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U.S. Monetary Policy's Distributional Impacts: Evaluating Wealth and Employment Outcomes by Race and Gender

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Abstract

Researchers studying racial and gender inequality have long argued that purportedly "identity-neutral" policies such as monetary policy can contribute to economic disparities. Black and Hispanic men and women are more likely to hold precarious jobs that are particularly vulnerable to declines in economic activity caused by contractionary policy. Rising interest rates may also influence the wealth distribution, although the direction is theoretically ambiguous. Women and minoritized groups in the United States hold less wealth than others, with the median black household owning about 10% of the wealth of the median white household. Interest rate increases can benefit savers via capital income effects, but expansionary policy may also increase housing and stock prices. This project examines how monetary policy impacts wealth and employment inequality by race and gender in the U.S. from the 1980s to 2007 by aggregating household-level data from the PSID and CPS to the state level and merging them with other indicators. The paper applies two novel panel data methodologies recently established in the literature. First, the effects of monetary policy are evaluated by leveraging the differential impacts of federal funds rate changes on economic activity by state. Potential channels for these impacts are then examined using state-level mediator variables. Unemployment rates are found to increase more for black workers than white workers in response to a monetary policy shock, regardless of gender. Wealth impacts are found to be smaller for black households in absolute terms, but these gaps disappear when measured relative to average wealth. Preliminary results suggest that these labor market and wealth effects hold when the time period is extended to include unconventional monetary policy as measured by the Wu-Xia shadow federal funds rate.

JEL Classification Codes: E5 - Monetary Policy, Central Banking, and the Supply of Money and Credit

1 Introduction

Disparities in economic outcomes by race and gender in the United States persist and have been highlighted during the COVID-19 pandemic.[1] Black and Hispanic women experienced some of the greatest increases in unemployment rates in April 2020. The closure of schools and childcare centers the following fall prompted hundreds of thousands of additional women to leave the labor force entirely. Meanwhile, black households hold less than 15% of the average wealth of white households (Bhutta et al. 2020), gaps that may have worsened since 2020 (Kartashova and Zhou 2021).

Policies and institutions that appear to be "identity neutral" on their surface may interact with existing inequities to worsen or ameliorate them. Between the 1980s and 2000s, U.S. monetary policy became one subject of such arguments as researchers reflected on a combination of monetary tightening, contractions in economic activity, and black unemployment rates that reached as high as 20% (Abell 1991; Carpenter and Rodgers 2004; Hull 1983; Thorbecke 2001; Zavodny and Zha 2000). The current moment renews some of these questions. Federal Reserve Chairman Jay Powell has warned that the interest rate hikes necessary to quell high inflation in 2022 would bring "pain to households and businesses" (Cox 2022). There has been little discussion of the distributional of those costs, although at least one U.S. Federal Reserve Bank president has recently suggested that the central bank should address racial inequality as part of its mandate (Bostic 2020).

In this paper, I examine whether monetary policy has distributional impacts on employment and wealth by gender and race in the United States and explore mediators that may explain these impacts, with a focus on the outcomes of black and white men and women. Most of the existing empirical literature in this area focuses on the distribution of employment impacts, and the results are mixed. Several studies using U.S. data between the 1970s and 2008 find that black workers experience greater employment losses due to an increase in the policy rate than white workers, with symmetric effects for expansionary policy (Carpenter and Rodgers 2004; Hull 1983; Seguino and Heintz 2012; Thorbecke 2001). Other research finds racial differentials in absolute but not relative terms (Zavodny and Zha 2000) or economically insignificant differentials (Bartscher, Kuhn, Schularick, and Wachtel 2022). Only one study empirically examines the impacts of U.S. monetary policy on racial wealth inequality (Bartscher et al. 2022).

[1]The author gratefully acknowledges the helpful feedback of attendees of the 2022 Northeast Ohio Economics workshop, the 2023 ASE sessions at the Allied Social Science Associations annual meeting, and the 2023 Eastern Economic Association annual meeting. Daniel Cooper, María José Luengo-Prado, and Giovanni P. Olivei generously shared data and code. All remaining errors are the responsibility of the author.

One contribution of this paper is thus to expand on the scarce literature regarding the impacts of monetary policy on the wealth distribution. Bartscher et al.'s (2022) analysis studies only one potential channel connecting changes in the policy rate to wealth inequality. It therefore does not shed light on the net effect of policy changes on the racial wealth gap. This paper is the first to use household-level wealth data to examine the net impacts of monetary policy. I aggregate data from successive waves of the Panel Study of Income Dynamics (PSID) to the state-race-gender-year level between 1984 and 2007. Using various absolute and relative measures of wealth, I test for differential impacts of monetary policy shocks on wealth holdings for each group.

This paper also expands upon the methodologies used in previous work. The papers cited above largely use time series data and VAR techniques. However, impulse response results from VARs are known to be sensitive to the choice of variable ordering. Because monetary policy is set in response to prevailing economic conditions and shapes expectations about the future, changes in a direct measure of monetary policy such as the federal funds rate is likely to be endogenous to future outcomes. Other forms of time series analysis that may be able to identify exogenous changes in the federal funds rates still suffer from limited degrees of freedom. This limitation becomes more significant when data are not available at a monthly or quarterly frequency, which is often the case for race- and gender-disaggregated wealth and labor market data.

To address these limitations, I adopt novel developments in the use of panel data in research on monetary policy. This approach builds on Seguino and Heintz (2012), who develop a panel of U.S. states between 1979 and 2007 to identify how changes in the federal funds rate affect unemployment ratios by race and gender. They use a two-stage approach to account for state idiosyncrasies in economic activity and incorporate a range of covariates. Because endogeneity concerns regarding the federal funds rate and its relationship to national economic conditions may remain, I apply the method recently developed by Cooper, Luengo-Prado, and Olivei (2022) in their study of monetary policy's impacts on housing prices. They develop a state-year measure of relative monetary policy stance that allows them to also incorporate state and year fixed effects in their analysis.

The key identifying assumption is that monetary policy has differential effects by state, an assumption that is widely supported in the literature. The first-stage regression yields an estimated gap between 1) the equilibrium real rate of interest necessary to bring state i to full employment within two years of time t and 2) state i's observed real federal funds rate (using a state-specific inflation rate) in year t. This gap is in turn used to determine the impact of relatively loose or tight monetary policy on outcomes in a particular state and year. Bootstrapping is used to account for the uncertainty of the estimated state interest rate gaps. I apply this methodology to estimate first-order effects of monetary policy on wealth and employment outcomes by race and gender.

I also adopt a second methodology focused on identifying the mediators of first-order effects. Within the limited literature on monetary policy's distributional impacts, few have empirically investigated potential channels for these impacts. Yet panel data offers a powerful tool for this purpose by avoiding some of the endogeneity concerns associated with changes in monetary policy (Leahy and Thapar 2022). I use this approach to examine mediators including the state's industrial composition, the black share of the population (as investigated by Seguino and Heintz 2012), and gaps in financial inclusion.

The results indicate that contractionary monetary policy shocks disproportionately increase the unemployment rate for black men and women in the U.S. These gaps in absolute terms are economically significant and robust to the inclusion of linear time trends, bootstrapping standard errors, and a range of aggregation rules. A one percentage point increase in the relative monetary policy stance is associated with a 0.6 to 0.8 percentage point larger increase in the unemployment rate for black men and women than white men and women. These differentials are robust to the bootstrapping of standard errors, the addition of linear time trends, and alternative cutoff thresholds when aggregating micro data to the state level. The mediator analysis suggests that these effects decline with a larger black population share in the state or a smaller manufacturing or construction employment share. The two mediators are consistent with job competition effects, such that discrimination heightens in areas where labor markets are more sensitive to interest rate changes.

The results for wealth are mixed. In absolute terms, they suggest that portfolio effects may be dominant. Namely, a one percentage point increase in the policy rate is associated with declines in the value of average wealth holdings among white men and dual-headed households of about \$33000 (or a decline in median wealth of about \$8000). The absolute wealth losses are 75% to 90% smaller for black male and dual-headed households and female-headed households regardless of race, depending on the wealth measure used. In logged terms, however, wealth losses are indistinguishable across groups or slightly greater for black female-headed households. Logged wealth shifts focus away from outliers in the wealth distribution and identifies relative effects, suggesting that smaller absolute losses in wealth for may still represent equally large or larger losses given the lower average wealth of black and female-headed households. Preliminary results extending the time series to include periods of unconventional monetary policy, measured using the Wu-Xia (2016) shadow rate series, suggest that the main unemployment and wealth effects are robust to including the post-2008 period of quantitative easing.

The paper begins with a discussion of the theoretical channels by which monetary policy may be expected to influence labor market and wealth outcomes, drawing on the monetary policy literature as well as work from stratification economics. Section 3 describes the econometric model and identification strategy. Data and aggregation methods are presented in Section 4. Sections 5 and 6 present results, and robustness checks and an extension to the post-2008 period follow. The concluding section reflects on the implications for policymakers and unanswered questions.

2 Theoretical Channels and Relevant Literature

A change in the Federal Funds Rate could differentially impact women and black workers via multiple channels. However, the implied impacts (greater or lesser sensitivity) are ambiguous *a priori* for both labor market and wealth outcomes. Since 1980, unemployment rates have tended to be about 1.5 to 2.5 times higher for Black and Hispanic workers in the U.S., while they have become roughly equal between men and women. Yet these gaps may not remain constant in response to changes in policy and labor markets. Black and Hispanic women are more likely to hold precarious jobs, for instance. These jobs may be defined as ones that are "uncertain, unprotected, or economically insecure"; they tend to be vulnerable during economic contractions and to be expanded as firms increase production capacity during expansions (Albelda, Bell-Pasht, and Konstantinidis 2020). On the other hand, racial minorities and women are underrepresented in interest-rate-sensitive industries such as manufacturing (Takhtamanova and Sierminska 2009).

Unemployment rates are likely to understate the true extent of labor force disruption for women, however. Extended unemployment or underemployment can encourage workers to leave the labor force (via the discouraged worker effect) while also encouraging other members of the household to join the labor force as a way of making up for lost income (the added worker effect). Since women are more likely to be secondary earners in U.S. households, their labor force participation rate is likely to be more sensitive to changes in unemployment rates, which may lead the rate for women to change for reasons not directly related to job gains or losses. The empirical model of unemployment rates described in the following section includes a control for labor force participation to account for this measurement problem, following Seguino and Heintz (2012).

Discrimination by race and gender may also heighten as the pool of "good jobs" shrinks with contractionary policy. Neoclassical models of discrimination suggest that discrimination is a function of either idiosyncratic biases or beliefs regarding the average productivity of specific groups (Becker 1957). The stratification and feminist economics literatures have framed discrimination differently. They argue that discrimination is a form of competition that can occur both within and outside of markets as individuals invest in advantageous group identities (Chelwa, Hamilton, and Stewart 2022; Seguino and Heintz 2012). In periods where good jobs are plentiful, discrimination again black men or women is likely to diminish and conversely when unemployment rates rise. The extent to which discrimination can be used by a dominant group in this way depends in part on the size and power of the subaltern group. When the group is large enough, discrimination may be infeasible or costly (e.g., firms may forego too many highly qualified employees) (Dymski and Aldana 2014). Therefore, the black population share may be a mediator of the extent to which interest rate increases heighten discrimination against black candidates.

Financial exclusion has not been widely addressed as a channel by which interest rates may have differential impacts by race and gender. One critical transmission mechanism for monetary policy is via lowering borrowing costs for firms, entrepreneurs, and consumers. However, it is well documented that there are fewer bank branches in communities of color, and black and Hispanic households are more than twice as likely to be unbanked as white households (Federal Deposit Insurance Corporation [FDIC] 2020). With less access to bank loans, any change in the short-term interest rate is likely to have less of an impact in these communities. In terms of wealth, the effects are even more direct. Households without bank accounts save less on average (Fitzpatrick 2013) and are unable to benefit directly from increased rates of return on deposits.

Other transmission mechanisms beyond financial exclusion connect monetary policy to households' net worth. Female-headed households and minorities in the United States tend to have less wealth and hold more debt relative to income than other groups, with these gaps increasing during downturns (Long 2018; Pfeffer et al. 2013; Schmidt and Sevak 2006; Szymborska 2022). The median black household owns only 10% to 15% of the wealth of the median white household (Bhutta et al. 2020; Darity et al. 2018). Although most wealth data is only available at the household level, women in opposite-sex couples are less likely to hold their own bank account when they have less bargaining power, suggesting that some groups of women may have less access to household assets if the household were to dissolve

(Klawitter and Fletschner 2011).

The relationship between these net worth gaps and monetary policy is ambiguous. As noted above, interest rate increases as part of contractionary policy benefit savers and harm debtors. These changes in capital income and debt servicing costs could indirectly widen existing wealth gaps. At the same time, expansionary policy increases the value of some assets, including housing and stocks, which are a greater share of wealth for white and dual-headed households than others (Bartscher et al. 2022). Quantitative easing and the purchase of corporate bonds during the COVID-19 pandemic may have had even greater direct impacts on equity prices (Young 2018). However, it is unclear whether contractionary policy will symmetrically reduce wealth gaps as a result. Petach and Tavani (2020) note that black households tend to experience lower returns to wealth than white households, even after controlling for portfolio composition, income, time preferences, or financial decision-making. Higher wealth households, including white households, may be better able to mitigate wealth losses due to declining asset prices by shifting towards alternative debt holdings. Moreover, larger absolute changes in wealth may not equate to larger relative changes. During the 2007 crisis, low-wealth and minoritized households experienced smaller wealth declines in absolute terms, but larger declines in relative terms, leading to a widening of wealth inequality (Pfeffer et al. 2013).

While there is a broader literature on monetary policy's impacts on the income distribution broadly (see Kappes 2022), a smaller set of papers has empirically examined monetary policy's role in racial and gender stratification. Beginning in the 1980s, a series of studies used U.S. time series data to investigate whether widening gaps in unemployment rates between black and white workers could be explained in part by monetary tightening. These papers generally found evidence for this hypothesis using descriptive statistics (Hull 1984) and VAR methods (Carpenter and Rodgers 2004; Thorbecke 2001). One dissenting paper applying a Bayesian VAR model found that the impacts of monetary policy were greater in absolute but not relative terms for black workers (Zavodny and Zha 2000), and Abell (1991) found heterogeneous effects across gender within race. Takhtamanova and Sierminska (2009) report no differential effects by gender alone using a broader sample of high-income economies.

Most recently, Bartscher, Kuhn, Schularick, and Wachtel (2022) use instrumental variable local projection to study whether monetary policy shocks affect the racial distribution of income and wealth. The authors use the extended Romer and Romer (2004) series as applied in Coibion et al. (2017) to instrument for changes in the nominal federal funds rate and estimate the response of asset prices and interest rates to monetary policy shocks. They then derive the resulting changes in net worth for black- and white-headed households based on average portfolio composition, using data from the 2019 Survey of Consumer Finances. The results suggest that contractionary policy decreases employment and thus earnings more for black households than white households, but the authors describe the effect as relatively small. They find that expansionary monetary policy shocks cause an economically significant increase in the racial wealth gap via their impact on stocks and housing values. As noted above, however, this approach only addresses the expected contribution of the portfolio composition channel and leaves the net effects of monetary policy on the wealth distribution as an open question.

In this literature, Seguino and Heintz (2012) applied a panel data approach most similar to the methods used in this paper. They aggregate micro data on employment outcomes from the outgoing rotation group of the Current Population Survey (CPS) to the state level to estimate ratios of unemployment rates by race and gender for state i in year t. They merge these ratios with standard state macroeconomic data series to develop a panel dataset spanning from 1979 to 2008 and conduct a two-stage analysis. In the first stage, they estimate state-level economic growth as a function of national measures of economic activity and state fixed effects. The residuals and fixed effects of this regression are in turn used to identify idiosyncratic variation in economic activity by state. This estimated measure is in turn used as a control variable in the main regression. Their results indicate that contractionary policy exacerbates existing unemployment rate gaps and may be mediated by the black share of the state population. This methodology does not propose an exogenous measure of monetary policy but argues that monetary policy changes are unlikely to be endogenous to changes in the ratio of unemployment rates as opposed to changes in aggregate unemployment rates. This project build on Seguino and Heintz's (2012) work by incorporating a wealth analysis, identifying monetary policy shocks using panel data techniques, and extending the time period to include periods of unconventional monetary policy.

3 Methods

As a baseline for comparison, the relationship between shifts in monetary policy and state-level outcomes is first modeled using the following fixed effects regression

$$y_{ijt} = \alpha_i + \gamma_j + \theta_j r_{t-1} + \boldsymbol{\beta X_{ijt}} + \epsilon_{ijt} \tag{1}$$

for gender/race group j in state i in year t, where y_{ijt} is the outcome of interest (the unemployment rate or measure of net worth), r_{t-1} is the nominal federal funds rate (in percentage points), and $\boldsymbol{X_{ijt}}$ is a set of additional covariates. Errors are clustered at the state-group level. The groups included in this analysis are black women, black men, white women, and white men. The coefficient of interest is θ_j, as it indicates how much more or less responsive the outcome variable is to a shift in monetary policy for a specific group.

Because monetary policy is set at the national level and is thus state-invariant, time fixed effects cannot be identified separately from the federal funds rate. However, monetary policy is set in response to past, current, and expected future economic conditions and often includes so-called "forward guidance," wherein the central bank seeks to establish expectations for future rate hikes. To the extent that race and gender differentials in state outcomes are correlated to national trends, the federal funds rate will be endogenous to existing and future economic conditions and the θ_j coefficients will be biased.

To address this concern, I apply a version of the methodology developed by Cooper, Luengo-Prado, and Olivei (2022) in their study of monetary policy's impacts on housing prices. Their approach leverages the well-documented finding that states vary in their sensitivity to monetary policy changes as a potential source of variability in monetary policy that is conditionally exogenous to current and future economic conditions.

First, I derive an equilibrium interest rate for each state by estimating the state-level IS curve

$$u_{it} = \phi_i + \zeta_t + \lambda_{1i} u_{it-1} + \lambda_{2i} u_{it-2} + \nu_i r_{it-1} + \epsilon_{it} \tag{2}$$

for state i in year t where u_{it} is the gap between the unemployment rate in year t and the rate in 1995/96 (a period of generally low unemployment rates), and r_{it-1} is a short-term interest rate measure. The time- and state-varying short-term interest rate used here is the real federal funds rate, calculated as the nominal federal funds rate minus a smoothed measure of state-level inflation. Following Cooper et al. (2022), the inflation rate is calculated by dividing the BEA's annual real and nominal Gross State Product figures to derive a GSP deflator for each state and year. To reduce the noisiness of the GSP deflator-based inflation measure, a fitted inflation measure is derived using national core CPI inflation and GSP growth in each state and year relative to national growth. Details on the estimation of the smoothed state-level inflation measure can be found in Appendix A, and the sources of the state and national time series used to estimate Equation 2 are presented in Section 4.

Separate interest rate coefficients (ν_i) and lagged unemployment rate coefficients (λ_{1i} and λ_{2i}) are estimated for each state, with shared time fixed effects. These coefficients and their standard errors are presented in Figure 1 by state and summarized in Table 1. The interest rate coefficient indicates that a one percentage-point increase in the federal funds rate is associated with an increase in the unemployment rate gap of 0.333 percentage points (where the average unemployment rate gap is 0.71 percentage points), but the effect ranges from 0.209 to 0.527 across states. An F-test of joint significance rejects the null hypothesis of equivalent interest rate sensitivities across states ($p = 0.000$).[2]

Table 1: Estimated state-level IS curve parameters

	mean/sd	min	max	count
Interest rate coefficient	0.333 (0.0648)	0.209	0.527	51
Unemployment rate gap t-1 coefficient	0.977 (0.145)	0.584	1.345	51
Unemployment rate gap t-2 coefficient	-0.255 (0.156)	-0.520	0.219	51
Total unemployment rate gap effect	0.722 (0.1000)	0.504	0.935	51
State fixed effect coefficient	0.0871 (0.206)	-0.624	0.380	51
Two-year interest rate effect	0.992 (0.208)	0.596	1.659	51

Notes: Coefficients are estimated by regressing the unemployment rate gap in state i in year t on two lags of the unemployment rate gap, the smoothed real federal funds rate, and state and year fixed effects. All coefficients other than the state and year fixed effects are allowed to vary by state.

Cooper, Luengo-Prado, and Olivei (2022) show that, by iterating the IS curve equation forward two periods, the rate of interest that will close the unemployment gap in two years r^*_{it} can be calculated as

[2] When the F-test is limited to the subset of 41 states used in the unemployment rate analysis with an aggregation threshold of N = 5 (see Section 4) or the smaller subset of 19 states used for the wealth analysis, the null is again rejected with $p = 0.001$ and $p = 0.0861$, respectively.

$$
\begin{aligned}
r_{it}^{*} = & - \left[(\lambda_{1i}^{2} + \lambda_{2i}^{2})u_{it} + \lambda_{1i}\lambda_{2i}u_{it-1}\right] \times \left(\frac{1}{\lambda_{1i}\nu_i + \nu_i}\right) \\
& - (1 + \lambda_{1i})\phi_i \times \left(\frac{1}{\lambda_{1i}\nu_i + \nu_i}\right)
\end{aligned}
$$

The average equilibrium interest rate is -0.685 percent. Figure 2 illustrates how this average varies over time compared with aggregate real GDP growth, generally rising during expansions and falling during contractions as expected. The equilibrium interest rate is in turn used to calculate a state- and time-varying measure of relative monetary policy stance. This rate gap $\tilde{r}_{it}$ is calculated as the difference between the real federal rate r_{it} and the equilibrium interest rate needed to close the unemployment gap in two years r_{it}^{*}. A one-point increase in this gap can be interpreted similarly to a one-point increase in the federal funds rate, as it indicates a situation where the real federal funds rate is one percentage point further away from the rate that would be needed to restore full employment in state i at time t—i.e., policy that is relatively contractionary for that state.

The model estimated in this paper is the same as Equation 1 but with time fixed effects added:

$$
y_{ijt} = \alpha_i + \gamma_j + \eta_t + \theta_j r_{it-1}^{*} + \boldsymbol{\beta X_{ijt}} + \epsilon_{ijt} \tag{3}
$$

where the controls $\boldsymbol{X_{ijt}}$ include the lag of each outcome y_{ijt}, the current and lagged rate of real GSP growth for state i, and (for the unemployment rate analysis) the current period labor force participation rate for state i and group j. As Cooper et al. (2022) note, the rate gap r_{it-1}^{*} is a function of differences in time-invariant state economic conditions, state-invariant national economic conditions, and time- and state-varying conditions that include idiosyncratic differences in state responsiveness to monetary policy. The first two–including responses to rate changes that are common across states–will be captured by the state and time fixed effects, respectively. As a result, the rate gap variable is arguably exogenous to the prevailing conditions that shape monetary policy and responses to policy that are common nationally. Because the controls include two measures of past and current economic conditions in state i and year t, the only time- and state-varying conditions that could be identifying the impacts of monetary policy are those that are exogenous to state GSP growth and lagged outcomes. For example, variation in the industrial composition of states could influence the responsiveness of each state to rate changes.

These same time- and state-varying factors may also impact the *distribution* of impacts by race and gender. Such potential mediators are explored in the third and final part of

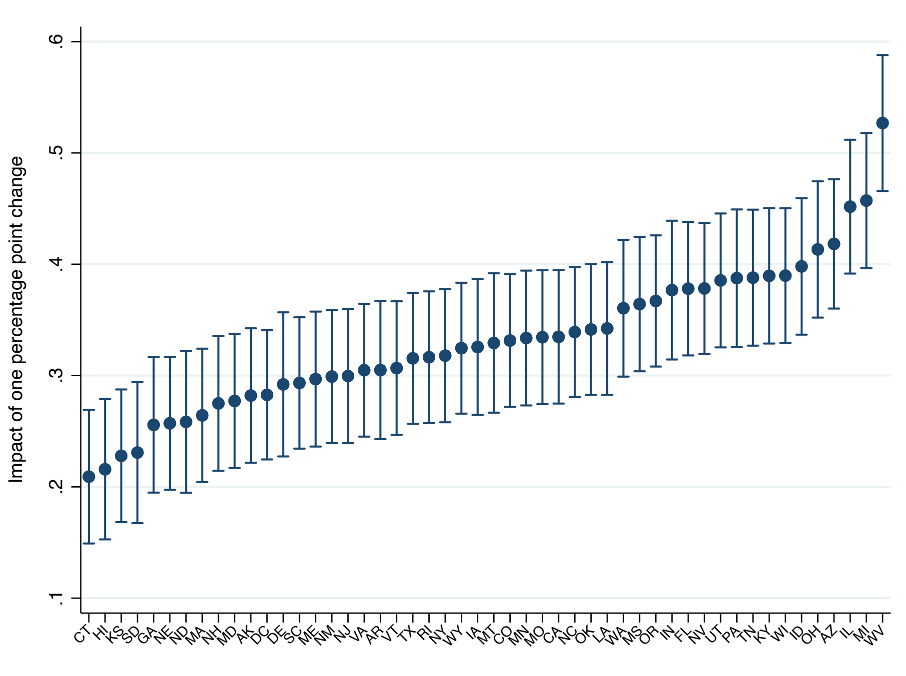


Figure 1: Estimated interest rate effects by state

Notes: Bars indicate standard errors.

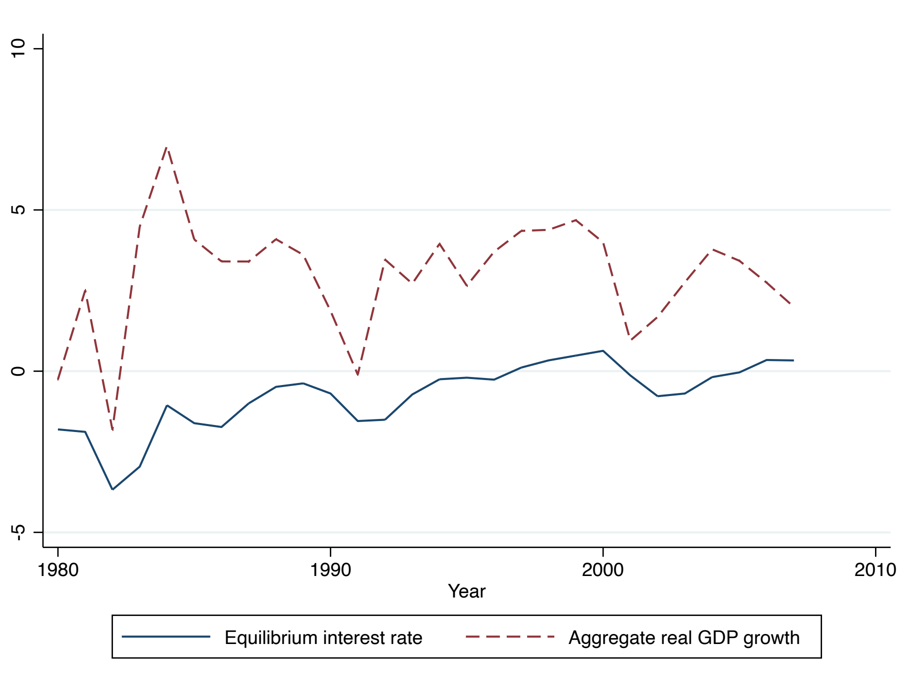


Figure 2: Aggregate real GDP growth and average state equilibrium interest rate over time

Notes: The equilibrium interest rate is calculated from the derived state-level IS curve coefficients as described in Cooper, Luengo-Prado, and Olivei (2022). Yearly averages are unweighted.

the analysis. Mediators are only considered for the unemployment rate outcome due to the relatively limited degrees of freedom for wealth outcomes. This analysis draws on the method recently applied by Leahy and Thapar (2022) in their study of whether monetary policy's impacts on a particular state is impacted by the age distribution in that state. They compare a panel of U.S. states to a panel of different economies. Where a country-level analysis must account for variability in monetary policy rules, financial systems, and other institutions, a state-level analysis benefits from each state being subject to a shared monetary policy. As in the baseline case, estimates of the common impacts of monetary policy will likely be biased. However, because monetary policy is not set in expectation of how different states will respond to a monetary policy shock, the same endogeneity concern is unlikely to hold when the coefficient of interest is heterogeneity in monetary policy impacts.

The mediator analysis model is

$$u_{ijt} = \alpha_i + \gamma_j + \eta_t + \theta_j r_{it-1} + \xi_j z_{it-1} + \omega_j z_{it-1} r_{it-1} + \boldsymbol{\beta X_{itj}} + \epsilon_{ijt}$$

where u_{ijt} is the unemployment rate, r_{t-1} is the measure of the policy rate (the rate gap $\tilde{r}_{it}$), and z_{it} = one of three mediator variables. I follow Leahy and Thapar (2022) in allowing time fixed effects (η_t) to absorb the shared effects of monetary policy across states. The coefficients of interest are ω_j, indicating how the race/gender differences in the impact of a rate change on unemployment rates vary for a one-unit increase in the mediator variable. Three mediator variables are considered and are discussed in more detail in Section 6.

4 Data

Most series are standard state- and national-level data from the BEA (GSP, Core PCE Index, U.S. GDP growth, state unemployment rates, industry employment shares) and Federal Reserve Board of Governors (nominal federal funds rate), averaged to an annual frequency and covering the time period 1980 to 2007 unless noted otherwise. Washington D.C. is included among the states for all analyses.

Data on unemployment rates and wealth at the state-race-gender-year level are aggregated from microdata. Following Seguino and Heintz (2012), I use the CPS Annual Social and Economic Supplement for employment outcomes. Four race-gender categories are identified: black men, black women, white men, and white women. Together, these categories account for approximately 150,000 individual-level observations per year between 1980 and 2007. The employment status for adults, race and gender identifiers, and the state of residence are used to calculate labor force participation and unemployment rates for each race-gender category in a particular state and year.

For wealth outcomes, I similarly aggregate microdata on household wealth to the state level. There are three main sources for wealth data for the United States: the Panel Study of Income Dynamics, the Survey of Income and Program Participation, and the Survey of Consumer Finances. Each survey has strengths and weaknesses for the study of household wealth. The SCF offers greater detail on the types of assets held and has been found to be of higher quality than the SIPP's wealth data, but also tends to oversample very high wealth households. Conversely, SIPP oversamples low-income households (Eggleston and Klee 2015). Finally, while both the SCF and SIPP extend from the 1980s to 2007, these surveys are administered less frequently than the PSID after 1999 (every three years for the SCF and every three to five years after 1996 for the SIPP). Given these considerations, I use the PSID for wealth data, combining all available surveys beginning in 1980 (1984, 1989, 1994, 1999, 2001, 2003, 2005, and 2007).

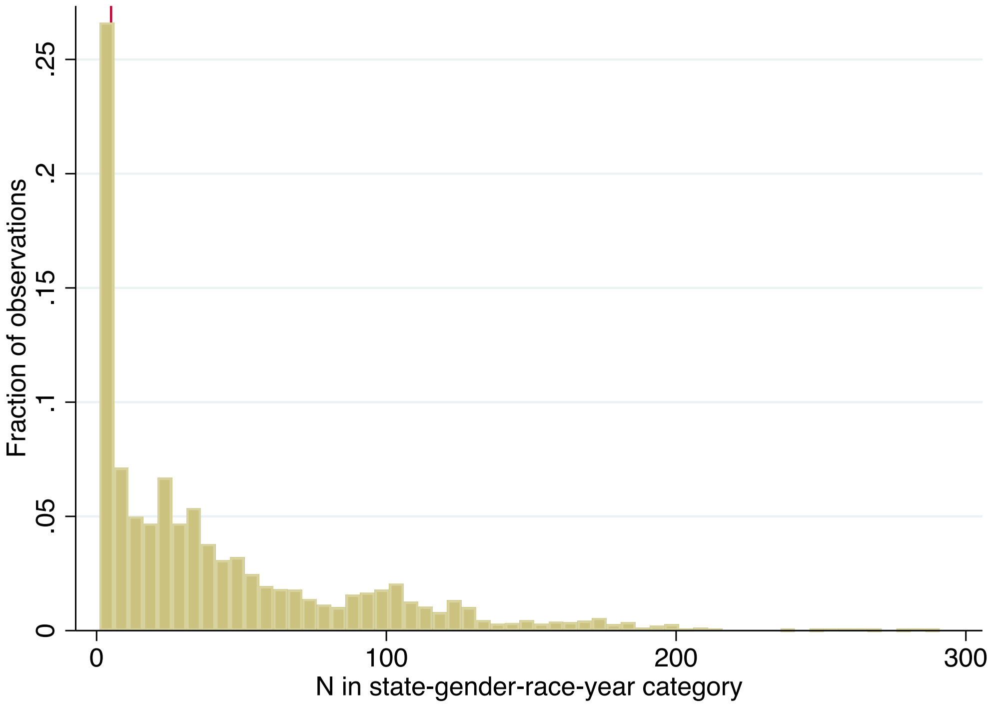
The PSID data are aggregated to the same state-race-gender categories as the CPS data. Wealth offers two challenges for measurement. First, wealth distributions are heavily skewed to the right. High-wealth outliers therefore have a strong influence on sample averages and multivariate regression. Second, the natural log transformation that would account for this high-variance distribution cannot be applied to zero or negative values. Yet zero and negative net worth are common, and the prevalence of those values varies systematically with race (Friedline, Masa, and Chowa 2015). To aggregate wealth data in light of these limitations, I calculate both average and median wealth for each state-race-gender-year observation. The resulting observations include no observations with negative or zero values. As a result, a natural log transformation is applied to the wealth averages, providing a third, relative measure of wealth.

However, wealth is only reported at the household level. Race and gender are therefore reported for the household head. The PSID systematically links household head status with gender rather than defining household head in terms of financial decision-making or another criterion. All households headed by an opposite-sex couple automatically have a male head of household. With these constraints in mind, I combine gender/household structure categories in the wealth analysis such that "women" are "female-headed households" and "men" are "dual- or male-headed households." "Household" here may refer to either families or to individuals or couples living alone, without other family members. This choice is motivated by two observations. First, male-headed households are relatively rare in the US, particularly among households with children: According to 2007 Census data, 50.8% of households were headed by married couples, 4.4% were male-headed families, 12.4% were female-headed families, 14.9% were men living alone, and 17.5% were women living alone. Separating out male-headed households would pose problems of sample size. Second, work on gendered wealth gaps in the U.S. suggest that these gaps are driven by the lower wealth holdings of single-headed households with children. Combining 1) men living alone with male- with dual-headed families and 2) women living alone with female-headed families therefore balances a focus on gender alone with a focus on gendered caregiving responsibilities.

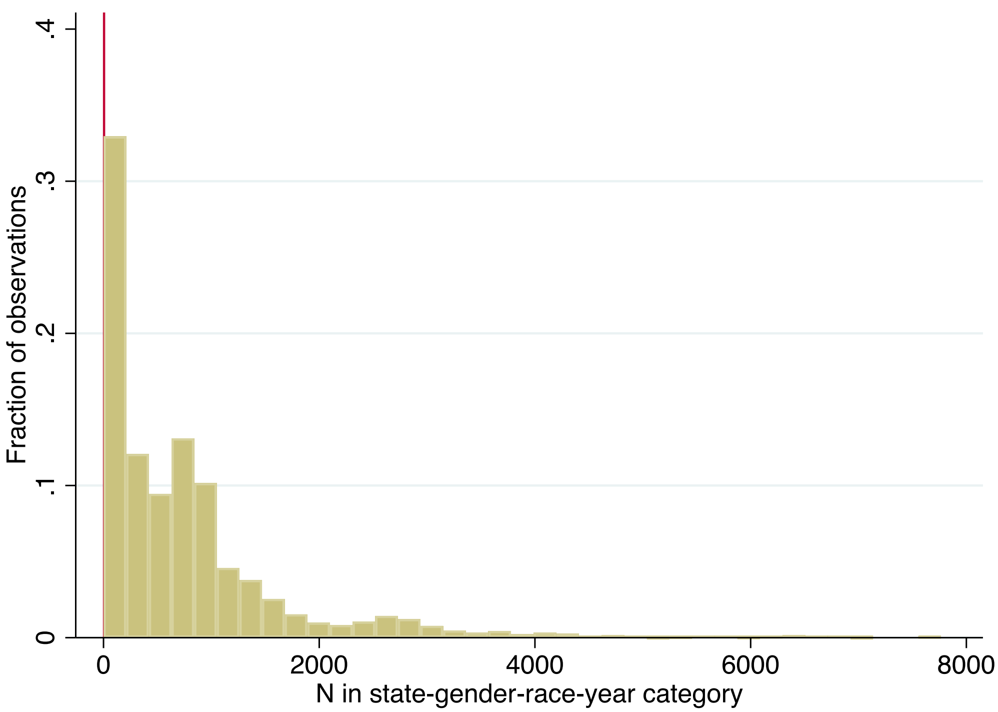
Whereas the CPS ASEC is administered every year, the PSID is administered less frequently: every five years before 1999 (1984, 1989, and 1994) and every two years thereafter. In addition, the PSID sample is substantially smaller than the CPS, with only about 7000 households observed in each year. These limitations to sample size have an important implication for aggregating to the state level. Many state-gender-race-year observations do not exist because no households from a certain state-gender-race category were observed in a given year. This is a larger issue in the PSID, where 18.7% of the potential possible panel observations (51 entities $\times$ years observed $\times$ four race/gender categories) are missing, compared to the CPS (2.5%). Among those observations that do exist, the number of households used to produce the aggregated values vary widely, as shown in Figure 3a for the PSID and Figure 3b for the CPS.

There is no guidance in the literature on how to address this problem. Previous work has dropped states entirely if they do not meet a threshold for the minimum number of individuals observed (Seguino and Heintz 2012). Similarly, I drop a state from the sample if the number of individuals observed in any given race-gender category falls below a chosen threshold in any year. However, there is no obvious choice for this threshold. As a first pass, I choose a cutoff of $N = 5$ and then test four alternatives as a robustness check ($N = 1, 10, 20, 35$). The $N = 5$ threshold results in 62.5% of observations being dropped from the PSID sample and 18.6% of observations from the CPS sample. A list of the states included in the analysis based on each choice of threshold can be found in Appendix B. The results that follow adopt the $N = 5$ cutoff unless otherwise noted.

Summary statistics for unemployment rates, wealth measures, and other covariates are listed in Table 2. As expected from aggregate data, unemployment rates are higher for black workers and for men on average, while household wealth is lower for black-headed and female-headed households. The averages and standard deviations of the wealth variables reflect their expected high variance. Notably, the logged measure of wealth reduces this variance and the



(a) PSID



(b) CPS

Figure 3: Histograms of state-gender-race-year observations by sample size

Notes: Histograms omit observations where $N = 0$. Red line indicates cutoff threshold of $N = 5$, such that states with any gender-race-year observations with fewer than 5 individual or household observations are dropped from the analysis.

influence of the right hand tail of wealth, but does so at the cost of understating the extent of racial wealth inequality.

Table 2: Summary statistics

Panel A: CPS Sample

	White men	Black men	Black women	White women	Total
Unemployment rate	6.020	14.83	12.31	5.045	9.549
	(2.631)	(10.18)	(8.963)	(2.025)	(8.111)
Labor force	79.70	76.89	69.25	65.27	72.78
participation rate	(3.341)	(7.250)	(8.295)	(5.166)	(8.556)
Real GSP growth	2.957	2.957	2.957	2.957	2.957
	(2.987)	(2.987)	(2.987)	(2.987)	(2.986)
N	1148	1148	1148	1148	4592

Panel B: PSID Sample

	White men	Black men	Black women	White women	Total
Average household	304869.3	84211.0	34603.0	134679.0	139590.6
wealth	(200516.1)	(187283.6)	(79366.8)	(93290.4)	(181201.9)
Median household	118064.0	21457.6	5266.8	50901.7	48922.5
wealth	(73699.0)	(18573.0)	(7463.3)	(50714.4)	(62887.7)
Log of average	12.44	10.82	9.850	11.59	11.18
household wealth	(0.607)	(0.830)	(1.029)	(0.669)	(1.247)
N	152	152	152	152	608

Notes: Standard deviations in parentheses. Averages are unweighted. Samples are limited to states that meet cutoff threshold of $N = 5$ for each dataset.

5 Main results

Table 3 presents the results for the main covariates of interest in the baseline analysis, which is used as a point of comparison with the results that apply Cooper et al.'s (2022) equilibrium rate gap measure of state-level relative monetary policy. Because neither time fixed effects nor a linear time trend are applied in these specifications, the effect of the nominal (effective) federal funds rate is likely confounded by variation in future and present national economic conditions. The direction of this bias is not clear *a priori.* Two corrections to these results will be considered: first, applying the Cooper et al. (2022) method of calculating state-specific measures of relative monetary policy stances and, as a robustness check to both results, incorporating linear time trends that can account for secular developments that have been roughly constant over time.

As expected, fixed effects for each race-gender category pick up large racial gaps in unemployment rates. Black men's (women's) unemployment rates are on average 4.2 (2.2) percentage points higher than those of white men. There is no significant difference in unemployment rates between white men and women since 1980. For white men, a one percentage point increase in the nominal federal funds rate in the previous year is associated with a 0.181 percentage point increase in the unemployment rate in the current year. This effect is about 0.36 percentage points greater for black workers regardless of gender, consistent with a situation in which the employment outcomes of black workers are more sensitive to changes in monetary policy than those of white workers.

The average level of the federal funds rate between 1980 and 2007 was 6.38%, with a standard deviation of 3.52 percentage points. Meanwhile, the unweighted average unemployment rate by state and year (as reported in Table 2 above) ranges from 5.05% for white women to 14.83% for black men. Given these averages, the interest rate findings reported in Table 3 indicate that a one SD increase in the policy rate is associated with a 10 to 12% relative increase in the unemployment rate for white workers and a 13 to 15% effect for black workers. A change in the policy rate should therefore be associated with an increase in racial unemployment ratios according to these results.

Similarly, the previously discussed racial wealth gaps are evident in the results of Columns 2 through 4 as is the gap in wealth across female- and male- or dual-headed households. As expected, absolute gaps wealth are larger when measured as state-race-gender averages rather than medians, although the size of the gap relative to the mean value in the sample is much larger when measured in median terms. The logged wealth results are in the middle of the two and indicate that black male- and dual-headed households' wealth is 64% lower than that of white male- and dual-headed households on average. The analogous results are 71.9% lower for black female-headed households and 33.1% lower for white female-headed households.

Table 3: Baseline regression results using nominal federal funds rate

	(1) Unemployment Rate	(2) Average Wealth	(3) Median Wealth	(4) Logged Wealth
L.Nominal FFR	0.181***	-18074.6***	-5630.8***	-0.0386**
	(0.0226)	(6225.4)	(1555.9)	(0.0176)
Black men	4.209***	-190692.0***	-112048.1***	-1.023***
	(0.544)	(43489.4)	(17155.6)	(0.184)
Black women	2.241***	-261952.9***	-120492.3***	-1.269***
	(0.528)	(45101.1)	(15826.8)	(0.205)
White women	-0.774	-174740.1***	-69381.2***	-0.402***
	(0.545)	(39848.3)	(16701.1)	(0.110)
Black men × L.Nominal FFR	0.369***	4106.9	6788.8***	0.0185
	(0.0785)	(9852.5)	(1842.8)	(0.0316)
Black women × L.Nominal FFR	0.362***	11798.3**	6038.2***	-0.0426
	(0.0902)	(5251.3)	(1475.0)	(0.0326)
White women × L.Nominal FFR	-0.0301	9982.0*	3400.3*	-0.0155
	(0.0406)	(5308.4)	(1891.3)	(0.0192)
Time FEs	No	No	No	No
Observations	4592	532	532	532
Adjusted R-squared	0.412	0.488	0.638	0.756

Notes: Standard errors in parentheses. Stars indicate significance at the * 10%, ** 5%, and *** 1% levels. Regressions control for current and lagged real GSP growth, the lagged value of the dependent variable, and group and state fixed effects. "Men" are "male- or dual-headed" and "women" are "female-headed HHs" in wealth regressions. Errors are clustered at the group-state level. Aggregation threshold used is $N = 5$, where any state that has one or more race-gender-state-year observations below N is dropped from the sample.

A one percentage-point increase in the FFR is associated with a decline in average wealth of approximately $18000 dollars for white male- and dual-headed households (or about $5600 when medians are used instead of averages). In logged terms, this represents a 3.86% decline in wealth relative to the average. These declines are smaller in absolute terms for other groups, particularly black female-headed households. They lead to a net effect of monetary policy for groups other than white male- and dual-headed households that is nearly zero when the median wealth measure is used. These results are consistent with the portfolio effect described by Bartscher et al. (2022): White (dual-headed) households are more likely than other groups to own homes and business wealth, both of which are particularly sensitive to monetary policy shifts, and hold higher levels of such wealth. The result may be larger losses during contractionary periods. In logged (relative) terms, however, there is no evidence of race- or gender-varying impacts. That is, smaller absolute losses may be roughly similar relative to the lower starting wealth of black and female-headed households. This finding is less readily consistent with portfolio effects, which would also imply larger relative losses for white dual-headed households.

Table 4 presents the results for the model incorporating time fixed effects and a state varying measure of the relative monetary policy stance per Cooper et al. (2022). The results for monetary policy's labor market effects using the rate gap are qualitatively similar but larger in magnitude. While part of this shift is expected to be due to the change in identification strategy, the scale of the rate gap variable also differs. Among the 41 states included in the CPS subsample for an observation threshold of $N = 5$, the average rate gap is only 3.85 with a standard deviation of 2.89, smaller than the same figures for the nominal federal funds rate.

A one percentage point increase in the rate gap is associated with a 0.53 percentage point increase in the unemployment rate for white workers, while the effect sizes are 1.309 and 1.169 percentage points for black men and women respectively. Despite the absolute increase, these differentials are smaller in magnitude relative to the effect on white male workers than in the baseline estimation: The baseline differentials led to a total effect on unemployment rates for black workers that was about three times that experienced by white workers. Accordingly, the effect size of a one SD change in the rate gap relative to starting unemployment rates is similar for white and black men (about 25.4%) and largest for black women (27.4%) and white women (30.2%). As a result, an increase in the rate gap would not increase racial ratios in unemployment rates, even though the risk of unemployment rises more for black workers than others.

Table 4: Regression results using estimated rate gap (adapted from Cooper et al. [2022])

	(1) Unemployment Rate	(2) Average Wealth	(3) Median Wealth	(4) Logged Wealth
L.Rate gap	0.528***	-32985.6**	-7999.0**	-0.0626
	(0.143)	(14490.3)	(3853.8)	(0.0450)
Black men	4.405***	-213844.6***	-54520.1***	-1.094***
	(0.459)	(36186.6)	(12990.4)	(0.158)
Black women	2.899***	-301513.5***	-57896.5***	-1.657***
	(0.593)	(44154.0)	(13765.3)	(0.199)
White women	-0.578	-206315.1***	-32013.6***	-0.604***
	(0.559)	(38222.7)	(10584.7)	(0.101)
Black men × L.Rate gap	0.781***	10877.4	6900.5**	-0.0162
	(0.119)	(14766.6)	(2943.8)	(0.0414)
Black women × L.Rate gap	0.641***	31902.4***	6101.1**	-0.0459
	(0.114)	(8443.6)	(2716.4)	(0.0372)
White women × L.Rate gap	-0.0328	28306.3***	3630.9	0.0168
	(0.0483)	(8710.5)	(3035.4)	(0.0251)
Time FEs	Yes	Yes	Yes	Yes
Observations	4592	532	532	532
Adjusted R-squared	0.452	0.519	0.779	0.770

Notes: Standard errors in parentheses. Stars indicate significance at the * 10%, ** 5%, and *** 1% levels. Regressions also control for current and lagged real GSP growth, the lagged value of the dependent variable, and group and state fixed effects. "Men" are "male- or dual-headed" and "women" are "female-headed HHs" in wealth regressions. Errors are clustered at the group-state level. Aggregation threshold used is $N = 5$, where any state that has one or more race-gender-state-year observations below N is dropped from the sample.

The policy measure's effect on wealth for white male- and dual-headed households similarly rises. Average wealth tends to fall by about $33000 for white men when the rate gap rises by one percentage point, while the same figure is $8000 when measured using medians, although these results are only significant at the 10% level and are insignificant in logged terms. The differentials by race and gender are slightly less positive relative to the effect for white men when the median measure is used, but the results are otherwise qualitatively similar. The results for the log of wealth in Column 4 provide no evidence that changes in the rate gap have distributional effects in relative terms and thus on the extent of racial or gender wealth gaps.

6 Mediator analysis

As discussed above, a variety of state- and time-varying factors could explain why interest rate changes have differential labor market and wealth effects by race and gender. In this section, I examine three potential variables that could be mediating this relationship, based on hypotheses from previous work. This approach is inspired by the mediator analyses of recent papers on monetary policy impacts, including Seguino and Heintz (2012) and Leahy and Thapar (2022).Three mediator variables are considered, with the data source listed in parentheses:

1. **The black share of the state population in state i and year $t-1$ (U.S. Census Bureau).** The black population share tests hypotheses from the stratification economics literature regarding the operation of discrimination as a form of non-market power. In their analysis of monetary policy's impacts, Seguino and Heintz (2012) hypothesize a quadratic relationship between black population share and discrimination. They argue that a rising black population share may initially reduce discriminatory outcomes if greater diversity in the population reduces stereotyping. At some tipping point, however, a rising black share of the population may lead to rising discrimination on the basis of "threat effects": A sufficiently large black population makes discrimination a viable tool for maintaining group advantages and thus individuals in the dominant group are more willing to invest in "racialized identity" (610). In their study of racial gaps in mortgage lending, Dymski and Aldana (2014) propose an inverted U-shaped relationship instead: A rising black population share may initially induce rising levels of discrimination due to such threat effects, but further increases may reduce discrimination if discrimination against a large share of the population becomes unfeasible or costly. Following this previous work, the black share of the state population enters the regression model quadratically instead of linearly.

2. **The share of non-farm employment in manufacturing and construction in state i and year $t-1$ (BEA).** Industrial composition has been a widely cited explanation for differences in monetary policy impacts. White men are disproportionately represented in interest-rate sensitive industries including manufacturing and construction. If the composition of these industries by race and gender remain constant, we would expect to see the gap in monetary policy impacts to become less positive or potentially negative as the share of state employment in these industries rises. However, theories of discrimination are again relevant here: If states where manufacturing and construction are relatively important see the greatest job losses (gains) during monetary tightening (loosening), then the "value" of discrimination as a tool of labor market competition will rise (fall). In this case, the relationship between the mediator and racial gaps would be positive.

3. **Gap in bank branches per 100,000 people (FDIC, 1994 onward only)**

$$bdgap_{it-1} = bd^b_{it-1} - bd^a_{it-1}$$

where bd^a_{it-1} is the bank density per 100,000 people in areas with above median black population share for state i in year $t-1$ (and conversely for bd^b_{it-1}). A rising value of this variable can be interpreted to indicate larger racial gaps in financial services access. As discussed in Section 2, lower levels of access to financial services in predominantly black and Hispanic communities may reduce the impacts of monetary policy in those areas.

The results of the mediator analysis are presented in Figure 4. Each graph plots coefficient estimates for the race and gender *difference* in the effect of the equilibrium rate gap on the unemployment rate at different levels of the mediating variable. Two results are significant. First, there is a modest but statistically significant negative relationship between the black share of the population and the gap between the interest rate effect on black men's and white men's unemployment rates. This result is inconsistent with the "threat effects" hypothesis of Seguino and Heintz (2012) but aligns in part with theories that predict discrimination to be increasingly infeasible as the black population share rises (Dymski and Aldana 2014). It should be noted, however, that relatively few states have a black population share of 32 to 40%, so the gradient is less relevant in practice. The distribution of the mediator variables is shown in Figure 5.

Second, the same difference in effects between black women and white men rises with

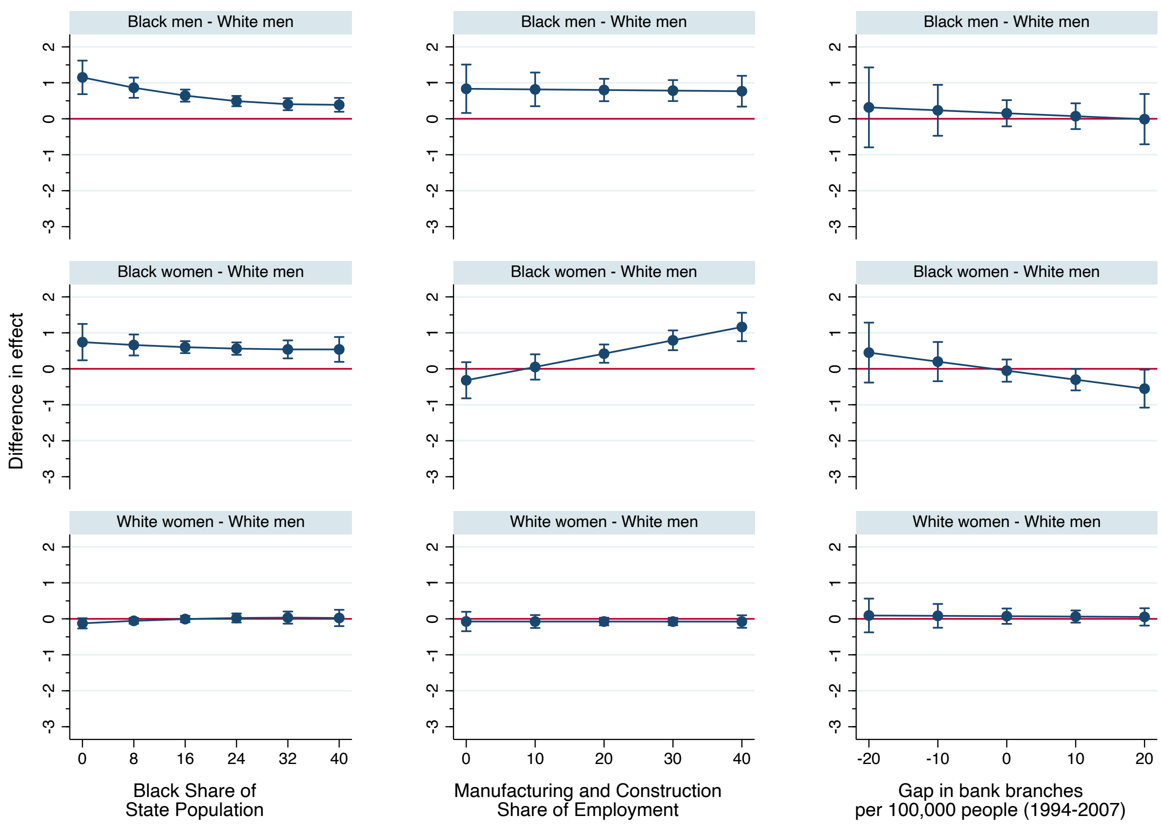


Figure 4: Relative effect of the equilibrium rate gap on unemployment by mediating variables
Notes: Each column presents results from separate regression with interaction of mediator variable and race-gender groups. Bars indicate 95% confidence intervals.

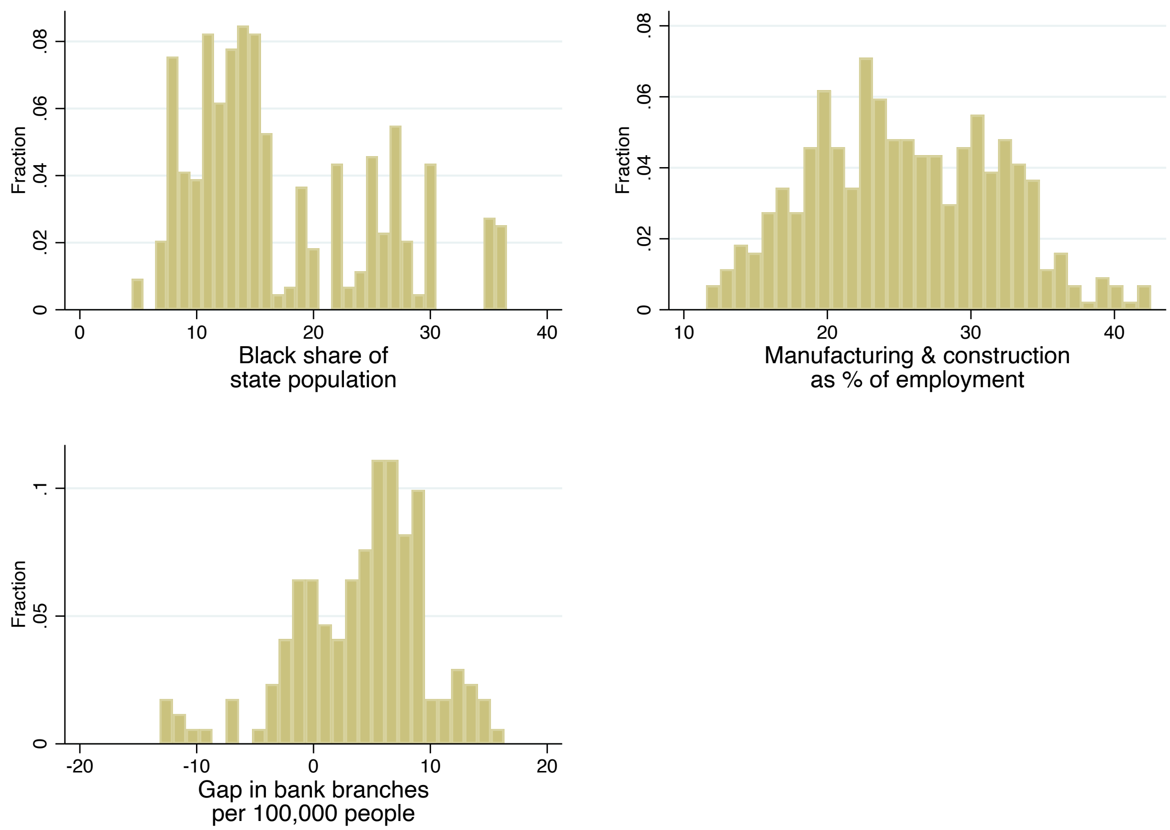


Figure 5: Histograms of mediator variables

Notes: Aggregation threshold used is $N = 5$, where any state that has one or more race-gender-state-year observations below N is dropped from the sample.

increases in the manufacturing and construction share of employment in a state. Both this result and the previous one are consistent with a broader job competition story. In states where manufacturing and construction employment shares are high, overall job scarcity is likely to rise in the case of contractionary policy as a result of the interest-rate sensitivity of the industry. This may result in rising competition in other industries, including those where black women are more likely to be present.

7 Robustness checks

7.1 Linear time trends

One concern may be that social and economic changes unrelated to monetary policy may have occured at the same time that interest rates have largely fallen. For example, there has been a secular decline in black unemployment rates since the 1980s, even if they remain higher than white unemployment rates.

Time fixed effects cannot be used in conjunction with the nominal federal funds rate in the baseline model. However, if the stronger assumption of constant change over time that is correlated with the regressors and dependent variable is justified (as the comments above suggest), then a linear time trend can be applied. Adding the interaction of race/gender categories and the time trend allows for these trends to differ by race and gender.

If the combination of time fixed effects and the Cooper et al. identification strategy succeed in identifying monetary policy shocks via idiosyncratic state variation in responsiveness to MP, then adding linear time trends should have a minimal impact on the estimates. The risk of this strategy is the high correlation between the time trends and the monetary policy measures. For instance, the Pearson correlation coefficient between the linear time trend and the nominal federal funds rate between 1980 and 2007 is -0.8098, falling to -0.7336 for the rate gap measure. A high degree of multicollinearity could result in underestimates of the monetary policy impact if some of the real impacts of rate changes are absorbed by the time trends.

Tables 5 and 6 show the results of adding linear time trends to the baseline regressions and Cooper et al. (2022) results (with and without time fixed effects in the latter case). Nearly all unemployment rate effects and differentials are explained away by the year fixed effects in the baseline specifications. As expected, wealth has risen over the sample period and unemployment rates have fallen. The decline in unemployment rates has been steeper for black men, although this decline falls short of conventional significance levels in Table 6, Columns 1 and 2. Wealth has increased for all groups over time. The absolute (relative) increases have been smaller (the same) for female-headed and black households. The core results of interest are qualitatively unchanged in the results using the Cooper et al. (2022) rate gap measure of monetary policy, particularly when both time fixed effects and the linear time trend are included.

Table 5: Regression results with linear time trends

	(1) Unemployment Rate	(2) Average Wealth	(3) Median Wealth	(4) Logged Wealth
L.Nominal FFR	0.00210	1466.8	-539.8	0.0114
	(0.0320)	(6036.9)	(2040.5)	(0.0181)
Black men × L.Nominal FFR	-0.0121	-11232.3	2397.3	0.00687
	(0.136)	(11355.4)	(2369.4)	(0.0367)
Black women × L.Nominal FFR	0.136	-4255.7	1424.0	-0.0461
	(0.163)	(6410.4)	(1913.4)	(0.0374)
White women × L.Nominal FFR	-0.00626	-554.2	2480.6	-0.0180
	(0.0368)	(5922.2)	(2389.3)	(0.0202)
Year	-0.0897***	14434.0***	1664.4**	0.0387***
	(0.0151)	(2348.4)	(782.8)	(0.00592)
Black men × Year	-0.207***	-11649.8***	-1466.2*	-0.00625
	(0.0563)	(2364.8)	(805.9)	(0.0105)
Black women × Year	-0.126*	-12468.0***	-1426.0*	0.00399
	(0.0676)	(2505.1)	(782.2)	(0.0135)
White women × Year	0.0106	-8456.2***	-85.13	-0.00189
	(0.0213)	(2432.4)	(889.0)	(0.00833)
Time FEs	No	No	No	No
Observations	4592	532	532	532
Adjusted R-squared	0.421	0.521	0.775	0.770

Notes: Standard errors in parentheses. Stars indicate significance at the * 10%, ** 5%, and *** 1% levels. Regressions also control for current and lagged real GSP growth, the lagged value of the dependent variable, and group and state fixed effects. "Men" are "male- or dual-headed" and "women" are "female-headed HHs" in wealth regressions. Errors are clustered at the group-state level. Aggregation threshold used is $N = 5$, where any state that has one or more race-gender-state-year observations below N is dropped from the sample.

Table 6: Regression results with linear time trends

	(1)	(2)	(3) Average Wealth	(4) Median Wealth	(5) Logged Wealth
	Unemployment Rate				
L.Rate gap	0.107**	0.582***	-26920.9*	-7424.5*	-0.0630
	(0.0450)	(0.137)	(13734.6)	(3894.2)	(0.0459)
Black men × L.Rate gap	0.690***	0.711***	841.7	5715.0*	-0.0279
	(0.131)	(0.139)	(15179.3)	(3030.4)	(0.0429)
Black women × L.Rate gap	0.486***	0.510***	22609.6**	5098.0*	-0.0370
	(0.159)	(0.166)	(8650.8)	(2788.2)	(0.0405)
White women × L.Rate gap	-0.0462	-0.0467	22623.6**	3646.0	0.0212
	(0.0553)	(0.0573)	(8730.3)	(3291.8)	(0.0278)
Year	-0.0671***	0.00500	11004.5***	1259.6	0.0318***
	(0.0130)	(0.0184)	(2311.3)	(798.1)	(0.00804)
Black men × Year	-0.0290	-0.0347	-9667.6***	-1374.2**	-0.0105
	(0.0355)	(0.0371)	(1906.2)	(667.9)	(0.00925)
Black women × Year	-0.0563	-0.0654	-9580.6***	-1221.5*	0.00892
	(0.0525)	(0.0533)	(2109.7)	(666.0)	(0.0128)
White women × Year	0.0000340	-0.00780	-6158.4***	-205.6	0.00383
	(0.0228)	(0.0216)	(2083.0)	(736.0)	(0.00864)
Time FEs	No	Yes	Yes	Yes	Yes
Observations	4592	4592	532	532	532
Adjusted R-squared	0.433	0.452	0.529	0.781	0.770

Notes: Standard errors in parentheses. Stars indicate significance at the * 10%, ** 5%, and *** 1% levels. Regressions also control for current and lagged real GSP growth, the lagged value of the dependent variable, and group and state fixed effects. "Men" are "male- or dual-headed" and "women" are "female-headed HHs" in wealth regressions. Errors are clustered at the group-state level. Aggregation threshold used is $N = 5$, where any state that has one or more race-gender-state-year observations below N is dropped from the sample.

When the Cooper et al. (2022) results are used, more of the original results from Table 4 remain. The main unemployment rate effect is explained away, but the differentials by race remain positive, even as unemployment rates have declined more quickly for black men. These results suggest that the Cooper et al. approach succeeds in identifying relative impacts of monetary policy that are independent of secular shifts over time.

7.2 Alternative observation thresholds

As noted in Section 4, a threshold of $N = 5$ was chosen for the analysis, such that any state where the number of individuals (CPS) or households (PSID) observed within a particular race-gender-state-year category fell below 5 in any given year would be dropped entirely from the analysis. Choosing an appropriate threshold involves trade-offs. On one hand, aggregating a small number of observations to form state-level estimates introduces substantial sampling variation and a high number of outliers (e.g., state-race-gender unemployment rates of 100%). Yet setting too high of a threshold will reduce the size of the panel, leading to a loss of statistical power and potential external validity problems.

To assess this problem, I repeat the main analysis with the Cooper et al. rate gap estimates and the mediator analysis for five thresholds: $N = 1, 5, 10, 20$, and 35. The lowest threshold of $N = 1$ keeps all states that have no missing race-gender-year observations. This leads states with 1) small populations and 2) small black population shares to be dropped from the analysis, particularly in the PSID. The highest threshold, $N = 35$, was chosen as the threshold where only two states remain in the PSID sample (California and Michigan).

The results for each threshold are reported in Table 7. The absolute effects of rate gap changes on the unemployment rate by race and gender remain similar across specifications, although the gaps shrink in magnitude as the threshold increases. The similarities are expected given that few additional states are dropped from the CPS dataset with each successive thresholds. The estimated coefficients remain similar but the standard errors for the average and median wealth results increase markedly with higher observation thresholds. By contrast, the predictive power of monetary policy changes rise with these thresholds for logged wealth. Such a trend is consistent with the high variability of average and median wealth estimates.

Table 7: Regression results using estimated rate gap (adapted from Cooper et al. [2022])

N = 1

	(1) Unemployment Rate	(2) Average Wealth	(3) Median Wealth	(4) Logged Wealth
L.Rate gap	0.526***	-19217.5**	-7047.4***	0.0739
Black men × L.Rate gap	0.778***	9341.6	7380.5***	0.00531
Black women × L.Rate gap	0.624***	23539.1***	6169.0***	0.0398
White women × L.Rate gap	-0.0413	21363.0***	4851.9*	0.0539
Observations	4699	672	672	672

N = 5

L.Rate gap	0.528***	-32985.6**	-7999.0**	-0.0626
Black men × L.Rate gap	0.781***	10877.4	6900.5**	-0.0162
Black women × L.Rate gap	0.641***	31902.4***	6101.1**	-0.0459
White women × L.Rate gap	-0.0328	28306.3***	3630.9	0.0168
Observations	4592	532	532	532

N = 10

L.Rate gap	0.741***	-45857.7**	-10650.1**	-0.0996*
Black men × L.Rate gap	0.602***	1851.9	5358.8*	-0.0358
Black women × L.Rate gap	0.522***	30547.2***	5530.4*	-0.0620
White women × L.Rate gap	-0.00681	26666.8***	3596.0	0.0189
Observations	3808	420	420	420

N = 20

	(1) Unemployment Rate	(2) Average Wealth	(3) Median Wealth	(4) Logged Wealth
L.Rate gap	0.749***	-33020.7**	-11666.3**	-0.121**
Black men × L.Rate gap	0.623***	9345.9	4567.0	-0.0382
Black women × L.Rate gap	0.546***	26065.5**	5228.9	-0.0639*
White women × L.Rate gap	-0.0533	24981.0**	4514.2	0.0174
Observations	3360	280	280	280

N = 35

	(1) Unemployment Rate	(2) Average Wealth	(3) Median Wealth	(4) Logged Wealth
L.Rate gap	0.641***	-59148.4***	-18261.4*	-0.321**
Black men × L.Rate gap	0.565***	30530.5	16357.6	0.0486
Black women × L.Rate gap	0.522***	21266.6	14887.7	-0.123***
White women × L.Rate gap	-0.0513	18031.7	13610.6	-0.0193
Observations	2688	56	56	56

Notes: Standard errors in parentheses. Stars indicate significance at the * 10%, ** 5%, and *** 1% levels. Regressions also control for current and lagged real GSP growth, the lagged value of the dependent variable, and group and state fixed effects. "Men" are "male- or dual-headed" and "women" are "female-headed HHs" in wealth regressions. Errors are clustered at the group-state level. Any state that has one or more race-gender-state-year observations below N is dropped from the sample.

When $N = 20$ and $N = 35$, only nine and two states (respectively) remain in the PSID dataset. The results with these thresholds indicate that a one-percentage point increase in the rate gap is associated with a 12 to 32 percent reduction in wealth for white male- and dual-headed households, while relative losses among black female-headed households are 6 to 12 percentage points greater. Caution should be taken when interpreting these results, however, as they may lack external validity and only reflect average outcomes in a handful of states.

7.3 Bootstrapped standard errors

Another concern is that the standard errors currently understate the variability inherent in the estimates because the rate gap measure is itself an estimated value. To address this, I conduct 200 bootstrap replications of the main analysis. I preserve the time panel component through cross-sectional or clustered resampling, where the unit of observation drawn with replacement is the state. All year-gender-race observations for that state are maintained as is. One challenge with this approach is that the number of observations will vary with each iteration when the threshold described in the previous section is applied. I use the most parsimonious threshold of $N = 1$ to minimize this problem. The results are presented in Table 8. The core results remain qualitatively unchanged, although the standard errors have risen significantly for the wealth analyses. The mediator analysis results are not shown but are also consistent with the results without bootstrapping.

Table 8: Regression results with bootstrapped standard errors

	(1) Unemployment Rate	(2) Average Wealth	(3) Median Wealth	(4) Logged Wealth
L.Rate gap	0.526**	-19217.5	-7047.4**	0.0739
	(0.231)	(12543.7)	(3342.0)	(0.103)
Black men × L.Rate gap	0.778***	9341.6	7380.5***	0.00531
	(0.123)	(9594.8)	(2698.8)	(0.0326)
Black women × L.Rate gap	0.624***	23539.1**	6169.0**	0.0398
	(0.128)	(9317.7)	(2640.6)	(0.0659)
White women × L.Rate gap	-0.0413	21363.0**	4851.9**	0.0539
	(0.0378)	(8519.6)	(2377.5)	(0.0354)
Time FEs	Yes	Yes	Yes	Yes
Observations	4699	672	672	672

Notes: Standard errors in parentheses. Stars indicate significance at the * 10%, ** 5%, and *** 1% levels. Regressions also control for current and lagged real GSP growth, the lagged value of the dependent variable, and group and state fixed effects. "Men" are "male- or dual-headed" and "women" are "female-headed HHs" in wealth regressions. Errors are clustered at the group-state level. Bootstrapping is clustered at the state level with 200 iterations. Aggregation threshold used is $N = 1$, where any state that has one or more race-gender-state-year observations below N is dropped from the sample.

7.4 Wealth without home equity

For policymakers interested in understanding the distributional impacts of monetary policy, the question of how wealth changes translate into welfare impacts is an important one. While monetary policy may lead to changes in home values, for example, the loss of home equity may be temporary and have limited effects on consumption. Yet the loss of more liquid wealth, such as savings, implies larger consumption effects.

The PSID has limited data on specific asset types, and an analysis of fully disaggregated holdings is beyond the scope of this paper. As a first pass, however, I repeat the wealth analysis using a narrower measure of net worth that excludes home equity. The results are presented in Table 9. Unlike the main analysis, a small number of race-gender-state-year observations (6 of 532) had non-positive wealth values when home equity is not included. These six observations were changed to 1 to allow for a log transformation to be applied.

Table 9: Regression results for wealth without home equity

	(1) Average Wealth	(2) Median Wealth	(3) Logged Wealth
L.Rate gap	-25793.8**	-1277.8	-0.144
	(12693.5)	(2165.2)	(0.101)
Black men × L.Rate gap	1833.9	827.0	-0.0670
	(15649.0)	(1287.7)	(0.0775)
Black women × L.Rate gap	24390.1***	945.2	-0.168**
	(7411.5)	(1283.1)	(0.0805)
White women × L.Rate gap	25824.3***	-573.6	0.0489
	(7551.9)	(1505.6)	(0.0354)
Time FEs	Yes	Yes	Yes
Observations	532	532	532
Adjusted R-squared	0.395	0.613	0.506

Notes: Standard errors in parentheses. Stars indicate significance at the * 10%, ** 5%, and *** 1% levels. Regressions also control for current and lagged real GSP growth, the lagged value of the dependent variable, and group and state fixed effects. "Men" are "male- or dual-headed" and "women" are "female-headed HHs" in wealth regressions. Errors are clustered at the group-state level. Aggregation threshold used is $N = 5$, where any state that has one or more race-gender-state-year observations below N is dropped from the sample.

The results support the findings of Bartscher et al. (2022) that the prominence of home equity in the wealth holdings of white, dual-headed households explains much of the greater sensitivity of this group's wealth to changes in interest rates. This is most evident in the results using median wealth, where no wealth effects are evident. The results for logged wealth, however, point towards greater relative wealth losses among black female-headed households in response to an increase in the equilibrium rate gap. This is consistent with the findings under certain alternative thresholds (see Table 7).

8 Unconventional Monetary Policy

None of the existing research on monetary policy's race- and gender-differentiated effects have considered the impacts of unconventional monetary policy (UMP) in the U.S., such as quantitative easing following the 2008 Global Financial Crisis. During periods where the short-term interest rate already fell to zero, the federal funds rate is no longer a meaningful measure of the monetary policy stance, leading previous empirical papers to end the analysis at 2007 or 2008.

As a first pass at extending the analysis to include UMP, I repeat the analysis using Cooper et al.'s (2022) approach but create a new monetary policy time series that combines the same effective federal funds rate data with a "shadow rate" as estimated by Wu and Xia (2016).[3] As explained by Krippner (2012), the zero lower bound on short-term interest rates is a function of the fact that investors seeking a low-risk asset can always invest in currency. Otherwise, it would be conceivable for very safe assets to have negative interest rates for extended periods of time. The shadow rate is an estimate of what that negative interest rate might be in a world without currency–i.e., the implied value of holding currency as a risk-free investment. While shadow rates have existed since the 1990s with the work of Black (1995), more recent developments such as those by Krippner (2013) and Wu and Xia (2016) have rendered Black's approach more tractable (Bullard 2012).

The Wu-Xia shadow rate series is nearly equivalent to the federal funds rate at any time when the observed rate is not at the zero lower bound. As a result, I use the same nominal federal funds rate series as earlier in the analysis, substituting values with the shadow rate for observations between 2010 and 2015. This approach allows me to extend the time period under analysis to between 1980 and 2019. I run two sets of models in these preliminary results. Table 10 presents results for the entire analytical period, and Table 11 examines

[3]The estimates used here are accessible through the Federal Reserve Bank of Atlanta's Center for Quantitative Economic Research (2022).

only the post-2007 period (2008 to 2018).

The results from Table 10 suggest that the main results are largely robust to including the period of quantitative easing and interest rates at the zero lower bound. The estimated interest rate coefficient for the IS curve is 0.357, only slightly higher than the original estimate of 0.333 (Table 1).[4] The impact of a one-point change in the real or implied policy rate on median wealth becomes more negative, increasing in magnitude more than the interaction terms by race and gender. These results indicate smaller gaps in losses between black and white households than the original estimates, possibly reflecting losses in wealth that were particularly severe for black households in the aftermath of the 2008 Financial Crisis and slow to recover (Pfeffer et al. 2013; Zhang and Feng 2017). However, when the analysis is limited to the post-2007 period, as in Table 11, changes in monetary policy are found to have few statistically significant effects on most outcome variables. The interest rate effect falls to 0.142 in IS curve estimates, with the coefficient for some states falling slightly below zero. This may be a function of the reduced sample size as well as differences in the transmission of UMP. Previous work pointing to differences across states in the impacts of monetary policy have largely focused on conventional policy. A recent working paper suggests that UMP as measured using the Wu-Xia shadow rate series appears to have more consistent effects across U.S. states than conventional changes in short-term interest rates (Chiarotti 2021).

[4] First-stage results available upon request.

Table 10: Estimates using the blended FFR/Wu-Xia shadow rates series, 1980 to 2019

	(1) Unemployment Rate	(2) Average Wealth	(3) Median Wealth	(4) Logged Wealth
L.Rate gap	0.657***	-39745.0***	-10429.1***	-0.125*
	(0.114)	(12104.9)	(3438.6)	(0.0659)
Black men	5.616***	-235541.4***	-52526.2***	-1.087***
	(0.305)	(35954.3)	(10545.1)	(0.144)
Black women	3.638***	-262223.3***	-56419.7***	-1.510***
	(0.365)	(41192.4)	(10956.0)	(0.164)
White women	-0.808*	-160710.8***	-32144.5***	-0.448***
	(0.444)	(27807.3)	(9717.6)	(0.0685)
Black men × L.Rate gap	0.555***	27221.2***	3959.7***	-0.00243
	(0.0800)	(4963.8)	(1388.2)	(0.0242)
Black women × L.Rate gap	0.447***	30139.2***	2979.8**	0.0299
	(0.0810)	(5209.3)	(1248.6)	(0.0394)
White women × L.Rate gap	-0.0241	18101.8***	1082.1	0.00295
	(0.0325)	(5260.2)	(1950.0)	(0.0165)
Time FEs	Yes	Yes	Yes	Yes
Observations	6560	988	988	988
Adjusted R-squared	0.473	0.601	0.676	0.668

Notes: Standard errors in parentheses. Stars indicate significance at the * 10%, ** 5%, and *** 1% levels. Regressions also control for current and lagged real GSP growth, the lagged value of the dependent variable, and group and state fixed effects. "Men" are "male- or dual-headed" and "women" are "female-headed HHs" in wealth regressions. Errors are clustered at the group-state level. Aggregation threshold used is $N = 5$, where any state that has one or more race-gender-state-year observations below N is dropped from the sample.

Table 11: Estimates using the blended FFR/Wu-Xia shadow rates series, 2008 to 2019

	(1) Unemployment Rate	(2) Average Wealth	(3) Median Wealth	(4) Logged Wealth
L.Rate gap	-0.00816	-8455.8***	-2786.2	0.0122
	(0.0111)	(2633.7)	(1724.3)	(0.0317)
Black men	5.647***	-325287.1***	-80001.5***	-1.271***
	(0.375)	(73838.0)	(17160.5)	(0.234)
Black women	3.121***	-344737.6***	-83494.1***	-1.733***
	(0.340)	(79934.0)	(16980.1)	(0.272)
White women	-0.952**	-222855.4***	-55645.0***	-0.588***
	(0.430)	(46198.3)	(16731.2)	(0.118)
Black men × L.Rate gap	0.000657	5498.4	1718.5*	-0.00523
	(0.00346)	(3467.1)	(985.6)	(0.0126)
Black women × L.Rate gap	0.00469	6324.1*	1915.0*	0.0110
	(0.00641)	(3271.0)	(991.9)	(0.0138)
White women × L.Rate gap	-0.000500	5505.6	1741.1*	0.0161*
	(0.00315)	(3890.7)	(952.6)	(0.00964)
Time FEs	Yes	Yes	Yes	Yes
Observations	1968	456	456	456
Adjusted R-squared	0.571	0.636	0.604	0.603

Notes: Standard errors in parentheses. Stars indicate significance at the * 10%, ** 5%, and *** 1% levels. Regressions also control for current and lagged real GSP growth, the lagged value of the dependent variable, and group and state fixed effects. "Men" are "male- or dual-headed" and "women" are "female-headed HHs" in wealth regressions. Errors are clustered at the group-state level. Aggregation threshold used is $N = 5$, where any state that has one or more race-gender-state-year observations below N is dropped from the sample.

9 Conclusions

This study combined two sources of household-level data with standard state- and national-level macroeconomic data series to examine whether changes in U.S. monetary policy impact unemployment rates by race and gender. There is a small but growing body of work that considers monetary policy's distributional impacts. While one previous paper has conducted a comparative statics analysis of the predicted effects of monetary policy on the racial wealth gap, this paper is the first to directly assess how wealth outcomes for black and white female-, male-, and dual-headed households change as a result of monetary policy. I also draw on recent innovations in the use of panel data to identify monetary policy shocks and estimate their effects.

I find that monetary policy shocks disproportionately affect the unemployment rates of black workers in the U.S.. Previous work has come to similar conclusions, although some have argued that differentials should only be measured relative to average unemployment rates (Zavodny and Zha 2000). The results of this study are mixed, but the preferred specifications suggest that contractionary monetary policy would not widen racial unemployment gaps. The mediator analysis is consistent with the theory that discrimination's contribution to racial differentials in monetary policy's impact with rise as competition over increasingly scarce good jobs rises.

Previous work has suggested that expansionary monetary policy should reduce racial wealth gaps by leading to smaller wealth losses among black households due to the types of wealth held (Bartscher et al. 2022). The results of this study confirm that absolute losses are greater among white male- and dual-headed households, as would be expected given the greater magnitude of wealth holdings and the composition of those holdings. However, the portfolio composition hypothesis also implies that white male and dual-headed households should experience greater relative losses than other groups, which is not supported by the results. Some specifications suggest that relative losses may actually be greater for black female-headed households than other groups, particularly when focusing on measures of wealth other than home equity. These findings regarding both labor market and wealth outcomes indicate that monetary policy interacts with social stratification by race and gender, with contractionary policy often but not always worsening existing gaps.

This analysis faces limitations common to work on both monetary policy and household finance. Most notably, the household wealth regressions may lack statistical power due to the small panel available. The time series dimension is short (eight years with gaps of two to five years), and the panel is limited to a set of nineteen states. The latter issue raises potential concerns about external validity as well if the states that are sufficiently large and have sufficient racial diversity to meet observation thresholds differ in systematic ways from other states. Potential extensions of this work could complement PSID data with data from other U.S. wealth surveys such as the SIPP or SCF to fill in gaps across survey years.

Finally, multiple unanswered questions remain about the distributional effects of monetary policy and potential heterogeneity in those effects. The analysis above assumes that monetary policy has symmetric effects. Certain groups can only be described as more or less sensitive to monetary policy, regardless of whether policy changes are contractionary or expansionary. In reality, the direction of a monetary policy shock may matter. Previous economic downturns would lead us to expect as much in the area of wealth outcomes, for instance. The 2008 Financial Crisis led both to initial wealth losses that were greater in relative terms for non-white headed households (Pfeffer et al. 2013) and to slower rates of recovery for those same households in the years that followed (Zhang and Feng 2017). The application of state-level data and new panel data techniques in this study provides a methodology with sufficient flexibility to provide robust answers to this and other questions regarding U.S. monetary policy's distributional impacts.

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A Fitted inflation rate

State-level inflation is calculated as the percentage change in the GSP deflator (real GSP/nominal GSP). Cooper et al. (2022) note that this raw inflation measure is volatile over time for specific states, although average inflation rates across states closely track national inflation based on the core PCE index. As a result, they derive a smoothed state-level inflation measure $\hat{i}_{it}$ as the fitted values of the following model

$$i_{it} = \alpha_i + \mu p_t + \tilde{g}_{it} + \tilde{g}_{it-1} + \epsilon_{it} \tag{4}$$

where i_{it} is the state inflation rate, α_i is a state fixed effect, p_t is the core PCE inflation rate for the U.S., and $\tilde{g}_{it}$ is the difference between real GSP growth and U.S. GDP growth in year t. Estimates are weighted based on the size of the labor force in state i in year t. Unlike Cooper et al. (2022), I do not constrain the coefficient on core PCE inflation to equal 1, although the results are largely unchanged with or without the constraint.

The results of this regression are presented in Table 12. Figure 6 compares the time series of national core PCE inflation, average state inflation using the GSP deflator, and the average smoothed state inflation measure.

Table 12: Regression results for fitted inflation rate

	State Inflation Rate
Core PCE inflation (%)	0.969***
	(0.0164)
Relative real GSP growth (%)	0.0702***
	(0.0159)
L.Relative real GSP growth (%)	-0.0209
	(0.0160)
Constant	0.138
	(0.246)
Observations	1428
R^2	0.721

Notes: Standard errors in parentheses. Stars indicate significance at the * 10%, ** 5%, and *** 1% levels. State inflation rate calculated as percentage change in GSP deflator. State fixed effects not shown. Full panel of 50 states and Washington D.C. is used, covering period between 1980 and 2007.

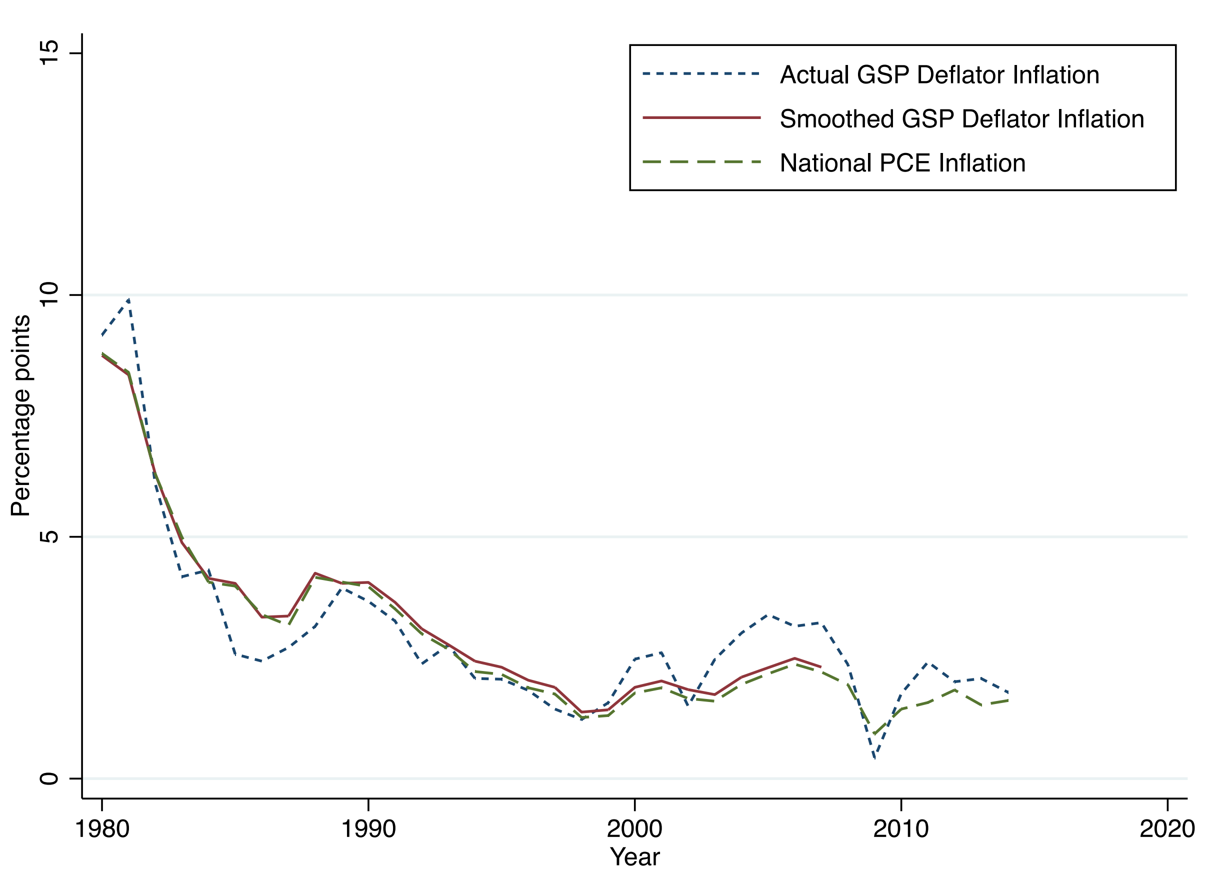

Figure 6: Average inflation measures by year, 1980-2007

Notes: State inflation rate calculated as percentage change in GSP deflator. Full panel of 50 states and Washington D.C. is used, covering period between 1980 and 2007.

B States included in final sample

Table 13: States included by cutoff threshold

	N = 1	N = 5	N = 10	N = 20	N = 35
Alabama	1	1	1	1	1
Alaska	1	1	1	0	0
Arizona	1	1	0	0	0
Arkansas	1	1	1	1	1
California	1	1	1	1	1
Colorado	1	1	1	0	0
Connecticut	1	1	1	1	1
Delaware	1	1	1	1	1
District of columbia	1	1	1	1	1
Florida	1	1	1	1	1
Georgia	1	1	1	1	1
Hawaii	1	0	0	0	0
Idaho	0	0	0	0	0
Illinois	1	1	1	1	1
Indiana	1	1	1	1	0
Iowa	1	1	0	0	0
Kansas	1	1	1	1	0
Kentucky	1	1	1	1	0
Louisiana	1	1	1	1	1
Maine	0	0	0	0	0
Maryland	1	1	1	1	1
Massachusetts	1	1	1	1	1
Michigan	1	1	1	1	1
Minnesota	1	1	0	0	0
Mississippi	1	1	1	1	1
...					

States included by cutoff threshold (continued)

	N = 1	N = 5	N = 10	N = 20	N = 35
...					
Missouri	1	1	1	1	1
Montana	0	0	0	0	0
Nebraska	1	1	0	0	0
Nevada	1	1	1	1	0
New hampshire	0	0	0	0	0
New jersey	1	1	1	1	1
New mexico	1	1	0	0	0
New york	1	1	1	1	1
North carolina	1	1	1	1	1
North dakota	0	0	0	0	0
Ohio	1	1	1	1	1
Oklahoma	1	1	1	1	0
Oregon	1	1	0	0	0
Pennsylvania	1	1	1	1	1
Rhode island	1	1	1	0	0
South carolina	1	1	1	1	1
South dakota	0	0	0	0	0
Tennessee	1	1	1	1	1
Texas	1	1	1	1	1
Utah	0	0	0	0	0
Vermont	0	0	0	0	0
Virginia	1	1	1	1	1
Washington	1	1	0	0	0
West virginia	1	1	1	0	0
Wisconsin	1	1	1	1	0
Wyoming	0	0	0	0	0
Proportion	82.4%	80.4%	66.7%	58.8%	47.1%

Notes: $N = X$ indicates that a state was dropped from the analysis if the number of households/individuals observed in that state fell below X for any race-gender-year observation. A value of 1 indicates that state is included in sample as defined by shown observation threshold.

Table 14: States included by cutoff threshold

	N = 1	N = 5	N = 10	N = 20	N = 35
AK	0	0	0	0	0
AL	1	1	0	0	0
AR	1	1	0	0	0
AZ	0	0	0	0	0
CA	1	1	1	1	1
CO	0	0	0	0	0
CT	1	0	0	0	0
DC	0	0	0	0	0
DE	0	0	0	0	0
FL	1	1	1	1	0
GA	1	1	1	0	0
HI	0	0	0	0	0
IA	0	0	0	0	0
ID	0	0	0	0	0
IL	1	1	1	1	0
IN	1	1	1	0	0
KS	0	0	0	0	0
KY	1	0	0	0	0
LA	1	0	0	0	0
MA	0	0	0	0	0
MD	1	1	0	0	0
ME	0	0	0	0	0
MI	1	1	1	1	1
MN	0	0	0	0	0
MO	1	1	1	1	0
MS	1	1	0	0	0
...					

States included by cutoff threshold (continued)

	N = 1	N = 5	N = 10	N = 20	N = 35
...					
MT	0	0	0	0	0
NC	1	1	1	1	0
ND	0	0	0	0	0
NE	0	0	0	0	0
NH	0	0	0	0	0
NJ	1	1	1	0	0
NM	0	0	0	0	0
NV	0	0	0	0	0
NY	1	1	1	0	0
OH	1	1	1	1	0
OK	0	0	0	0	0
OR	0	0	0	0	0
PA	1	1	1	1	0
RI	0	0	0	0	0
SC	1	1	1	0	0
SD	0	0	0	0	0
TN	1	0	0	0	0
TX	1	1	1	1	0
UT	0	0	0	0	0
VA	1	1	1	1	0
VT	0	0	0	0	0
WA	1	0	0	0	0
WI	0	0	0	0	0
WV	0	0	0	0	0
WY	0	0	0	0	0
Proportion	57.6%	45.6%	36.0%	24.0%	4.80%

Notes: $N = X$ indicates that a state was dropped from the analysis if the number of households/individuals observed in that state fell below X for any race-gender-year observation. A value of 1 indicates that state is included in sample as defined by shown observation threshold.