44



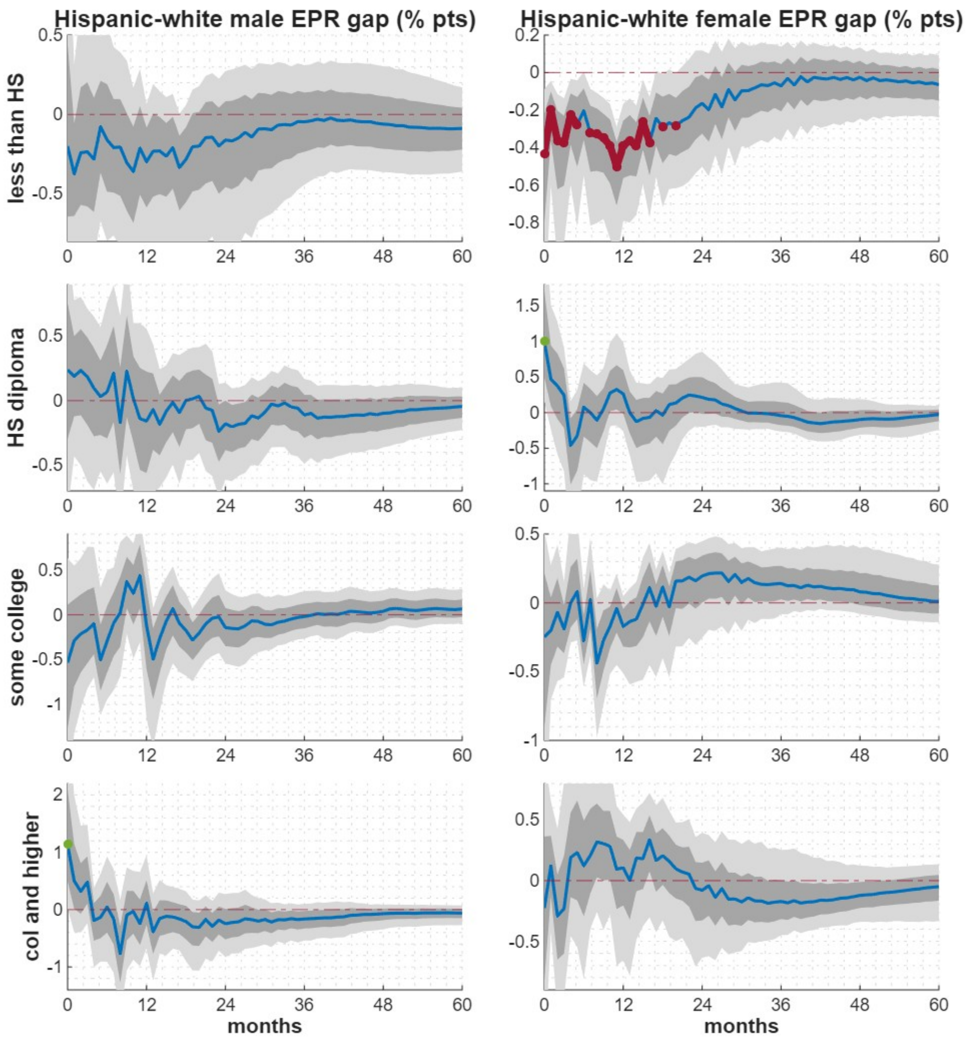
Notes: Responses of EPR (employment-population ratio) gaps to a 25 b.p. contractionary monetary policy shock. Separate seven-variable VARs estimated over 1992:01-2020:02 with 12 lags. Monetary policy instrument as in Jarociński and Karadi (2020). Shaded areas are 90% (light) and 68% (dark) confidence bands computed as in Montiel Olea et al. (2021). Larger dots on point estimates indicate statistical significance at the 10% level.

Figure A.8. Black-White Sex-Specific Employment Gap Responses by Education Level, Ages 45-64



Notes: Responses of EPR (employment-population ratio) gaps to a 25 b.p. contractionary monetary policy shock. Separate seven-variable VARs estimated over 1992:01-2020:02 with 12 lags. Monetary policy instrument as in Jarociński and Karadi (2020). Shaded areas are 90% (light) and 68% (dark) confidence bands computed as in Montiel Olea et al. (2021). Larger dots on point estimates indicate statistical significance at the 10% level.

Figure A.9. Hispanic-White Sex-Specific Employment Gap Responses by Education Level, Ages 45-64



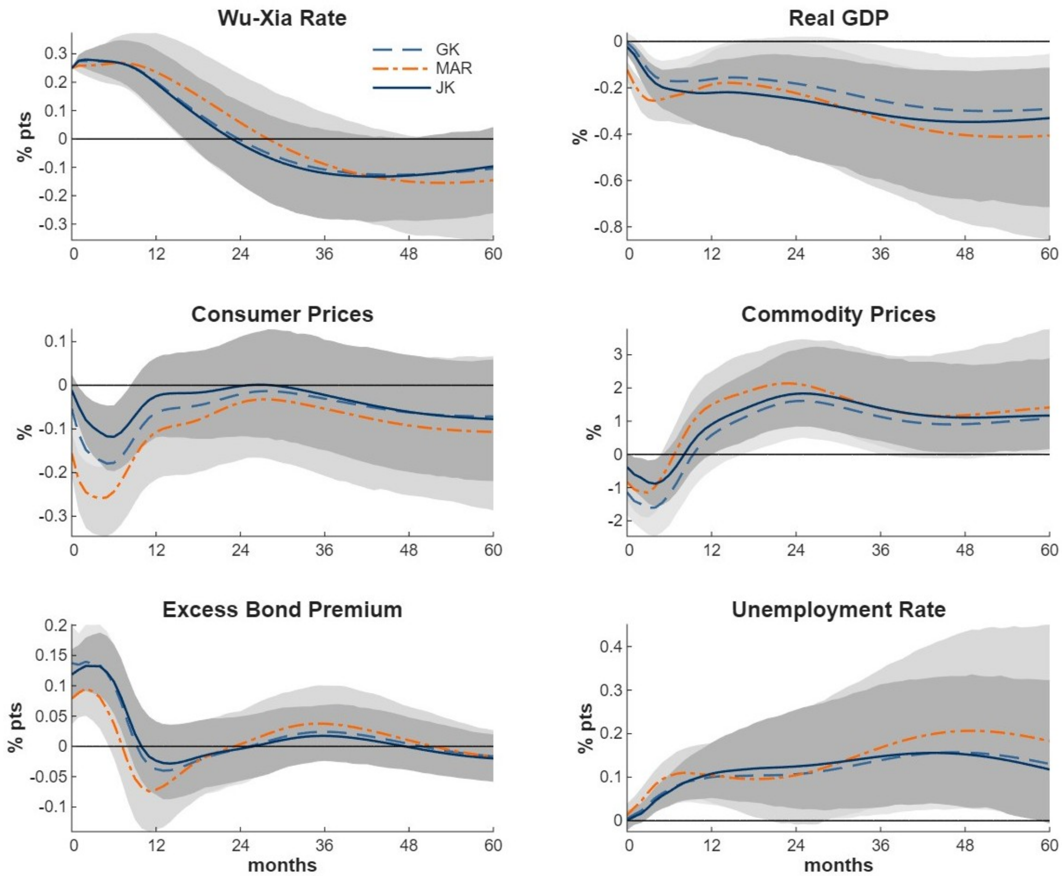
Notes: Responses of EPR (employment-population ratio) gaps to a 25 b.p. contractionary monetary policy shock. Separate seven-variable VARs estimated over 1992:01-2020:02 with 12 lags. Monetary policy instrument as in Jarociński and Karadi (2020). Shaded areas are 90% (light) and 68% (dark) confidence bands computed as in Montiel Olea et al. (2021). Larger dots on point estimates indicate statistical significance at the 10% level.

Appendix B. Bayesian SVAR-IV

We estimate a Bayesian version of our baseline SVAR-IV using replication materials from Miranda-Agrippino and Ricco (2021), collecting a view that resembles the third figure presented in their paper. First, we substitute their macroeconomic data with our own to reflect our sample. Next, we swap their original monetary policy instrument with an equivalent series, updated through December 2015 by Degasperi and Ricco (2021). All other baseline model specifications remain as we describe in our paper. As is done in the original Miranda-Agrippino and Ricco (2021) figure we are replicating, we show results using three instruments – Gertler and Karadi (2015) (GK), the extended Miranda-Agrippino and Ricco (2021) (MAR), and Jarociński and Karadi (2020) (JK) (see Figure B.1).

Overall, the Bayesian JK responses closely resemble our own baseline estimates while being more computationally expensive and requiring the assignment of a prior. Furthermore, the Bayesian framework used by Miranda-Agrippino and Ricco (2021) does not produce confidence bands that are robust to the potential weak-instrument problem.

Figure B.1. Bayesian SVAR-IV Monetary Policy Shock Responses



Notes: Responses to a 25 b.p. contractionary monetary policy shock. VARs estimated over 1992:01-2020:02 with 12 lags. Monetary policy instrument varies. Shaded areas are 90% posterior coverage bands.

Appendix C. Alternative SVAR-IV Specifications

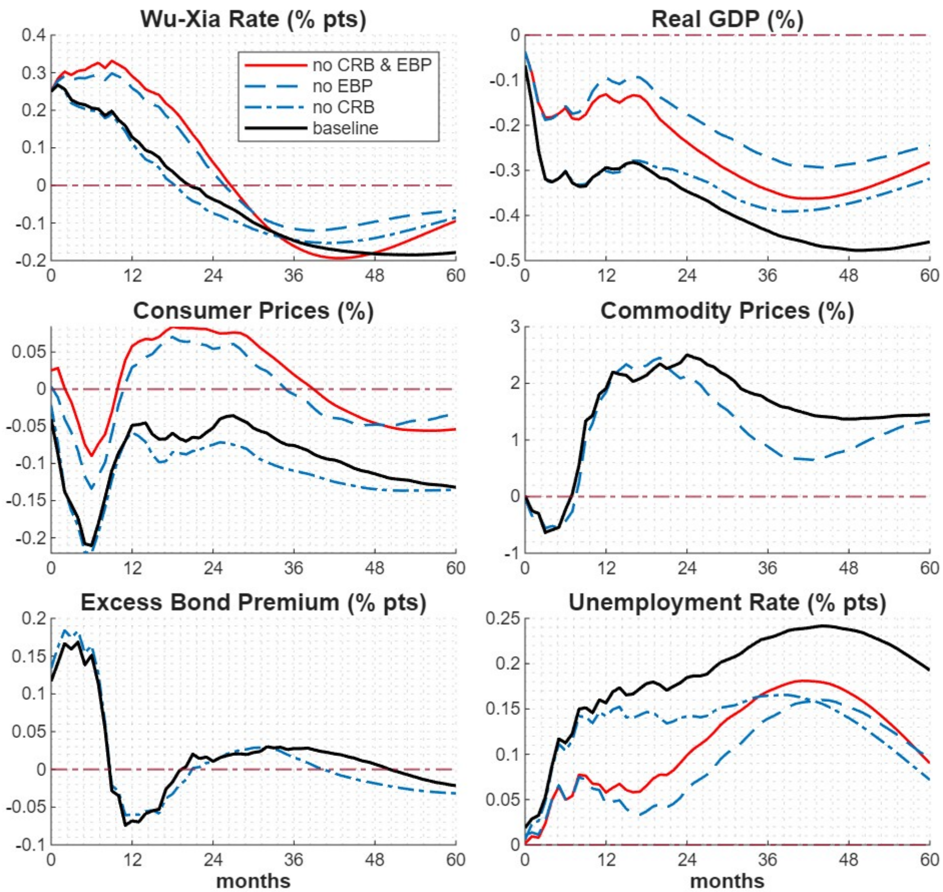
We consider three VAR models for which commodity prices (CRB), the excess bond premium (EBP), or both are omitted and compare them to our baseline SVAR-IV. Figure C.1 shows the notable qualitative differences in the responses from the models for which EBP or both CRB and EBP are omitted, namely resulting in milder movements of GDP and consumer prices, with the latter even increasing a year after the shock.

There are differences in the first-stage F statistics also (heteroskedasticity-consistent version in brackets) associated with the instrument regressions: 14.91 (8.15) for the baseline version, 13.92 (6.9) for the version without CRB, 18.7 (9.36) for the version without EBP, and 18.13 (9.63) for the version excluding both CRB and EBP. If we were to remove any variables, this exercise suggests that we should exclude commodity prices; however, doing so would somewhat increase the risk of instrument irrelevance.

We also consider two VAR models with reduced number of lags, 9 and 6, and compare their respective IRFs to those resulting from our baseline SVAR-IV with 12 lags. In Figure C.2, we similarly see slightly milder responses to consumer prices in the two models with fewer lags, although overall responses appear somewhat aligned.

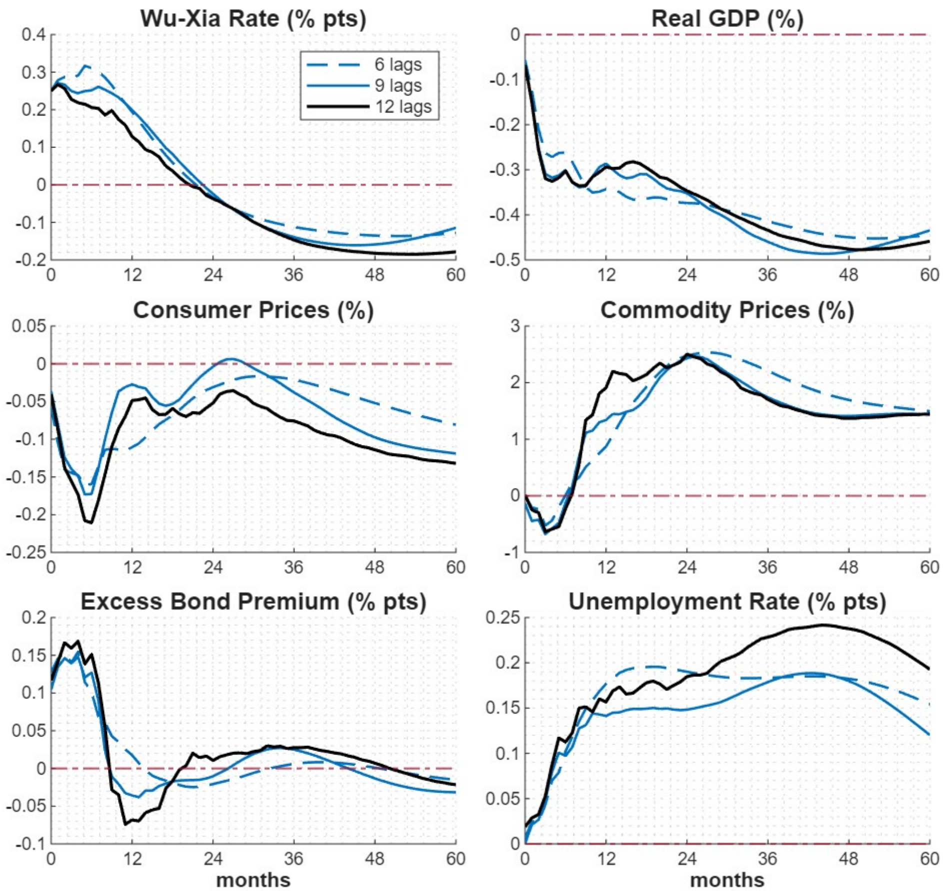
The first-stage F statistics (heteroskedasticity-consistent version in brackets) associated with the first-stage instrument regressions are 14.91 (8.15) for the baseline version with 12 lags, 13.69 (6.93) for the version with 9 lags, and 19.22 (11.61) for the version with 6 lags.

Figure C.1. Monetary Policy Shock Responses Under Different Variable Selections



Notes: Responses to a 25 b.p. contractionary monetary policy shock. VARs estimated over 1992:01-2020:02 with 12 lags. Monetary policy instrument as in Jarociński and Karadi (2020).

Figure C.2. Monetary Policy Shock Responses Under Different Lag Lengths



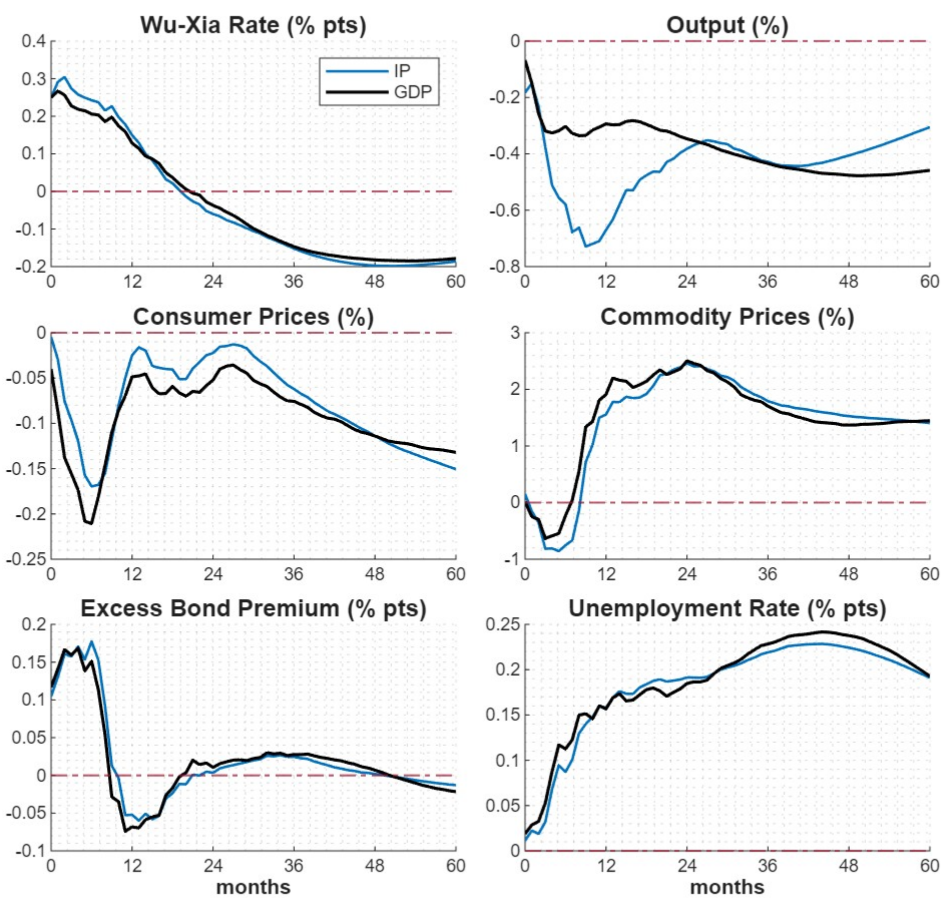
Notes: Responses to a 25 b.p. contractionary monetary policy shock. VARs estimated over 1992:01-2020:02 with varying number of lags. Monetary policy instrument as in Jarociński and Karadi (2020).

Appendix D. Comparing Aggregate Output Measures

We perform a robustness exercise for the aggregate output indicator in the VAR. We estimate two SVAR-IV models with all other specifications as in the baseline version. In the first model, we use the log of the Brave et al. (2019) real GDP and in the second, the log of industrial production (IP). We compare IRFs which are shown in Figure D.1.

We notice that both variables yield close response estimates for the remaining variables in the system. The response of real GDP is milder and greater in the long run, while that of industrial production is sharper in the short run.

Figure D.1. Monetary Policy Shock Responses Under Different Aggregate Output Measures



Notes: Responses to a 25 b.p. contractionary monetary policy shock. VARs estimated over 1992:01-2020:02 with 12 lags. Monetary policy instrument as in Jarociński and Karadi (2020).

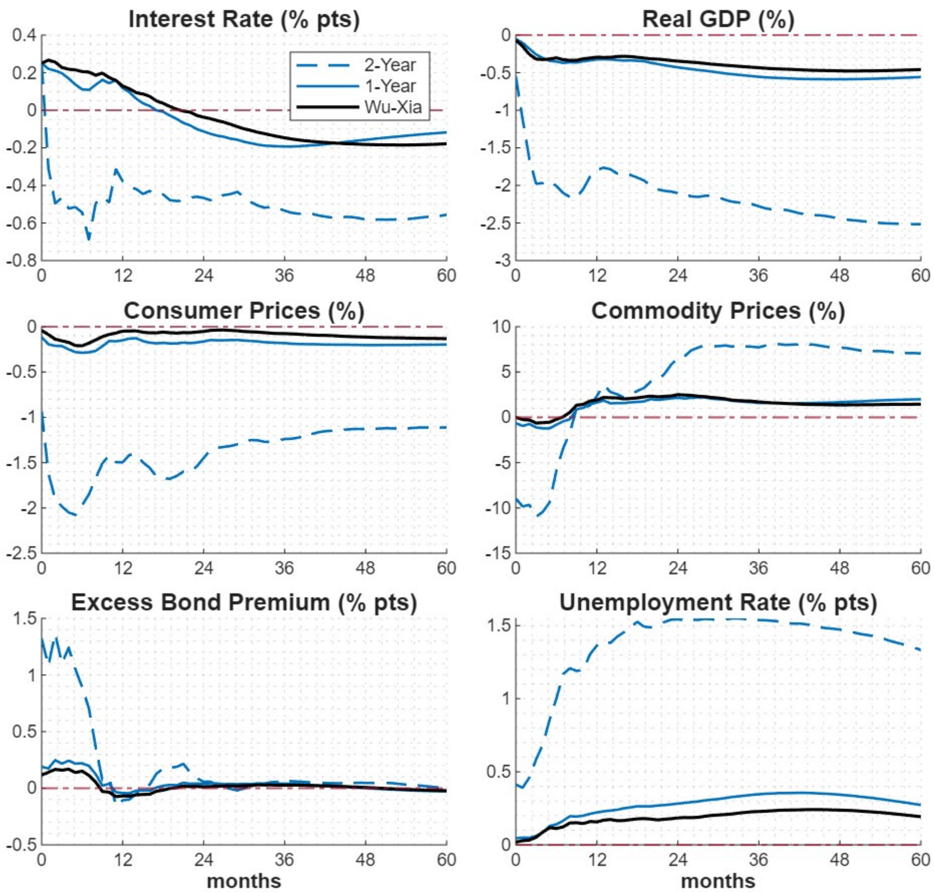
Appendix E. Comparing Monetary Policy Measures

We perform a robustness exercise for the monetary policy measure. We estimate three SVAR-IV models (all other specifications are as in the baseline) using the Wu-Xia rate and the one- and two-year Treasury yields, and compare IRFs (shown in Figure E.1).

We notice that the Wu-Xia and one-year rates produce similar estimates, with the latter realizing somewhat more pronounced responses. Alternatively, when using the two-year rate, our model produces puzzling results. There are also apparent differences in the first-stage F statistics (heteroskedasticity-consistent version in brackets): 14.91 (8.15) when using the Wu-Xia rate, 8.35 (2.89) when using the one-year Treasury yield, and 0.09 (0.05) when using the two-year Treasury yield.

This exercise indicates that the Wu-Xia rate is the monetary policy variable that is most compatible with our external instrument and that the use of short-term Treasury yields (especially the two-year rate) is concerning when we impose sample start date restrictions, and the zero-bound period represents a large portion of the entire sample time frame.

Figure E.1. Monetary Policy Shock Responses Under Different Monetary Policy Measures



Notes: IRFs to a 25 b.p. contractionary monetary policy shock. VARs estimated over 1992:01-2020:02 with 12 lags. Monetary policy instrument as in Jarociński and Karadi (2020).

Appendix F. Comparing External Instruments

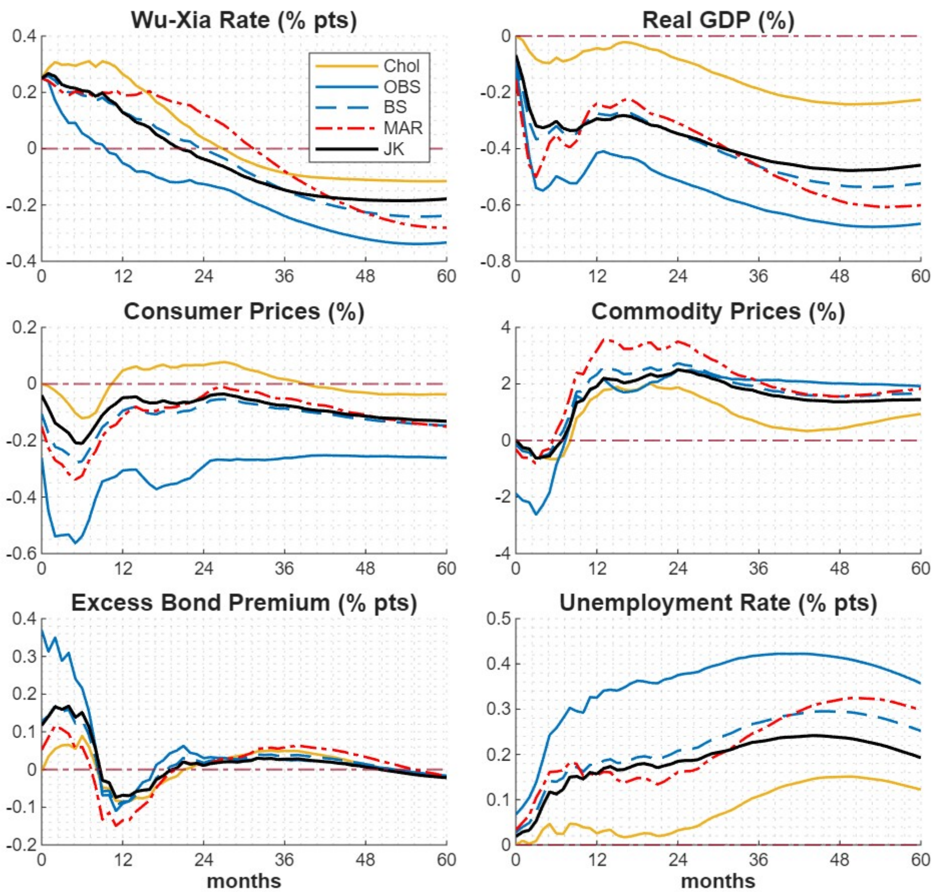
We are presented with four potential external instruments: the instrument used in Jarociński and Karadi (2020) (JK) (series available from 1990:01 to 2016:12); the Miranda-Agrippino and Ricco (2021) instrument (MAR) updated by Degasperi and Ricco (2021) (series available from 1991:01 to 2015:12); the unadjusted Bauer and Swanson (2023) FOMC instrument (BS) (series available from 1988:02 to 2019:12); and the orthogonalized Bauer and Swanson (2023) FOMC instrument (OBS) (series available from 1988:02 to 2019:12). We estimate baseline SVAR-IVs with each instrument along with a VAR identified via a Cholesky decomposition, ordering the Wu-Xia rate first. Figure F.1 presents the IRFs.

For each first-stage regression, we achieve the following F statistic (heteroskedasticity-consistent F statistic in brackets): 14.91 (8.15) for JK, 6.59 (4.35) for MAR, 9.58 (6.54) for BS, and 4.30 (3.01) for OBS. This suggests that JK is most likely to satisfy the relevance condition. Qualitatively, we see that the estimates from the first three instruments are similar, while the Cholesky decomposition estimates – biased towards zero, and the OBS estimates – worrying large. As documented in Bauer and Swanson (2023), OBS tends to exaggerate responses while also being a weak instrument. Based on this comparison, we choose to use the JK series to estimate the responses of racial and ethnic labor market outcome gaps to monetary policy shocks.

We also check how the available instruments compare to the "best practice" IRF estimates of Bauer and Swanson (2023). To do so most precisely, we use their macroeconomic dataset and follow their variable specification. Since here we estimate a reduced-form VAR from January 1973 to February 2020, we take the subset of reduced-form residuals that match the length and time frame of each available instrument to compute the respective two-stage estimation for each instrument.

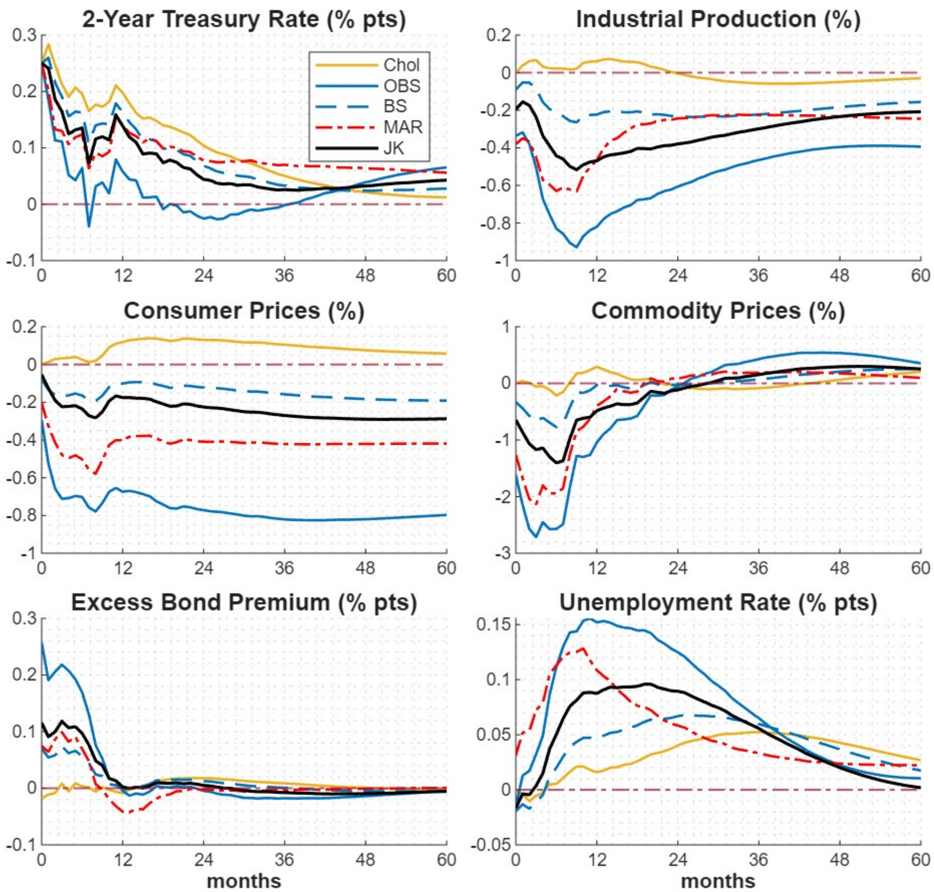
Figure F.2 presents resulting IRFs along with IRFs identified via a Cholesky decomposition with the 2-year Treasury rate ordered first. We realize the following first-stage F statistics (heteroskedasticity-consistent version in brackets): 9.13 (5.64) when using JK, 5.63 (3.71) when using MAR, 9.01 (7.00) when using BS, and 3.35 (2.86) when using OBS. Although each instrument would be considered weak, JK remains most relevant. More importantly, we find that the responses identified with JK most closely resemble the "best practice" estimates reported in Bauer and Swanson (2023) both qualitatively and quantitatively even though their instrument is orthogonalized with respect to macroeconomic and financial data and ours is not. We cannot facilitate this comparison directly because the "best practice" Bauer-Swanson instrument series is not made publicly available – only the FOMC versions are. On the other hand, the Cholesky estimates suggest puzzling increases following a monetary contraction, most notably in consumer prices, and otherwise biased-to-zero responses.

Figure F.1. Monetary Policy Shock Responses Under Different Instruments



Notes: IRFs to a 25 b.p. contractionary monetary policy shock. VARs estimated over 1992:01-2020:02 with 12 lags. Monetary policy instrument varies.

Figure F.2. Monetary Policy Shock Responses Under Different Instruments Using the Bauer and Swanson (2023) Dataset



Notes: IRFs to a 25 b.p. contractionary monetary policy shock. VARs estimated over 1973:01-2020:02 with 12 lags. Monetary policy instrument varies.

Appendix G. Monthly CPS Sample Sizes

Table G.1. Monthly CPS Sample Sizes of Race-Sex-Age Groupings

			white			**Black**			**Hispanic**		
type	sex	age group	min	mean	max	min	mean	max	min	mean	max
labor force	male	16-24	2,081	3,267	5,047	281	416	673	521	743	1,003
		25-44	8,220	11,475	16,503	1,008	1,294	1,668	1,450	2,158	2,665
		45-64	7,927	9,917	11,468	649	907	1,127	480	981	1,522
		65+	869	1,424	2,130	45	93	170	26	82	183
	female	16-24	1,914	3,155	4,768	309	475	687	365	569	739
		25-44	7,317	10,145	14,256	1,287	1,646	2,077	1,060	1,622	2,022
		45-64	6,852	8,890	10,440	780	1,087	1,351	382	808	1,323
		65+	656	1,096	1,719	46	106	201	11	60	141
population	male	16-24	3,632	4,939	6,367	590	817	1,030	763	1,167	1,470
		25-44	9,046	12,331	17,411	1,204	1,524	1,911	1,568	2,335	2,835
		45-64	9,653	12,221	14,099	949	1,314	1,689	616	1,205	1,850
		65+	5,204	6,614	8,277	396	550	782	204	377	669
	female	16-24	3,385	4,927	6,473	624	920	1,198	789	1,125	1,478
		25-44	9,398	12,954	18,294	1,620	2,105	2,776	1,728	2,428	2,912
		45-64	10,178	12,817	14,527	1,268	1,696	2,135	726	1,331	1,988
		65+	6,913	8,531	9,822	682	869	1,159	301	503	859

Notes: The table reports a summary of the monthly group-specific CPS observation counts from Jan 1992 to Feb 2020 inclusive (actual, not weighted). The reported means are rounded to a whole number.

Table G.2. Monthly CPS Sample Sizes of Race-Sex-Age-Education Groupings

				white			Black			Hispanic		
type	sex	age group	education level	min	mean	max	min	mean	max	min	mean	max
labor force	male	25-44	less than HS	258	673	1,286	48	120	270	487	768	1,074
			HS diploma	2,193	3,623	5,939	319	500	722	422	688	868
			some college	2,218	3,199	4,445	291	394	490	256	424	571
			col and higher	3,449	3,980	5,004	192	279	352	152	278	424
		45-64	less than HS	328	629	1,295	55	129	271	201	362	512
			HS diploma	2,406	3,023	3,587	214	330	443	104	274	493
			some college	1,894	2,621	3,153	127	244	353	72	184	311
			col and higher	2,763	3,645	4,282	99	204	299	59	160	312
	female	25-44	less than HS	132	374	803	59	135	257	245	399	531
			HS diploma	1,209	2,668	5,160	320	531	850	315	485	583
			some college	1,966	3,128	4,278	398	573	729	263	428	596
			col and higher	3,261	3,974	4,308	309	406	530	133	311	567
		45-64	less than HS	141	388	839	45	120	231	133	238	335
			HS diploma	1,789	2,797	3,411	272	362	444	113	241	391
			some college	1,746	2,687	3,347	152	331	456	58	187	328
			col and higher	1,625	3,019	3,807	109	274	429	27	143	307
population	male	25-44	less than HS	369	835	1,498	79	180	360	540	836	1,143
			HS diploma	2,489	3,942	6,238	395	601	825	465	747	948
			some college	2,423	3,418	4,634	349	444	544	277	458	621
			col and higher	3,575	4,136	5,152	203	299	383	161	294	448
		45-64	less than HS	581	1,044	1,979	143	253	417	297	466	638
			HS diploma	3,032	3,876	4,585	310	487	688	135	338	590
			some college	2,220	3,188	3,851	151	332	474	83	221	365
			col and higher	3,066	4,112	4,897	115	243	361	69	180	339
	female	25-44	less than HS	271	683	1,428	101	244	499	486	765	972
			HS diploma	1,777	3,592	6,780	465	711	1,136	490	718	887
			some college	2,562	3,956	5,306	491	694	864	345	560	787
			col and higher	3,936	4,723	5,092	341	455	597	165	385	675
		45-64	less than HS	426	948	1,986	154	296	508	356	504	668
			HS diploma	2,973	4,327	5,313	428	588	711	198	380	597
			some college	2,486	3,696	4,496	213	471	671	87	262	468
			col and higher	2,123	3,846	4,930	137	341	534	43	185	387

Notes: The table reports a summary of the monthly group-specific CPS observation counts from Jan 1992 to Feb 2020 inclusive (actual, not weighted). The reported means are rounded to a whole number.

Appendix H. Unit Interval Predictions of LPMs

Table H.2. Unit Interval Predictions for Oaxaca-Blinder Decomposition LPMs

group	sex	obs. count	in-unit count	in-unit %
white	male	8,816,579	8,448,032	95.82
	female	7,870,823	7,830,147	99.48
Black	male	916,281	848,446	92.60
	female	1,120,489	1,066,080	95.14
Hispanic	male	1,340,088	1,311,205	97.84
	female	1,033,759	1,022,574	98.92

Notes: The table reports the number of observations used in each Oaxaca-Blinder-decomposition LPM along with the number of fitted values within the unit interval and their corresponding fraction expressed as a percentage.

Table H.1. Unit Interval Predictions for Marginal Effects LPMs

		Black-white						Hispanic-white					
		male			female			male			female		
age group	education level	obs. count	in-unit count	in-unit %	obs. count	in-unit count	in-unit %	obs. count	in-unit count	in-unit %			
16-24	less than HS	328,763	317,912	96.70	280,282	271,607	96.90	389,430	378,675	97.24	297,097	288,687	97.17
	HS diploma	410,368	403,001	98.20	327,106	323,220	98.81	442,304	434,674	98.27	334,531	330,768	98.88
	some college	400,133	395,244	98.78	466,995	465,180	99.61	416,879	411,780	98.78	475,619	473,752	99.61
	col and higher	105,818	100,312	94.80	152,659	147,201	96.42	106,884	101,192	94.67	151,691	146,302	96.45
25-34	less than HS	128,112	124,110	96.88	79,986	79,152	98.96	244,834	239,412	97.79	119,603	118,430	99.02
	HS diploma	648,091	606,675	93.61	470,328	463,251	98.50	694,610	652,114	93.88	468,382	461,710	98.58
	some college	579,521	534,905	92.30	584,515	571,924	97.85	591,781	546,768	92.39	566,222	554,375	97.91
	col and higher	658,978	618,571	93.87	729,386	723,144	99.14	662,699	622,219	93.89	719,710	713,888	99.19
35-44	less than HS	140,097	133,259	95.12	92,384	90,289	97.73	242,267	233,251	96.28	141,729	139,277	98.27
	HS diploma	745,393	680,651	91.31	610,970	598,145	97.90	762,476	697,903	91.53	597,318	585,621	98.04
	some college	635,053	573,155	90.25	666,675	648,927	97.34	632,830	571,362	90.29	635,691	620,179	97.56
	col and higher	780,681	724,802	92.84	751,154	744,152	99.07	776,653	723,754	93.19	728,481	723,219	99.28
45-54	less than HS	141,428	133,288	94.24	93,955	91,690	97.59	197,799	188,280	95.19	124,341	121,364	97.61
	HS diploma	697,984	630,688	90.36	638,970	626,414	98.03	688,821	623,291	90.49	615,649	604,271	98.15
	some college	602,780	559,697	92.85	640,210	623,722	97.42	590,201	548,685	92.97	610,124	594,763	97.48
	col and higher	781,459	747,063	95.60	704,597	695,605	98.72	772,558	739,019	95.66	676,820	668,521	98.77
55-64	less than HS	114,757	106,562	92.86	77,860	75,804	97.36	137,270	129,001	93.98	87,086	85,381	98.04
	HS diploma	435,386	403,340	92.64	428,632	420,677	98.14	425,621	394,783	92.75	411,034	403,238	98.10
	some college	365,515	351,443	96.15	379,685	369,544	97.33	357,798	344,671	96.33	361,102	351,474	97.33
	col and higher	519,516	505,336	97.27	408,491	402,538	98.54	513,664	500,874	97.51	391,718	385,547	98.42
65+	less than HS	63,416	57,796	91.14	42,228	40,496	95.90	65,679	59,790	91.03	40,696	38,583	94.81
	HS diploma	142,420	132,976	93.37	143,508	140,201	97.70	139,517	130,374	93.45	138,216	134,784	97.52
	some college	111,248	105,564	94.89	110,070	105,616	95.95	109,637	103,686	94.57	105,467	100,719	95.50
	col and higher	195,943	190,818	97.38	110,666	107,033	96.72	194,455	189,568	97.49	106,255	102,648	96.61

Notes: The table reports the number of observations used in each marginal-effects LPM along with the number of fitted values within the unit interval and their corresponding fraction expressed as a percentage.

Appendix I. Marginal Effects LPM Results in Table Form

Table I.1. Racial and Ethnic Differentials in the Marginal Effects of an Economic Contraction on the Probability of Being Unemployed

		Black estimates (% pts)				Hispanic estimates (% pts)			
		male		female		male		female	
16-24	less than HS	0.4729	(0.1510)	0.6406	(0.1517)	0.0013	(0.0834)	0.5904	(0.1067)
	HS diploma	0.4598	(0.1187)	0.3193	(0.1188)	-0.0376	(0.0770)	0.2094	(0.0862)
	some college	0.5910	(0.1129)	0.9622	(0.0928)	0.1800	(0.0750)	0.1912	(0.0637)
	col and higher	0.7953	(0.2530)	0.6284	(0.1807)	0.2012	(0.1723)	0.3879	(0.1438)
25-34	less than HS	1.0607	(0.2274)	0.3327	(0.2256)	-1.0973	(0.0898)	-0.5620	(0.1377)
	HS diploma	0.7915	(0.0876)	0.5856	(0.0853)	0.0247	(0.0526)	0.1063	(0.0666)
	some college	1.0380	(0.0827)	1.1058	(0.0704)	0.0173	(0.0564)	0.0373	(0.0559)
	col and higher	0.5775	(0.0790)	0.5599	(0.0613)	0.2128	(0.0607)	0.0072	(0.0520)
35-44	less than HS	1.0856	(0.2042)	1.0155	(0.2006)	-0.5373	(0.0797)	0.0929	(0.1124)
	HS diploma	0.7689	(0.0773)	0.5394	(0.0737)	-0.0238	(0.0512)	0.0102	(0.0595)
	some college	0.6133	(0.0744)	0.3183	(0.0593)	0.2184	(0.0572)	0.3439	(0.0611)
	col and higher	0.7022	(0.0680)	0.4024	(0.0523)	0.0737	(0.0508)	0.1063	(0.0516)
45-54	less than HS	0.4486	(0.1659)	0.4865	(0.1638)	-0.3641	(0.0852)	0.3146	(0.1062)
	HS diploma	0.4725	(0.0737)	0.3701	(0.0662)	0.1959	(0.0592)	0.1894	(0.0627)
	some college	0.5767	(0.0800)	0.2670	(0.0603)	0.3110	(0.0702)	0.3652	(0.0683)
	col and higher	0.5188	(0.0716)	0.3034	(0.0565)	0.2744	(0.0608)	0.2797	(0.0663)
55-64	less than HS	0.4074	(0.1727)	0.3355	(0.1700)	0.0505	(0.1033)	-0.1385	(0.1154)
	HS diploma	0.5104	(0.0958)	0.3153	(0.0797)	0.3214	(0.0904)	0.4369	(0.0886)
	some college	0.6258	(0.1064)	0.0879	(0.0766)	0.1879	(0.1018)	0.3035	(0.1052)
	col and higher	0.3750	(0.0948)	0.1566	(0.0737)	0.3745	(0.0929)	0.3786	(0.1027)
65+	less than HS	-0.0797	(0.2153)	0.0833	(0.2268)	0.2951	(0.1838)	0.0983	(0.2067)
	HS diploma	0.7739	(0.1812)	-0.1412	(0.1423)	0.2103	(0.1885)	-0.1473	(0.1649)
	some college	0.6889	(0.2476)	0.1356	(0.1820)	-0.1411	(0.1848)	0.3250	(0.2201)
	col and higher	0.4397	(0.2008)	-0.2182	(0.1668)	0.1615	(0.1670)	-0.2505	(0.2060)

Notes: Racial and ethnic differentials in the marginal effects of unemployment probability following a one percentage point increase in the 12-month average state-level unemployment rate. Estimates represent the percentage point difference between Black/Hispanic and white groups. Heteroskedasticity-consistent standard errors in parentheses.