The Long-Lived Cyclicality of the Labor Force Participation Rate*

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Abstract

How cyclical is the U.S. labor force participation rate (LFPR)? We examine exogenous state-level business cycle shocks, finding that the LFPR is highly cyclical, but with significantly longer-lived responses than the unemployment rate. After a negative shock, the LFPR declines for about four years—substantially lagging unemployment—and only fully recovers after about eight years. Our main specifications use age-sex-adjusted LFPR, and we show that using the unadjusted LFPR is problematic because local shocks spur changes in the population of high-LFPR age groups. LFPR cyclicality varies across groups, with larger and longer-lived responses among men, younger workers, less-educated workers, and Black workers.

Keywords: labor force participation, labor supply, labor force composition, labor force demographics, full employment, Okun's law, geographic mobility, labor mobility, regional migration

JEL Classification: E24, J21, J22, J61, J64

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> For policymakers, the key question is: What portion of the decline in labor force participation reflects structural shifts and what portion reflects cyclical weakness in the labor market?
>
> *Janet Yellen (2014)*

1 Introduction

How cyclical is the U.S. labor force participation rate? Many observers have noted that the labor force participation rate (LFPR)—the share of population 16 years or older that is either working or looking for work—exhibits some degree of cyclicality (see, for example, Aaronson et al., 2014*b*; Council of Economic Advisers, 2014; Erceg and Levin, 2014; Montes, 2018; Hornstein and Kudlyak, 2019). Measuring the degree of cyclicality in the LFPR is complicated, though, by the presence of trend movements reflecting structural changes in the labor market that are unrelated to the business cycle, including, for example, the prolific entry of women into the workforce through at least the 1990s, the aging of the baby boom generation since the late 1990s, and the longer-run decline in the prime-age male LFPR (see the reviews by Abraham and Kearney (2020) and Juhn and Potter (2006) for a discussion of these and other structural forces). Observers often disagree about the magnitudes of these trends, which results in substantial disagreement about the extent of cyclicality in labor force participation. Those disagreements can be particularly acute following recessions, such as the period following the Great Recession in which estimates of the cyclical portion of the LFPR shortfall varied from 20 to 60 percent (Council of Economic Advisers, 2014).

We estimate LFPR cyclicality using state-level business cycles, which sidesteps the need to identify trend changes in labor force participation at the national level. We use the local projections method introduced by Jordà (2005) to estimate the response of the state-level, age-sex-adjusted LFPR to changes in state-level output. By using this approach, we are able to identify the response of the LFPR to unexpected declines in output without imposing strict parametric assumptions or assuming that the effects of business cycle shocks dissipate in the long run. To avoid endogeneity between output and the labor market, we instrument for changes in state-level output with a shift-share instrument exploiting variation in local exposure to national changes in output across industries (Bartik, 1991; Blanchard and Katz, 1992; Dao, Furceri and Loungani, 2017).

We show that labor force participation *is* cyclical, but that its response to an exogenous output shock is long-lived. In response to a negative 1 percentage point output growth shock, the LFPR declines slowly yet persistently and does not reach its trough until 4 years later—at about 0.2 percentage point below its initial value. The LFPR then gradually recovers and eventually returns to its pre-shock level, but not until about 8 years after the initial shock.

The cyclical response of the LFPR substantially lags behind the unemployment rate. Following a negative 1 percentage point output growth shock, the unemployment rate spikes quickly and peaks a year later, with a peak response that is about 0.4 percentage point.[1] By the time that the unemployment rate fully recovers 6 years after the shock, the LFPR has only reached the halfway point of its cyclical recovery. The delay in recovery between the LFPR and the unemployment rate suggests that observers who focus only on the unemployment rate underestimate the extent of slack remaining in the labor market after a recession, particularly in the period approximately 6 years or more after the initial shock.

These results shed light on the extent of slack in the post-Great-Recession labor market, which was hotly debated by policymakers at the time. By 2014, the unemployment rate had nearly returned to its pre-recession level, but the LFPR had continued to decline, reaching about 3 percentage point below its pre-recession level. This led to substantial

[1] This estimated coefficient on the unemployment rate is within the range of Okun's law coefficients estimated in the literature (Ball, Leigh and Loungani, 2017), supporting that our method measures cyclicality accurately.

disagreement about whether the shortfall in participation reflected cyclical factors that could later rebound, or structural factors that would keep the LFPR depressed (Council of Economic Advisers, 2014; Aaronson et al., 2014*b*; Erceg and Levin, 2014; Krueger, 2017).

The actual path of the LFPR following the Great Recession lines up closely with our estimates, implying that much of this shortfall reflected cyclical factors. We scale our estimates to a Great-Recession-sized shock and compare them to the national age-sex-adjusted LFPR from 2007 through 2019.[2] The actual and predicted paths are broadly consistent: the predicted LFPR declines slowly yet persistently through the middle of the 2010s and then rebounds over the subsequent several years, similar to the actual path. By the end of 2019, only a small portion of the national age-sex-adjusted LFPR is left unexplained by our cyclical model, implying that the response of the LFPR after the Great Recession largely did not reflect any unusual features of this recession and instead was in line with the typical business cycle pattern.

Why does the LFPR typically take so long to recover? We distinguish between two possible explanations. The first, which we term "shadow unemployment", refers to the notion that nonparticipants are effectively the same as unemployed workers, just counted separately. The distinction between unemployment and nonparticipation in the CPS is notoriously subjective, and workers highly attached to the labor force can still end up misclassified as nonparticipating (Abowd and Zellner, 1985; Elsby, Hobijn and Şahin, 2015). The second explanation we examine is that nonparticipants may be engaged in "persistent non-market-work activities", which includes individuals enrolled in school, at home taking care of family, or other similar activities. Although many of these individuals transition into employment in any given month, the propensity to rejoin the labor force might not respond quickly to changes in labor market conditions, since these activities may take time to enter or exit.

[2] As we discuss in Section 5, the national age-sex-adjusted LFPR is the correct benchmark to compare our estimates to both because it controls for the aging of the baby boom generation over the period following the Great Recession and because we use the state-level age-sex-adjusted LFPR in our estimation.

We find that changes in shadow unemployment do not explain the delayed recovery of the LFPR. Although shocks do lead to increases in nonparticipants who self-report that they "want a job", this type of nonparticipation returns to its pre-shock level at the same time as the unemployment rate and well before the LFPR has fully recovered. Similarly, we find that shocks lead to an increase in churn between unemployment and nonparticipation, which we take as a measure of shadow unemployment, but this too subsides before the LFPR is fully recovered.

Instead, the delayed cyclical recovery is driven by persistent non-market-work activities, which build in response to a shock but take some time to unwind. Initially following a negative shock, the increase in persistent non-market-work activities is mainly driven by people either taking care of the home and family or going to school. These increases only start to dissipate several years after the shock, reflecting the stickiness of these choices to leave the labor force once they are made. Only after the labor market is well on its way to recovery do these types of nonparticipation return to pre-shock levels. This is also consistent with the patterns we document for flows from nonparticipation to employment: these flows drop initially in response to the shock and then surge only after the unemployment rate has fully recovered, driving the delayed recovery of the LFPR.

Our main approach controls for changes in the composition of state-level populations through age-sex adjustment. This approach removes any mechanical effect on the LFPR from changes in the age-sex structure of the population following local-level shocks, which may occur due to either in-migration or out-migration among particular age groups. In our baseline specification, we adjust for age by residualizing the individual-level LFPR on single-year-age-by-sex fixed effects; we use the state-year average of these age-sex-adjusted outcomes as the dependent variable in our local projections regressions. In this way, our estimates isolate the true cyclical response of the LFPR to an output shock without the influence of age-sex compositional changes.

This age-sex-adjustment is necessary, since we show that shocks lead to structural changes in the population of high-LFPR ages. Following a negative 1 percentage point shock to output growth, the population of 25 to 39 year olds gradually decreases over 10 years, eventually falling up to 4 percent below the pre-shock level, while other age groups see little change in population over the same period. Since 25 to 39 year olds tend to have higher LFPRs than other age groups, this response mechanically lowers the unadjusted state-level LFPR by about 0.2 percentage point in the long run. We explore whether the population changes along other demographic dimensions—such as educational attainment, race, and marital status—but find little further effects beyond age. Our finding that the local LFPR is persistently altered by changes in the population among young, prime-age people adds to the understanding of the migratory adjustment mechanism of local shocks documented by Blanchard and Katz (1992), Dao, Furceri and Loungani (2017), and Amior and Manning (2018).

We also document that the long-lived cyclicality of the LFPR is especially pronounced for less-advantaged groups in the labor market. Younger workers (ages 16 to 24) exhibit a much larger cyclical response of the LFPR than do prime-age workers (ages 25 to 54), while older workers (ages 55+) show a lower degree of cyclicality. Our estimates show a sharp difference by education level with less-educated workers experiencing a large decrease in LFPR after a shock, while more-educated workers experience no significant change in labor force participation. We also find substantial inequality in long-lived cyclicality across racial and ethnic groups, with the LFPR for Black workers exhibiting larger, longer-lived cyclicality than the LFPR for white workers.

Our paper contributes to the literature studying LFPR cyclicality. Several recent papers take a national-level approach, estimating a structural trend for the LFPR and using deviations from this trend to estimate LFPR cyclicality (Aaronson et al., 2014*a*,*b*; Council of Economic Advisers, 2014; Krueger, 2017; Montes, 2018; Hornstein and Kudlyak, 2019). This approach requires specifying the structural supply and demand forces that affect participation decisions. Hobijn and Şahin (2021) apply this approach to labor market flows rather than rates, using different components of flows to separate cyclical changes in LFPR from other long-run factors. Another approach, used by Aaronson et al. (2014*b*); Erceg and Levin (2014); Balakrishnan et al. (2015), is to rely on state-level variation as we do in our analysis. While some of these papers do argue that the cyclical response of LFPR can be delayed, one of the main contributions of our paper is to use a method that is particularly well-suited for causally estimating long lags in LFPR cyclicality. More precisely, unlike the previous papers in this literature, we estimate the dynamic response of LFPR to exogenous output shocks by using local projections, which allow for the possibility of very persistent effects on LFPR. Moreover, by using a shift-share instrumental variable approach, we are able to establish a link between exogenous shocks and the dynamic response of LFPR.[3] Our approach of using state-level variation to identify LFPR cyclicality and aggregate up to an estimate of national LFPR cyclicality follows the growing literature using regional variation to study macroeconomic phenomena (Nakamura and Steinsson, 2014, 2018; Beraja, Hurst and Ospina, 2019; Chodorow-Reich, 2019; Fukui, Nakamura and Steinsson, 2023).

Additionally, following the early work of Blanchard and Katz (1992), several papers investigate how employment adjusts in response to economic shocks at the local level (Decressin and Fatas, 1995; Bound and Holzer, 2000; Dao, Furceri and Loungani, 2017; Amior and Manning, 2018; Weinstein, 2018; Yagan, 2019; Hershbein and Stuart, 2024; Hornbeck and Moretti, 2024) as well as the relationship between shocks and migration (Cadena and Kovak, 2016; Monras, 2018; Howard, 2020). This literature has documented that local labor markets adjust following shocks through changes in migration that return the labor market to equilibrium. We contribute to this literature by showing that this migration channel can have persistent effects on LFPR through altering the composition of the population, primarily among 25 to 39 year olds, which makes it important when studying local shocks to use the age-sex-adjusted LFPR.

[3] Balakrishnan et al. (2015) also use a similar shift-share instruments, but for employment, while our paper uses output. Using the latter has several methodological advantages as we argue later on.

We also contribute to a literature following Okun (1973) that examines how different demographic groups fare during a long recovery. Some groups of workers may disproportionately benefit from a tight labor market, as discussed in Bradbury (2000), Hoynes (2000), Jefferson (2008), Hoynes, Miller and Schaller (2012), Wilson (2015), Cajner et al. (2017), Aaronson et al. (2019), Fallick and Krolikowski (2022), and Hotchkiss and Moore (2022). Some of these differences in the benefits of a tight labor market may stem from delayed recoveries of LFPR, since we estimate that some groups' LFPRs take substantially longer to recover after a typical recession. These differences in LFPR cyclicality make it necessary to sustain recoveries beyond the point at which unemployment has fully recovered if policymakers want to ensure all groups experience a full recovery.

2 Research Design

We measure the cyclicality of labor force participation by estimating its response to state-level business cycles in order to sidestep the issue of trend changes in participation, which complicate identifying cyclicality at the national level. In this section, we outline our research design, starting with the identification problem and our approach to solve it. We then turn to the issue of inference and the description of the data we use in this analysis.

2.1 Identification

Estimating the dynamic cyclical responses of national outcomes typically requires strict assumptions. For example, time series models usually assume a mean zero cyclical component, which rules out hysteresis by definition. Further, identification in those models relies on a trend component that is smooth and identifiable—a strong assumption for the LFPR, given the sharp and changing nature of LFPR trends for various subgroups of the population.

To meet these challenges, we use state-level panel data to estimate the dynamic cyclical responses of labor market outcomes to a state-level business cycle shock using the local projections method. In particular, we measure the impulse response functions (IRFs) of a shock by estimating the following series of regressions indexed by k:

$$y_{s,t+k} - y_{s,t-1} = \beta^{(k)}\text{Shock}_{s,t} + \Theta W_{s,t} + \epsilon_{s,t+k} \tag{1}$$

where $y_{s,t}$ represents the labor market variable of interest—for example, the LFPR—of state s in time t; k indexes the regression that measures the effect of the shock at time t on the dependent variable $t+k$ periods ahead; $\text{Shock}_{s,t}$ is the measure of the business cycle shock (defined below); and $W_{s,t}$ represents a vector of control variables. In our baseline specification, the controls include state and year fixed effects.

Our local projections regressions control for national trends through the inclusion of year fixed effects. This method does not impose strict assumptions about the smoothness of trends, as would be needed in national-level time series regressions. Nation-wide phenomena that affect labor market outcomes across all states equally, including demographic shifts (such as the aging of the baby boom generation) and national policy responses (such as monetary policy shocks), are controlled for nonparametrically by this approach.

We view local projections regressions as a better alternative in our setting than vector autoregressions (VARs). Stock and Watson (2018) point out that instrumented versions of VARs and local projections identify the same IRFs under standard conditions, but VARs may not correctly identify IRFs if the true IRFs are not invertible. In terms of efficiency, instrumented local projections have the same properties as VAR models with internal instruments, as documented by Plagborg-Møller and Wolf (2021). Additionally, Olea and Plagborg-Møller (2021) show that local projections have attractive properties for inference.[4] For these reasons, we use local projections in our main specification, but

[4]Herbst and Johannsen (2024) show that local projections can be biased in small samples when the outcome variable is highly persistent, but this bias is likely to be minimal in our setting. The dependent

in Appendix C.3 we conduct a test of invertibility from our estimated IRFs which rejects the null hypothesis, implying that local projections are a more appropriate choice for this setting than VARs.

Shocks: We measure the business cycle shock using real gross state product (GSP) growth as estimated by the BEA. Specifically, we define $\text{Shock}_{s,t} \equiv \Delta GSP_{s,t}$, where $\Delta GSP_{s,t}$ is the year-over-year percent change in GSP. That is, the shock is a one-time, temporary, one percentage point shock to GSP growth. All else equal, the shock leads to a permanently lower level of output.[5]

GSP estimates are based on the factor incomes earned and other costs incurred in production, which is the same concept for measuring output as is used by Gross Domestic Income (GDI) at the national level. For each state, GSP sums labor income, capital income, and business taxes, where each of the three components is estimated by industry. Note that labor income is based on wage and salary accruals (as opposed to disbursements), which implies that retroactive wage payments (bonuses) are counted for the year in which they were earned rather than when they were received.

We view our choice to define business cycles based on output as superior to alternative approaches that use employment. Using GSP provides a measure of business cycle fluctuations at the state level that is more comprehensive than only using employment, which omits fluctuations in productivity. Additionally, if shocks take time to propagate to the labor market, using output will correctly time business cycles, while employment-based business cycles will tend to lag behind the true timing of the shock. Lastly, estimating the response of LFPR to an output shock, rather than an employment shock, makes the results more interpretable in the context of Okun's Law, a key economic relationship used among many policymakers.

variable in our regressions, the state-level age-sex-adjusted LFPR, only has an autocorrelation coefficient of 0.81, lower than the 0.9–0.99 range in which this bias becomes acute.

[5] We show in Section 3 that the lower level of output in large part reflects a permanently lower level of output per employee.

Potential Endogeneity: The coefficient $\beta^{(k)}$ gives the k-period-later response of y to a one-time, temporary, one percentage point shock to GSP growth. For $\beta^{(k)}$ to identify a causal effect of the GSP shock on $y_{s,t+k} - y_{s,t-1}$, it must be the case that, conditional on the set of controls, the growth rate of GSP in period t is uncorrelated with the error term:

$$\mathbb{E}\left[\Delta GSP_{s,t} \cdot \epsilon_{s,t+k} | W_{s,t}\right] = 0$$

However, two key concerns suggest this requirement might not be met in practice. One concern is that employment may affect GSP, as lower employment (through higher unemployment, lower LFPRs, or both) will lower GSP if productivity is held constant. A second concern is that GSP growth could be autocorrelated, in which case estimates of $\beta^{(k)}$ may pick up the correlation between $y_{s,t+k} - y_{s,t-1}$ and GSP growth rates in future (or past) periods.

Instrument: To overcome these issues, we instrument for ΔGSP with a Bartik (1991) shift-share measure to isolate demand shocks at the state-level. The first-stage equation is as follows,

$$\Delta GSP_{s,t} = \alpha Z_{s,t} + \gamma W_{s,t} + \nu_{s,t} \tag{2}$$

where the shift-share instrument $Z_{s,t}$ is defined as

$$Z_{s,t} \equiv \sum_q \Delta GDI_{q,-s,t} \omega_{q,s,t-5}. \tag{3}$$

Industries are indexed by q, and $\omega_{q,s,t-5}$ represents the three-year moving average of industry q's share of total GSP in state s five years previously.[6] $\Delta GDI_{q,-s,t}$ represents the growth rate of national gross domestic income in industry q for period t using the "leave-one-out" approach—that is, we calculate $GDI_{q,-s,t}$ by summing up $GSP_{q,s,t}$ across

[6]During the first five years of available industry data, we calculate $\omega_{q,s,t-5}$ from industry q's share of total GSP in the first year of data instead.

all states except for state s.

This formulation of the shift-share instrument relies on industry variation in output, rather than employment. Many previous studies, including Blanchard and Katz (1992), Dao, Furceri and Loungani (2017), Adão, Kolesár and Morales (2019), and Goldsmith-Pinkham, Sorkin and Swift (2020), measure the response of employment to a shift-share instrument that uses industry variation in employment. However, we view industry variation in output as more appropriate for our setting, both because changes in output are likely to be more closely aligned to industry cycles and because output is a distinct variable measured separately from our outcomes of interest.

In our setting, identification with the shift-share instrument derives from exogeneity of the shares (Goldsmith-Pinkham, Sorkin and Swift, 2020). When an industry experiences a national-level change in output, this shock has a larger impact on local labor demand in states in which this industry accounts for a larger share of output. In this interpretation, the shift-share instrument amounts to pooling together many separate differences-in-differences event studies each based on a specific industry-share/time-period (Borusyak, Hull and Jaravel, Forthcoming). In Appendix B.3, we follow the approach of Goldsmith-Pinkham, Sorkin and Swift (2020) to decompose the shift-share instrument into the contribution of each industry-share/time-period.

Identifying Assumptions: In order for $Z_{s,t}$ to be a valid instrument, it must meet the following conditions (Stock and Watson, 2018):

$$\mathbb{E}\left[Z_{s,t} \cdot \Delta GSP_{s,t} | W_{s,t}\right] = \alpha \neq 0 \qquad \text{(relevance)} \qquad (4)$$

$$\mathbb{E}\left[Z_{s,t} \cdot \epsilon_{s,t} | W_{s,t}\right] = 0 \qquad \text{(contemporaneous exogeneity)} \qquad (5)$$

$$\left.\begin{array}{l} \mathbb{E}\left[Z_{s,t} \cdot \epsilon_{s,t+k} | W_{s,t}\right] = 0 \\ \mathbb{E}\left[Z_{s,t} \cdot \Delta GSP_{s,t+k} | W_{s,t}\right] = 0 \end{array}\right\} \text{for } k \neq 0 \qquad \text{(lead-lag exogeneity)} \qquad (6)$$

$Z_{s,t}$ captures predicted GSP growth for a given state, s, in time, t, based on that state's industry mix in period $t-5$. We argue that this is likely to meet the relevance condition since local output in a given industry is likely to be correlated with national output in that industry due to changes in industry technology or relative demand. The contemporaneous exogeneity assumption will hold as long as the national industry shocks used to construct $Z_{s,t}$ are unrelated to local changes in labor market outcomes (where we have removed any mechanical correlation by using a "leave-one-out" approach). Lead-lag exogeneity requires not only that $Z_{s,t}$ is uncorrelated with unobserved forces affecting local labor markets in other periods, but also that it is not correlated with any of the three components of $\Delta GSP_{s,t+k}$ in other periods (e.g. labor income, capital income, or business taxes). In Appendix C, we show that $\mathbb{E}[Z_{s,t} \cdot \Delta GSP_{s,t+k} | W_{s,t}] \approx 0$ for $k \neq 0$ in our sample, confirming this aspect of lead-lag exogeneity.[7]

2.2 Inference

This section describes three important issues for inference in our research design: the role of clustering in computing standard errors, how we weight observations, and testing for potential weak instruments.

Clustering: To quantify the uncertainty around our estimated impulse response functions, we compute heteroskedasticity-robust standard errors clustered at the state-level in our baseline specification. Adão, Kolesár and Morales (2019) raise concerns that this approach may understate uncertainty in shift-share designs. However, these concerns are primarily about settings where variation comes from a subset of industries, while our setting uses the full set of industries.[8] We validate this choice in Appendix C.2 with a

[7] If the shock were positively autocorrelated, then some of the effect estimated by $\beta^{(k)}$ would be the result of the persistence of the shock, thus biasing our estimate upward. Conversely, if the shock were negatively autocorrelated, the effects of an output shock on the LFPR would be stronger than what is estimated by $\beta^{(k)}$. Since we find that autocorrelation in $Z_{s,t}$ is minimal, this bias does not affect our estimates.

[8] Our analysis is most similar to the results shown in Panel B of Table 6 in Adão, Kolesár and Morales (2019), which shows that more sophisticated approaches to estimate confidence intervals are not meaningfully different from clustering by local labor market.

placebo exercise, which indicates that our clustered standard errors are, if anything, a bit conservative for this setting.

Weighting: We weight each regression of outcome $y_{s,t}$ for group j by the population n^j_{st} of group j in state s at time t. The smallest states have relatively few respondents in the CPS, which has the potential to generate noise when calculating state-level LFPRs for those smaller states and yield imprecise regression estimates. The noise issue compounds when slicing the data further into subgroups of the population, such as prime-age individuals, men and women, and levels of educational attainment. Weighting by state-level population reduces the influence of noise in our estimates.

Testing for Weak Instruments: To verify that our estimates are not affected by weak instrument problems, we conduct first-stage F-tests for each specification. In Appendix Table D.1, we report for each horizon k in our baseline specification the first-stage F-statistic introduced in Kleibergen and Paap (2006), since the error term may be nonhomoskedastic. Although the instrument and endogenous variable are the same in all specifications, the F-statistics may vary across regressions for different demographic groups due to the different state population weights for different groups.

2.3 Data

We combine state-level data from multiple sources to form an annual panel covering 1978–2017. Labor market outcome variables consist of the unemployment rate, the labor force participation rate, and the employment-to-population ratio, each of which is calculated from Current Population Survey microdata. For each rate, we compute the average over the calendar year in each state. Our main specification uses the CPS sample of civilian noninstitutionalized people ages 16 and over to compute each of these rates. In later sections, we compute these rates for subgroups of the population.

In order to control for shifting demographics, we age-sex-adjust each of our labor market outcome variables. That is, for an outcome $y_{i,s,t}$ for person i in state s and year t, we estimate the the following equation on our CPS sample:

$$y_{i,s,t} = \theta_{\text{age}(i),\text{sex}(i)} + \tilde{y}_{i,s,t} \tag{7}$$

where $\theta_{\text{age}(i),\text{sex}(i)}$ is a age-by-sex fixed effect. We then compute the average age-sex-adjusted outcome for state s in year t as

$$\tilde{y}_{s,t} \equiv \sum_{i \in (s,t)} \tilde{y}_i w_i \tag{8}$$

where w_i is the CPS sampling weight for person i. This procedure removes changes from our outcomes that are due to changes in the age structure of the population such as the aging of the baby boom generation, which has been shown to be responsible for variation in labor market outcomes over time (see, e.g., Shimer, 1999). We use the age-sex-adjusted rates in all of our main estimates, but return to examine the role of this adjustment compared to alternative adjustments and unadjusted rates in Section 7.1.[9]

Annual data on GSP for each state and year are obtained from the BEA.[10] GSP data by industry are from the BEA as well, using SIC-coded industries for 1978–1998 and NAICS-coded industries for 1998–2017. For the purposes of decomposing the variation in the shift-share instrument in Appendix B.3, we link a subset of industries between SIC and NAICS that are categorized in essentially the same way in both systems, and otherwise treat industries as distinct between the two systems.

[9]In our main specifications that use these age-sex-adjusted rates as outcomes, we do not adjust our standard errors for the uncertainty created by the age-sex adjustment step. To gauge the potential magnitude of this uncertainty, we took 100 draws from the estimated normal distribution for $\theta_{\text{age}(i),\text{sex}(i)}$, recomputed the age-sex-adjusted outcomes separately for each draw, and then re-estimated our main specification separately for each draw. The resulting 100 IRFs are nearly identical (the minimum and maximum at each horizon differ by less than 0.0005 p.p.), indicating that the uncertainty created by our age-sex adjustment does not meaningfully affect our results.

[10]GSP data at the quarterly frequency are available only from 2005:Q1 onwards.

3 Effects on Output and Productivity

Before examining effects on the labor market, we examine the effects of our shift-share instrument on output and labor productivity. Specifically, we estimate the two-stage least squares regressions of Equation 1 and Equation 2, where the outcome is either real GSP or real GSP per worker. For horizon $k = 0$, the effect on the growth rate output essentially reports the first-stage effect, and for horizons $k \neq 0$ this is informative about lead-lag exogeneity. We examine effects on labor productivity and output both in growth rates and in levels.

Figure 1 shows the estimated impulse responses of output and productivity to a temporary negative 1 percentage point output growth shock. The left panel shows the effect on yearly growth rates of productivity, along with the cumulated effect on the level of productivity. Productivity grows by about 0.5 percentage point less in the year when the shock takes place, but grows similarly afterwards. This leads to a level of productivity that is permanently about 0.25–0.5 percent lower after the shock than before. Productivity accounts for about half of the initial shock to output (shown in the right panel of Figure 1), with the remainder accounted for by employment. As productivity is stable after the initial shock, the further decline in output in year 1 and the subsequent partial recovery entirely reflect employment. This points to output shocks being initially driven by productivity before employment adjusts in response, with time aggregation leading to some of this response appearing in the same year as the shock. These estimates also indicate that our instrument picks up an important source of variation—productivity shocks—which would be omitted in an employment-based shift-share instrument.

4 Cyclicality of Labor Market Outcomes

Figure 2 presents our estimates of the impulse response functions for the age-sex-adjusted LFPR, unemployment rate, and employment-to-population ratio (EPOP) from 3 years before the shock to 10 years after the shock.[11] For ease of interpretation, we report all of our estimates as the response to a temporary negative 1 percentage point shock to GSP growth, so that the cyclical responses will have the same sign as in a recession.

The unemployment rate, LFPR, and EPOP all respond to cyclical shocks, but with varying timing. For the unemployment rate, a contractionary 1 percentage point shock to output growth causes a contemporaneous 0.25 percentage point increase in the unemployment rate. The increase in the unemployment rate continues in the following year and peaks at 0.4 percentage point one year after the shock. Our estimate of the total increase in the unemployment rate due to a negative 1 percentage point shock to GSP is within the range of Okun's law coefficients estimated in the literature of -0.5 to -0.4; see, for example, Ball, Leigh and Loungani (2017). Following the peak one year after the shock, the unemployment rate steadily declines by about 0.1 percentage point per year until it returns to its pre-shock value about six years after the shock and remains there. This asymmetric response of a sharp increase followed by a gradual decrease is consistent with the "plucking" dynamics of business cycles examined by Dupraz, Nakamura and Steinsson (2019).

The LFPR also shows a significant response to a negative shock, but with a substantial delay compared to the unemployment rate. Specifically, the LFPR declines by less than 0.1 percentage point in the year of the shock, much smaller than the increase in the unemployment rate. However, while the unemployment rate quickly peaks and begins to recover, the LFPR continues to steadily decline for several years after the shock, finally reaching a trough four years later at a level that is 0.2 percentage point below its initial value. After reaching its trough, the LFPR gradually recovers and only attains its pre-shock level eight years after the initial shock, two years after the unemployment rate has fully recovered.

The different patterns for the LFPR and unemployment rate reflect different *cyclical*

[11] For parsimony, we only show three years of pre-trends, but we have estimated pre-treatment effects up to 10 years before the shock (not shown) and find no significant pre-trends over that time period.

profiles, which we can show formally with a nonlinear Wald-type test. We denote the set of coefficients tracing out the impulse response of the LFPR as $\{\beta^{(k)}_{\text{LFPR}}\}$ and the set of coefficients for the unemployment rate as $\{\beta^{(k)}_{\text{UR}}\}$. Our null hypothesis is that the LFPR response has the same time profile as the unemployment rate but perhaps a different cyclical loading: $\beta^{(k)}_{\text{LFPR}} \equiv \frac{\beta^{(k)}_{\text{UR}}}{\phi}$ for each horizon k. Under this null, the ratio of coefficients $\frac{\beta^{(k)}_{\text{UR}}}{\beta^{(k)}_{\text{LFPR}}}$ is the same at every horizon k. To test this, we stack the samples used to estimated impulse responses for both variables and re-estimate Equation 1, from which we obtain a covariance matrix containing all coefficients for both impulse responses.[12] We use the delta method to construct a nonlinear Wald-type test statistic for the restriction that the ratio of coefficients is the same at each horizon. For the null hypothesis that lags 1 to 8 share the same ratio, we obtain a test statistic of 31.69 with a p-value of 0.000, enough to strongly reject the null hypothesis that the time profile is the same for both variables.

The combination of the LFPR and unemployment rate responses create cyclicality in the EPOP that is both large and long-lasting. The EPOP declines rapidly at the onset of the shock, reflecting the initial spike in the unemployment rate, and reaches its trough at about -0.4 percentage point two years after the shock. Thereafter, the EPOP steadily recovers by about 5–10 basis points per year until it is fully recovered seven years after the shock. While the EPOP shortfall in earlier years reflects high unemployment, the remaining EPOP shortfall in years 5 to 7 is almost entirely accounted for by the LFPR.

5 Implications for National LFPR Cyclicality

In this section, we lay out a framework for aggregating our results from state-level business cycles to the national level. Using this framework, we show that our estimates broadly match the observed dynamics of the LFPR following the Great Recession. In particular, the strength in the observed LFPR starting in 2014 lines up closely with the

[12]This is equivalent to a seemingly unrelated regression with the same right-hand-side variables in each equation (Davidson and MacKinnon, 1993).

delayed recovery of the LFPR in our estimates.

The aggregate LFPR at the state level can be expressed as an average of the LFPRs of each age-sex group in the state. We denote the LFPR for an age-sex group a in state s and time period t as $\text{LFPR}_{a,s,t}$ and the population weights for each group as $w_{a,s,t}$, and the first equality below gives this expression. The LFPR for each age-sex group can in turn be broken down into a cyclical component and a secular trend component unrelated to the business cycle. Let $C_{s,t}$ denote the measure of the business cycle in state s and time period t, let $\beta(L)$ denote the cyclical coefficients tracing out the impulse response, and let $\alpha_{a,s}$ and $\alpha_{a,t}$ be state-age-sex fixed effects and year-age-sex-specific secular trend components respectively. Substituting these components in for the group-specific LFPR yields the second equality below:

$$\text{LFPR}_{s,t} = \sum_a \text{LFPR}_{a,s,t} w_{a,s,t} = \sum_a \left(\alpha_{a,s} + \alpha_{a,t} + \beta(L) C_{s,t}\right) w_{a,s,t}$$

From this expression, the first-order approximation for changes in the state-level LFPR from period t to $t+k$ can be broken down into three components[13]:

$$\Delta\text{LFPR}_{s,t+k,t} \approx \underbrace{\sum_a \left(\Delta\alpha_{a,t+k,t} w_{a,s,t}\right)}_{\text{Trend}} + \underbrace{\beta(L)\Delta C_{s,t+k,t}}_{\text{Cycle}} + \underbrace{\sum_a \left(\alpha_{a,s} + \alpha_{a,t} + \beta(L) C_t\right) \Delta w_{a,s,t+k,t}}_{\text{Population changes}}$$

When using our methodology to estimate the cyclical response of the LFPR, the first and third terms drop out. The first term consists of national trends in LFPR, which can be broken down into a purely national component $\sum_a \left(\Delta\alpha_{a,t+k,t} w_{a,t}\right)$ that is absorbed by our time fixed effects and a residual component $\sum_a \left(\Delta\alpha_{a,t+k,t}(w_{a,s,t} - w_{a,t})\right)$. Under our identification assumption, the residual component is equal to zero.[14] The third term in the decomposition above is equal to zero in our setting since we use the age-sex-adjusted

[13]This approximation excludes a higher-order term involving the product $\Delta\alpha_{a,t+k,t} \cdot \Delta w_{a,s,t+k,t}$.
[14]This assumption is consistent with the lack of noticeable pre-trends in our LFPR estimates.

LFPR as the outcome, which mechanically adjusts for $\Delta w_{a,s,t+k,t}$.[15]

As a result, our methodology provides estimates for the cyclical coefficients $\hat{\beta}(L)$. These coefficients represent the lagged change in LFPR associated with a one unit change in output. Importantly, since we include time fixed effects, the change in output $\Delta C_{s,t+k,t}$ is measured relative to nationwide trend output growth, which accounts for gradual increases in GDP due to population growth and productivity, among other forces.

The national LFPR is an average of state LFPRs, and can be decomposed similarly. Averaging over states and dropping the state subscripts, the decomposition above becomes:

$$\Delta \text{LFPR}_{t+k,t} \approx \underbrace{\sum_a \left(\Delta\alpha_{a,t+k,t} w_{a,t}\right)}_{\text{Trend}} + \underbrace{\beta(L)\Delta C_{t+k,t}}_{\text{Cycle}} + \underbrace{\sum_a \left(\alpha_{a,t} + \beta(L)C_t\right)\Delta w_{a,t+k,t}}_{\text{Population changes}}$$

From this decomposition, we use our estimated coefficients $\hat{\beta}(L)$ to trace out the predicted change in the national age-sex-adjusted LFPR to a recessionary shock, namely the Great Recession. Using the age-sex-adjusted LFPR removes the third term, and we pick a time period (2007–2019) over which trends, aside from population aging, are estimated to have been close to zero on net (Montes, 2018), removing the first term.[16] Additionally, by using the age-sex-adjusted LFPR, we remove any contribution of migratory responses to shocks on the LFPR. Although we find significant migratory responses that affect the LFPR at the state level, as we will discuss in detail in Section 7.2, those migratory responses are unlikely to have an effect on the national LFPR to the extent that the patterns we identify reflect primarily interstate migration and not international migration.

[15] Note that even if we had not used the age-sex-adjusted LFPR, our inclusion of time period fixed effects would absorb any of the variation in $\Delta w_{a,s,t+k,t}$ that is common nationwide, for example the aging of the baby boom population. However, $\Delta w_{a,s,t+k,t}$ also includes age-selected migratory responses, which are *not* controlled for by time period fixed effects, but are controlled for by using the age-sex-adjusted LFPR.

[16] Montes (2018) estimates that all of the decline in the aggregate LFPR from 2007:Q4 through 2017:Q4 can be explained by population aging (see Table 5, p. 23). Although that model estimates the relationship of other structural factors on the LFPR, it finds that changes in structural factors that put downward pressure on the LFPR during that period, such as increases in disability take up and an increase in the share of men who are unmarried (and tend to participate less), were entirely offset by the increased upward pressure from rising educational attainment.

Figure 3 plots our main estimates of the cyclical response of LFPR applied to the Great Recession shock along with the actual age-sex-adjusted LFPR for this time period. For the Great Recession shock, we compute the decline from 2007:Q4 to 2009:Q2 in real GDP from BEA, minus the expected increase in real potential output over the same period from CBO's August 2007 projections (CBO, 2007), which is a measure of the change in the output gap over that period.[17] We use the change in GDP relative to potential output to align with our estimates, since our estimates control for changes in potential output through year fixed effects. The change in the calculated output gap over that period was -8.1 percentage points, and, thus, the predicted path of the LFPR from our estimates in Figure 2 is $-8.1 \times \hat{\beta}^{(k)}$ for each horizon k, and we plot this against the actual path of the LFPR in Figure 3.

The prediction from our estimates is broadly consistent with the actual age-sex-adjusted LFPR over this period, featuring a similar slow decline over 2009–2014 and subsequent rebound in later years. By 2019, only a small portion of the LFPR recovery is left unexplained by our model. This similarity suggests that the LFPR largely followed its usual cyclical dynamics over this period with little deviation.

We caution that this exercise depends crucially on the assumption that the local and national cyclical effects are equal, conditional on state and time fixed effects and in age-sex-adjusted terms. This assumption would be violated if the local cyclical effects capture responses beyond just the direct effect of the shock that would not occur in response to a national shock. For example, if local production features decreasing marginal product of labor, then net out-migration following a local shock increases local productivity, which could raise wages and entice nonparticipants to enter the labor force, but this channel would not occur to the same extent at a national level since international migration is significantly more costly than domestic migration. The literature has found mixed evidence on how net internal out-migration affects local labor market equilibrium; for example,

[17] We use the August 2007 projections in order to ensure that the potential output estimates are not affected by the downturn in LFPR that occurred after the recession.

Monras (2018) finds that net out-migration boosts local productivity while the results of Howard (2020) imply a drag on local productivity after net out-migration as the construction sector and housing market turn down.

In our context, we view the local estimates as reasonable approximations for the national cyclical effect. As we note in Section 3, we find essentially zero effect on productivity growth in years 1–10 following the initial shock, indicating that the net out-migration happening during this period is unlikely to be affecting the local labor market in ways that could lead to substantial indirect effects on the age-sex-adjusted LFPR. In this way, we view our exercise as providing a first-order approximation of the national-level cyclical response, but a comprehensive model quantifying each channel precisely is outside the scope of this paper.

6 What Drives the Long-Lived Cyclicality of Labor Force Participation?

In this section, we draw on two additional features of the CPS data to understand why the LFPR exhibits long-lived cyclicality. First, we use individuals' self-reported reasons for nonparticipation to examine the cyclicality of "discouraged" individuals—those who report wanting a job but remain out of the labor force—compared to individuals engaged in non-market-work activities, such as home production or schooling. Second, we examine the cyclicality of flows into and out of the labor force to understand whether the shortfall of participation is caused more by a lack of individuals joining the labor force or a surplus of individuals leaving the labor force.

6.1 Reasons for Labor Force Nonparticipation

Business cycle shocks may lead people to make decisions that have persistent effects on their labor supply, which could account for the long-lived cyclicality in the LFPR. Such decisions may include enrolling in school, staying at home and taking care for a family member, applying for disability benefits, or retiring. Alternatively, the long-lived cyclicality in the LFPR may reflect individuals becoming discouraged and stopping their search for work, even though they would still prefer to be employed.

To determine the extent to which each of these explanations may account for long-lived cyclicality, we use questions in the CPS that ask nonparticipants about their reason for being out of the labor force. Throughout the sample period, nonparticipants were asked whether they want a job, which provides an indication of desired labor supply. Additionally, from 1989 onward, nonparticipants were asked to categorize their main reason for being out of the labor force between being ill or disabled, in school, taking care of home or family, retired, or other, and this question is a full partition of the not-in-the-labor-force group.[18] For each of these questions, we compute the share of the population in each state-year that is made up by nonparticipants in each category, and estimate Equation 1 using these outcomes. The estimated impulse responses are shown in Figure 4. We show the IRFs only through eight years following the shock, since the estimates around lag eight become extremely noisy due to the limited sample.

Increases in schooling, staying at home due to family responsibilities, and rising self-reported disability all play important roles in shaping the cyclical response of aggregate labor force participation.[19] Initially, nonparticipants taking care of home/family consti-

[18] For both of these questions, surveys before 1994 only asked these questions to the roughly $\frac{1}{4}$ of nonparticipants who are part of the Outgoing Rotation Groups in months 4 and 8 in sample. Further, the "want a job" question is separate from the "main reason for being out of the labor force" question (e.g. some respondents who report being in school may also report wanting a job, while others in school may report not wanting a job).

[19] In Appendix Figure D.3 we show that increases in schooling are most prominent for young people, but also present for prime-age individuals. Women are more likely to report nonparticipation for home/family reasons than men.

tute the largest response, with schooling close behind. However, nonparticipants reporting illness or disability grow steadily in response from year two onward, and comprise a larger portion of the response in years 5 to 7 than people taking care of home or family. People in school grow steadily as well, before falling rapidly in years 7 and 8 when the overall LFPR is reaching its pre-shock level.

Interestingly, the cyclical response of labor force participation does not seem to be driven by retirement decisions. If anything, retirements appear to exert an upward pressure on the LFPR. This could indicate that recessions induce individuals to postpone retirements, perhaps due to a fall in the value of their retirement savings or to potentially offset income losses of their household members who may lose a job.

Separately, we also look at the cyclicality of labor force nonparticipants who say they want a job, which can represent labor market slack. Although nonparticipants who want a job drive essentially all of the early rise in nonparticipation, their participation recovers faster than nonparticipation as a whole, reaching its pre-shock level around the same time as the overall unemployment rate does (years 4–5). This pattern suggests that expansive definitions of the unemployment rate that include nonparticipants who want a job—BLS' U-5 measure includes some of them—are able to capture additional margins of slack beyond the main unemployment rate, but do not capture the long-lived cyclicality of participation.

6.2 Labor Market Flows

Additionally, the panel structure of the CPS allows us to examine the contributions of inflows and outflows to the long-lived cyclicality of the LFPR. Examining cyclicality of flows provides more insight into the cyclicality of stocks, as demonstrated in previous work including Elsby, Hobijn and Şahin (2015), Elsby et al. (2019), and Cairó, Fujita and Morales-Jiménez (2022). We calculate annual labor market transitions by matching individuals in the CPS over 12-month horizons, and express those flows as shares by dividing by population 16 years and over. To be consistent with our baseline results, we also adjust for age by residualizing the flow rates using person-level data to net out the composition component explained by the age distribution of the people in each state.

Three aspects of the response of flows (shown in Figure 5) are worth noting.[20] First, at the onset of a negative output shock, labor force entry drops, driven by a large decline in the flow from nonparticipation to employment, which could reflect decisions to prolong schooling or to stay home and take care of family, as discussed in the previous subsection. Second, from years 1 to 2 after the shock, flows between unemployment and nonparticipation in both directions rise notably. In particular, negative business cycle shocks lead to an increase of "in-and-outs", that is individuals who temporary leave the labor force, perhaps due to discouragement.[21] Flows between unemployment and nonparticipation remain elevated until roughly year 6, which is also how long the unemployment rate remains elevated (recall Figure 2). Finally, flows from nonparticipation to employment eventually surge around 8 years after the shock, which leads to the recovery of the LFPR. In terms of magnitudes, outflows are elevated by 2 to 3 basis points after a negative shocks, while inflows are depressed by about 5 basis points. Cumulatively, the net effect for flows is similar to the one estimated for the stock of labor force participants shown in Figure 2.

7 The Role of Changing Demographic Composition

In addition to changes in the age-sex-adjusted LFPR, shocks may lead to changes at the state level in the age structure of the population or other demographics. In this section, we examine how the demographic composition of the state-level population responds to

[20]For brevity, we do not report flows between employment and unemployment, since these are neutral with respect to the LFPR.

[21]Note that while the hazard rate of $U \to N$ flows ($U \to N$ flow divided by the stock of unemployment) declines in recessions, in part driven by compositional changes of unemployed and their higher eligibility for unemployment insurance, the stock of unemployed rises even more during recessions, leading to an increase of $U \to N$ flows as a share of the population (Elsby, Hobijn and Şahin, 2015).

output shocks, finding evidence that shocks induce permanent, structural composition shifts away from high-LFPR subgroups in affected states.

We start by showing that the unadjusted LFPR experiences a persistent shortfall after output shocks. However, this persistent effect is not the result of hysteresis but instead reflects changes in the demographic composition of the population at the state level, primarily the age distribution. We find little to no contribution from changes in education, race, ethnicity, and marital status.

Next, we examine how the state-level population in each single-year age group changes in response to output shocks, finding that declines are concentrated among 25 to 39 year olds. Since this age group tends to have higher LFPRs than other age groups, declines in its population pull down the unadjusted overall LFPR mechanically after an output shock. We emphasize that this phenomenon raises the importance of using age-sex-adjusted LFPRs to examine questions about cyclicality and hysteresis in response to local shocks.

7.1 Cyclicality of Adjusted and Unadjusted LFPRs

To investigate how demographics affect the cyclicality of the LFPR, we compare our age–sex-adjusted baseline estimates to two alternative benchmarks.

First, we estimate Equation 1 using the unadjusted LFPR. Figure 6 shows that the unadjusted LFPR steadily declines to its trough in year four, with similar timing but a steeper decline compared to the age-sex-adjusted LFPR. However, while the age-sex-adjusted LFPR subsequently recovers back to its pre-shock level, the unadjusted LFPR merely edges up a bit, but remains well below its pre-shock level even ten years after the shock. A nonlinear Wald-type test of the null hypothesis that the ratio of coefficients between the unadjusted LFPR and age-sex-adjusted LFPR are the same at all horizons (following the same procedure as in Section 4) produces a test statistic of 21.81 and a p-value of 0.0027, rejecting that the two IRFs have the same cyclical profile.

While a persistent shortfall of the unadjusted LFPR after a shock might be interpreted as evidence of hysteresis, we caution that this is not the case in our setting. By hysteresis, it is commonly meant that people become persistently less likely to participate in the labor market as a result of the shock. However, our estimates do not suggest that people experience persistently lower participation conditional on their demographics, as the age-sex-adjusted LFPR fully recovers on average by 8 years after a shock.

For our second benchmark, we consider a broader adjustment for multiple demographic characteristics. Using person-level data from the CPS, we regress a person's labor force participation indicator on demographic characteristics using the following linear-probability model:

$$Y_{i,s,m,t} = \psi_0 + \Psi^{i,m,t} D_{i,m,t} + \Psi^{s,m,t} W_{s,m,t} + \eta_{i,s,m,t} \tag{9}$$

where $Y_{i,s,m,t}$ is a indicator variable indicating whether person i in state s was participating in the labor force in month m of year t; $D_{i,m,t}$ is a vector of indicator variables over the demographic characteristics of person i in month m of year t that include age, gender, educational attainment, race/ethnicity, and marital status; and $W_{s,m,t}$ is a vector of state, month, year fixed effects. The age variables are single-year age indicators for ages 16 to 79 and a indicator variable for ages 80 years and older. The educational attainment indicators partition attainment into five categories: less than a high school degree, a high school degree, some college, a four-year college degree, and more than a college degree. The race/ethnicity indicators partition the population into four groups: non-Hispanic white, non-Hispanic Black, Hispanic, and other. Marital status is a single indicator indicating whether an individual is married. We include month-of-year indicator variables to account for seasonality.

Using the estimated coefficients from Equation 9, we predict whether a person is participating in the labor force based on their demographic characteristics and denote this by $\widehat{Y^{D}_{i,s,m,t}}$. With this fitted value, we calculate the demographically-adjusted LFPR as the residual, $\widehat{Y^{D.adj}_{i,s,m,t}}$. We then aggregate the person-level fitted and residual components to calculate monthly rates for the fitted and demographically-adjusted labor market variables in each state s, and then average across months within year t to create a fitted value component, $\widehat{y^{D}_{s,t}}$, and a demographically-adjusted component, $\widehat{y^{D.adj}_{s,t}}$. Finally, we use those fitted values and demographically-adjusted state-level variables as the dependent variable in Equation 1.

The additional demographic controls beyond age make little to no difference in estimating LFPR cyclicality. Figure 6 shows that the addition of adjustments for education, race/ethnicity, and marital status results in nearly the same estimated impulse response as our baseline estimates, which adjust for age and sex only. The similarity of adjusted values is mirrored in the fitted values, which both decline steadily in response to the shock. This pattern points to the age structure of the state-level population changing persistently in a way which would mechanically pull down the LFPR absent adjustment.

In Appendix Figure D.1 we repeat this exercise for the unemployment rate. In contrast to the LFPR, we find that demographics explain essentially none of the response of unemployment, both immediately following the shock and in the long-run afterwards.

7.2 Response of Population Composition to Cyclical Shocks

Why does the age-composition of the state-level population change in response to a business cycle shock? Blanchard and Katz (1992) provide empirical evidence that economic shocks at the state level trigger adjustments not only through unemployment, but also by triggering cross-state migration. More recently, Dao, Furceri and Loungani (2017) show that it still remains the case that net migration across states responds to spatial disparities in labor market conditions and especially so during recessions, though the effect has weakened somewhat over time. However, Amior and Manning (2018) show that long-term adjustment in regional populations tends to differ across demographic groups, and if the migration response to business cycles similarly differs across groups, then the composition of the population could be altered by these shocks. For example, if shocks lead to higher migration responses among prime-age people, who tend to have higher LFPRs, then these shocks could alter the composition of the population resulting in a permanently lower LFPR.

In this section, we examine how the age composition of a state's population across single-year-age groups responds to a business cycle shock. Understanding the changes in the age structure are essential not only for understanding how the population changes but also for understanding how national LFPR cyclicality may be related to local LFPR cyclicality. If shocks induce out-migration of selected groups, the response of the local LFPR, absent any demographic adjustments, may include both the direct cyclical effect as well as the effect of the migration response. However, national LFPR cyclicality would only contain the first effect, assuming that shocks do not induce sizeable migration out of the country. The response of the age-sex-adjusted LFPR, though, would be comparable to national LFPR cyclicality, since it would not be affected by the migration channel.

To estimate the effect of a business-cycle shock on the composition of the state's population, we estimate Equation 1 with the outcome $y_{s,t+k}$ being the log population of a single-year-age group in state s in period $t+k$.[22] We estimate this equation for each single-year-age group from ages 16 through 80. The interpretation of the estimated equation for single-year-age group 25 in period $k = 10$ would be, for example, the percent change in the level of the total 25-year-old population in state s between periods $t+10$ and $t-1$ caused by the business-cycle shock.[23]

[22] Relative to Amior and Manning (2018), our analysis focuses on single-year-age groups instead of coarse age groups, estimates annual dynamic responses instead of decadal responses, and estimates the response to exogenous output shocks.

[23] We use state-level data for the covered-area population for single-year-age groups from the U.S. Census Bureau. These population estimates use the most recent decennial census population counts as a base and then add births, subtract deaths, and add net migration (both international and domestic) to produce yearly population estimates for each age in each state. The covered-area population is slightly different from the civilian noninstitutional population, which is used to calculate LFPR and EPOP. The covered-area population includes active members of the armed forces as well as those in institutions (e.g. penal, mental facilities, and homes for the aged), whereas the civilian noninstitutional population does not include these

A negative business cycle shock causes the population between the ages of 25 and 40 to persistently decline in states exposed to the shock relative to those states without a shock (Figure 7). Prior to the shock, there is limited evidence that changes in the population are correlated with the business cycle shock, but as the shock takes hold, changes in the composition of the population become apparent. Two years after the shock, the population levels of 23 to 35 year olds are all noticeably below levels immediately prior to the shock. Declines in population among these ages continue as time passes, and the population effects of the shock expand to other ages. Ten years after the shock, the population levels of 29 to 31 year olds decline to about 5 percent below their pre-shock levels, and the population levels of all single-year-age groups between 25 and 39 years olds are at least 2 percent below their pre-shock values. The population responses 10 years after the shock tend to hold in years 11 through 15 (not shown), suggesting that a negative business cycle shock permanently lowers the population of 25 to 39 year olds in exposed states. Since 25 to 39 year olds are among the highest in LFPRs relative to other age groups, permanent declines in a state's population that are concentrated in this age range will also permanently lower its LFPR through compositional effects, all else equal.

There are several plausible reasons why the net-population response might be concentrated in individuals ages 25 to 39, although formally testing these theories is outside the scope of our paper. First, people in this age range may be less likely to be homeowners, on average, so it might be easier for them to move to a different state in response to a negative shock. Additionally, if a state has been hit by a negative business cycle shock, people from other states that are finishing school may be less likely to move to such a state. As a result, if a state experiences a recession, it could have a "missing generation" of recent graduates. This is consistent with the responses shown in Figure 7, as initially, the largest response is for people in their mid-20s. However, as time goes by and people get older, the response shifts to the right of the age distribution.[24]

groups. This distinction is not likely to matter in our analysis.

[24] Our results also show a small *increase* in the population 17 to 22 year olds. Although testing the reasons

8 Differences in Long-Lived Cyclicality Across Groups

Business cycles can have different effects on different demographic groups. We explore the differences across a number of demographic subgroups more comprehensively in Appendix A, but summarize the main findings here.

As with prior work using aggregate data, we find large differences in cyclical sensitivity (Jefferson, 2008; Hoynes, Miller and Schaller, 2012; Cajner et al., 2017). The increase in unemployment in response to a negative business cycle is larger for 16–24 year old workers than 25–54 or 55+ year olds, larger for men than women, larger for Black and Hispanic than white individuals, and substantially larger for workers with only a high degree or less education compared to workers with at least a college degree.

Groups with larger unemployment responses also see larger and *more persistent* decreases in LFPR. This disparity is particularly noticeable when comparing groups with different education levels: workers with a high school degree or less see their LFPR decline for 5 years following a negative shock and take another 5 years to fully recover, while workers with a college degree or more see essentially no shortfall of LFPR following a negative shock. Long-lived cyclicality is also especially pronounced among 16–24 year olds and Black workers, who see only a little recovery in their LFPR 6–8 years after a shock.

These results may speak to the findings in the literature that hot labor markets tend to disproportionately benefit certain demographic groups (Okun, 1973; Hotchkiss and Moore, 2022; Aaronson et al., 2019). We find that these same demographic groups experience greater and longer-lived cyclicality in their LFPRs. To the extent that periods of "hot labor markets" (e.g. the late 1990s or late 2010s) were simply periods in which the

behind the increase for this college-age group is beyond the scope of this paper, one plausible mechanism is that recessions cause reductions in income and wealth that make young people more likely to stay in state for their college education with more affordable tuition. Indeed, Molloy, Smith and Wozniak (2011), studying interstate migration patterns before and after the onset of the 2007-2009 recession for the 16 and over population in the CPS, find that one of the main reasons for moving that fell the most among interstate migrants between those two periods was "attend/leave college", and that the decrease in that category as a reason for moving disappears when restricting the sample to respondents ages 35 and older.

economy experienced a long-enough recovery for LFPR to return to its steady-state level, then we would tend to observe disproportionate employment gains for specific groups during those periods, especially relative to economic recoveries that were shorter-lived (e.g. the mid-2000s). However, these gains are not due to the economy running "hotter" than normal, instead they stem from the labor market reaching a full recovery.

9 Conclusion

We estimate the effect of a business cycle shock on the LFPR and show that the LFPR is cyclical, but it responds with a smaller elasticity, a more delayed impact, and a longer recovery than the unemployment rate. Our approach uses state-level variation in business cycles to estimate the cyclicality of LFPR and instruments for changes in state output with a shift-share instrument to establish a causal link between business cycle shocks and the dynamic response of LFPR. We estimate this dynamic response of LFPR to an output shock using the local projections regressions. This method is particularly well-suited for estimating LFPR's cyclicality and its lag structure compared to more traditional time series models, as its flexibility allows for the possibility of long-run effects of a business shock on LFPR, such as hysteresis, and does not impose strict assumption about the smoothness of trends—a particular concern for LFPR given the aging of the population and other longer-term structural change such as the inflow of women into the labor force.

Our results indicate that measuring labor market slack requires looking beyond the unemployment rate. While traditional views hold that the unemployment rate is a sufficient statistic for slack, the long-lived cyclicality of the LFPR poses problems for this view. During the period 5 to 7 years after a shock, the unemployment rate has essentially fully recovered, but the LFPR still has room to rise before it returns to its pre-shock level. Observers who focus solely on the unemployment rate during this period will thus prematurely conclude that the economy has reached full employment.

A complete view of labor market slack requires examining the LFPR in addition to the unemployment rate, and perhaps may go further to include the differential cyclicality of different demographic groups. Long-lived cyclicality is especially prone among younger workers, men, less educated workers, and racial and ethnic minorities, each of which is also more exposed to business cycles through unemployment. Our results indicate that these groups have the most to gain from maintaining business cycle recoveries until the LFPR has fully recovered.

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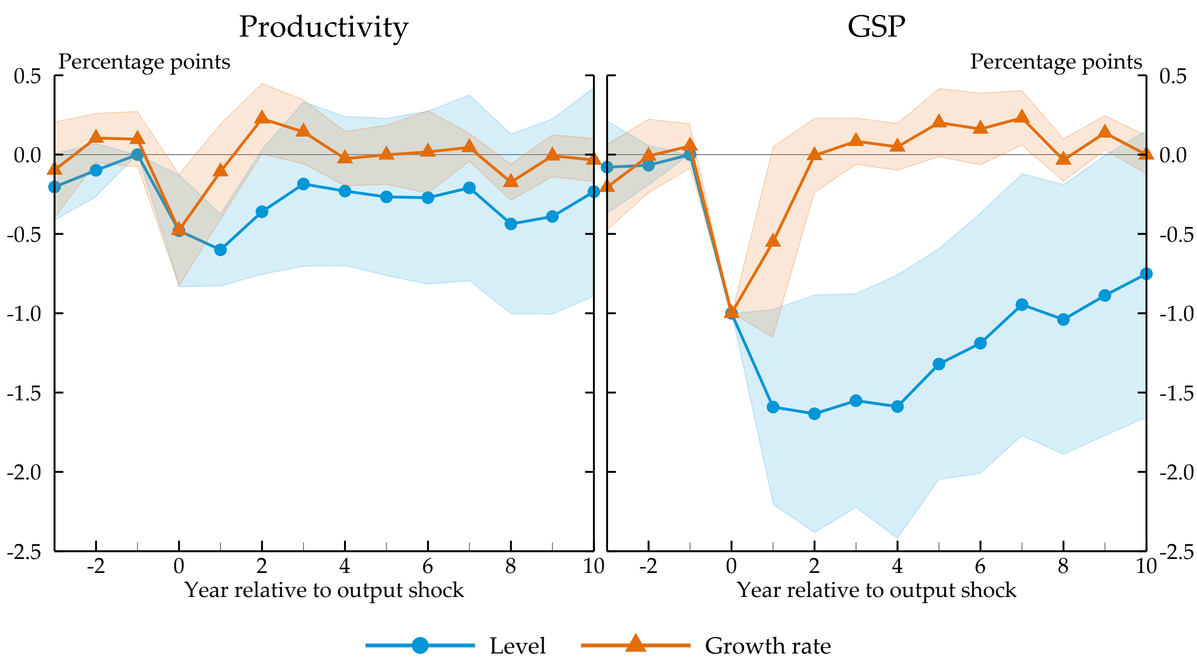
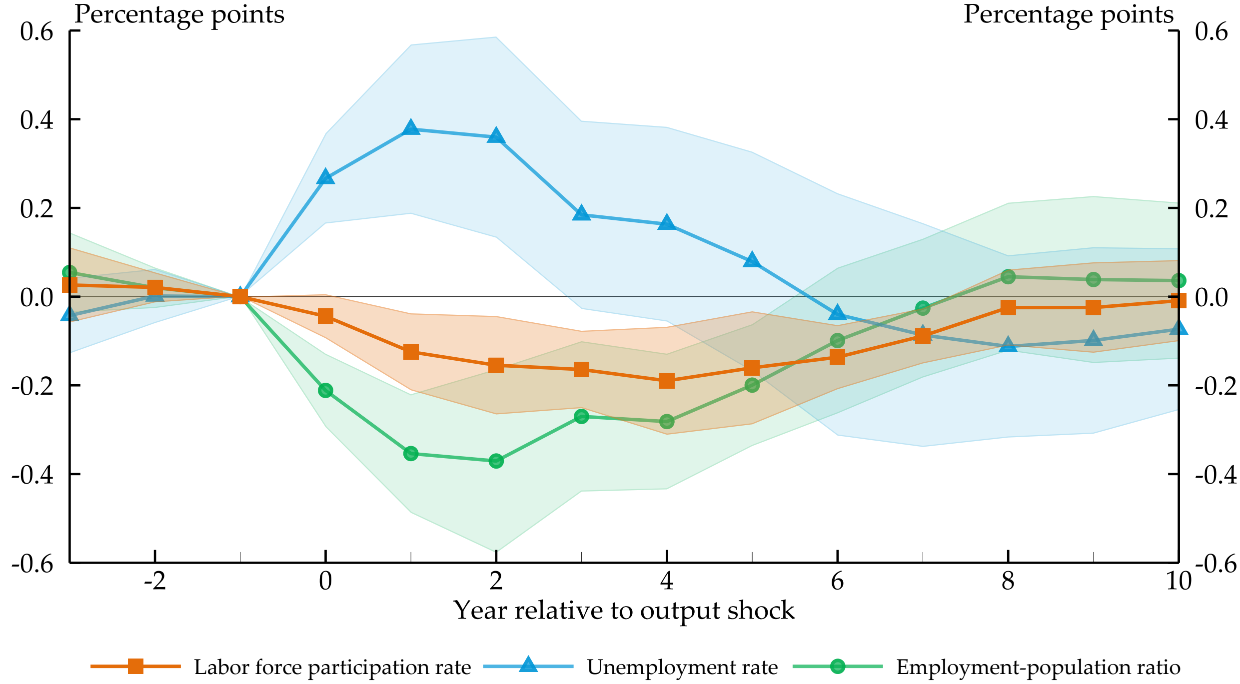
Figure 1: Effects of Shocks on Productivity and Output



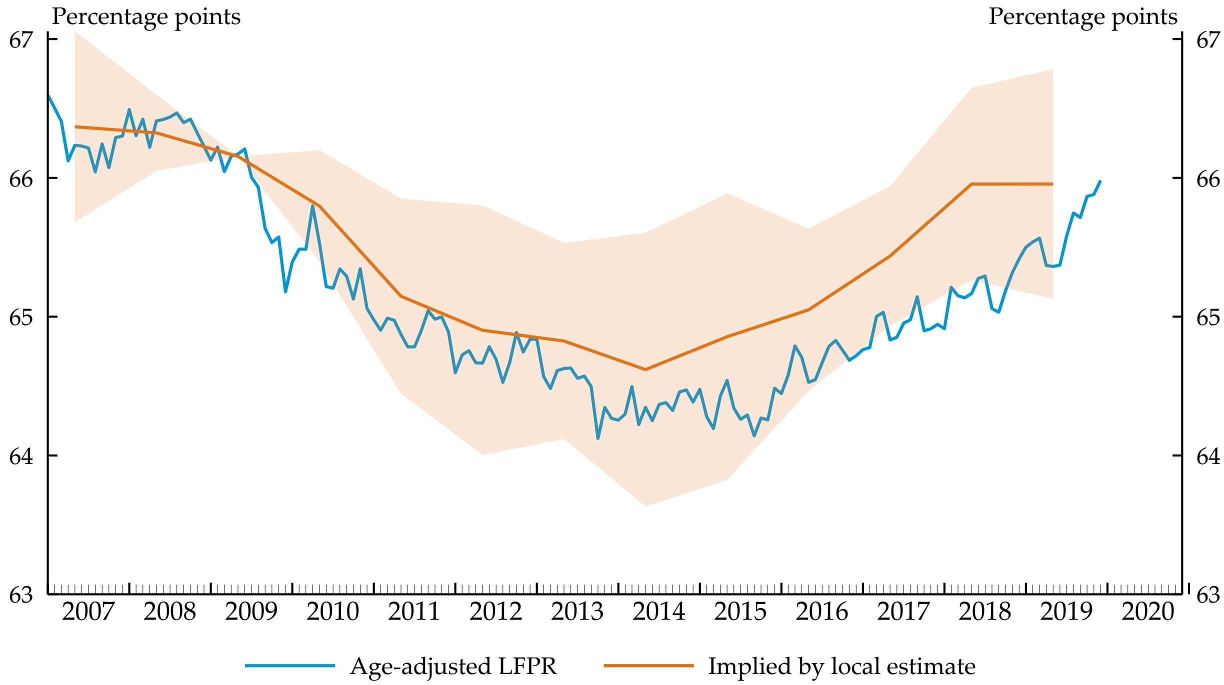
Note: Each line shows the estimated coefficients from Equation 1 for the specified outcome, either in levels relative to year -1 or in growth rates. The bands around each line show a 95% confidence interval, based on standard errors clustered by state. The left panel shows the response of real labor productivity, defined as real GSP per worker. Coefficients are normalized to show the effect of a temporary -1 percentage point shock to GSP growth in year 0. F-statistic: 13.1 for GSP at $k = 1$ (since the F-statistic at $k = 0$ is not defined), 12.8 for productivity at $k = 0$. Regressions control for state and year fixed effects and are weighted by population.
Source: BLS, BEA, and authors' calculations.

Figure 2: Estimated Cyclical Responses to a Negative Output Shock



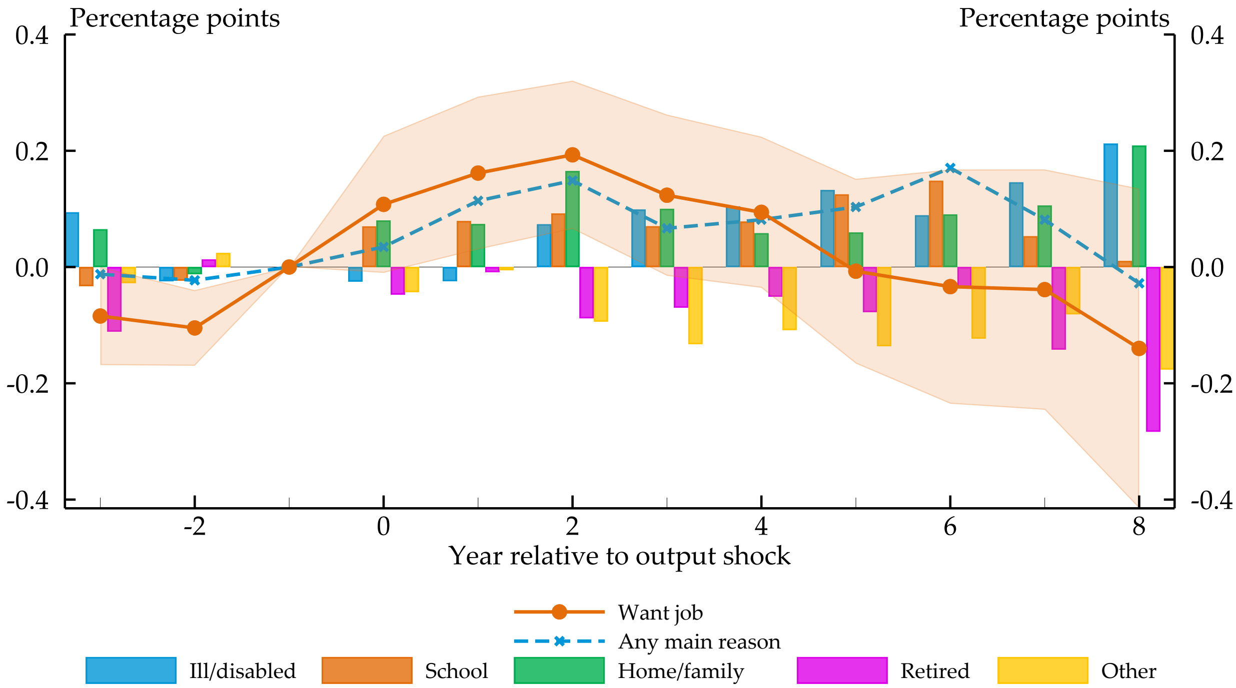
Note: Each line shows the estimated coefficients from Equation 1 for the associated labor market outcome. The bands around each line show a 95% confidence interval, based on standard errors clustered by state. Coefficients are normalized to show the effect of a temporary -1 percentage point shock to GSP growth in year 0. All outcomes are adjusted for changes in the age-by-sex composition of the population. F-statistic for $k = 0$: 12.9. Regressions control for state and year fixed effects and are weighted by population.
Source: BLS, BEA, and authors' calculations.

Figure 3: Actual and Predicted LFPR after the Great Recession



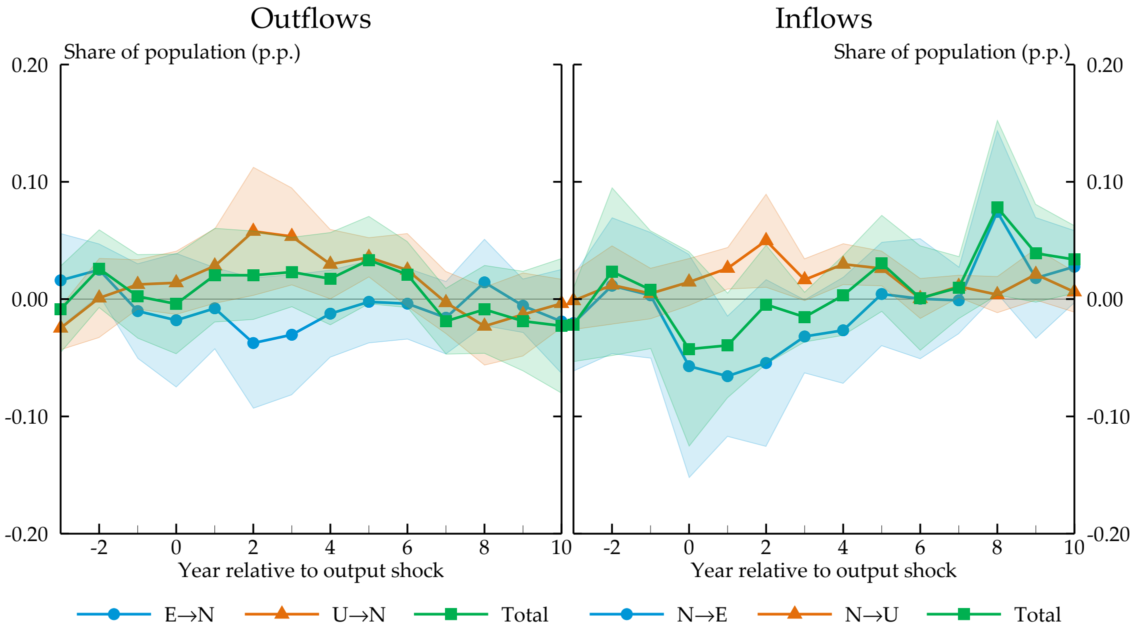
Note: The blue line shows the LFPR adjusted for changes in the age composition of the population since 2007. The orange line uses our main estimates of Equation 1 multiplied by -8.1 p.p., which is the decline in GDP relative to an estimate of potential output (that is, the change in the output gap) during the Great Recession (CBO, 2007). The bands around this line show a 95% confidence interval, based on standard errors clustered by state.
Source: BLS, BEA, and authors' calculations.

Figure 4: Cyclicality by Self-Reported Reason for Labor Force Nonparticipation



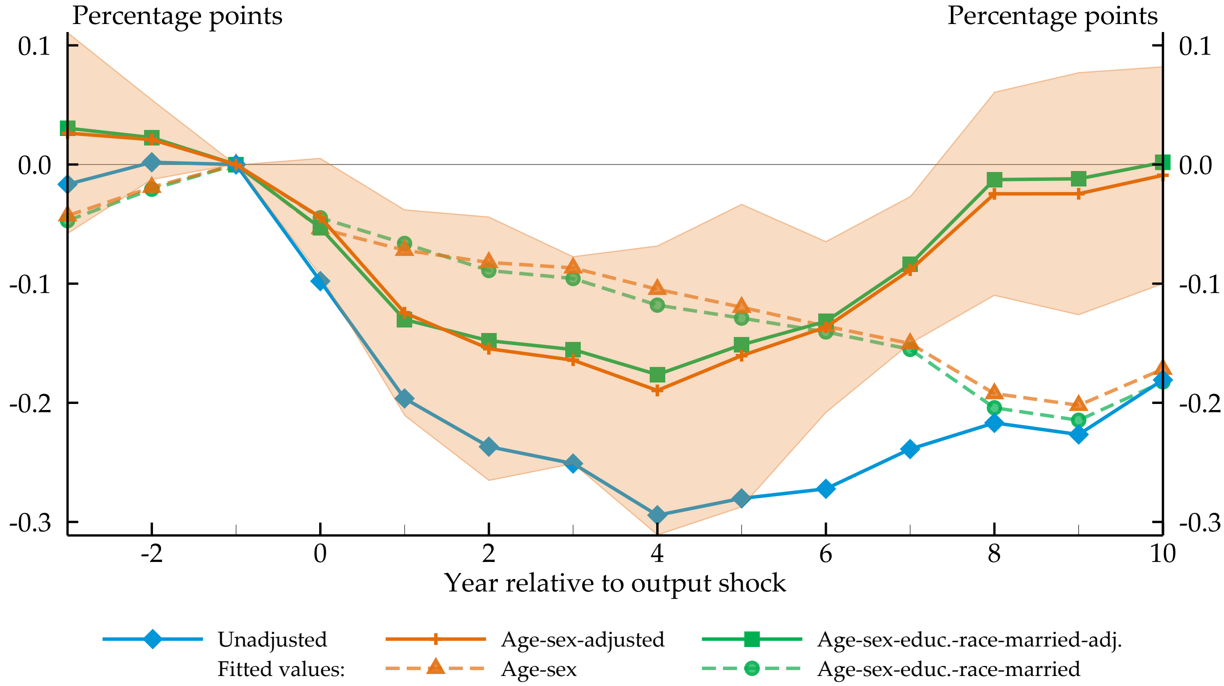
Note: Each line and set of bars shows the estimated coefficients from Equation 1 using as the outcome the share of the population out of the labor force and reporting the specified reason. The band around the orange solid line shows a 95% confidence interval, based on standard errors clustered by state. Reporting "want job" is not exclusive with reporting any of the main reasons. The blue dashed line is equal to the sum of the bars in each period. Coefficients are normalized to show the effect of a temporary -1 percentage point shock to GSP growth in year 0. All outcomes are adjusted for changes in the age-by-sex composition of the population. F-statistic for $k = 0$: 5.0. Regressions control for state and year fixed effects and are weighted by population. Standard errors clustered by state.
Source: BLS, BEA, and authors' calculations.

Figure 5: Estimated Cyclical Responses of Flows to a Negative Output Shock



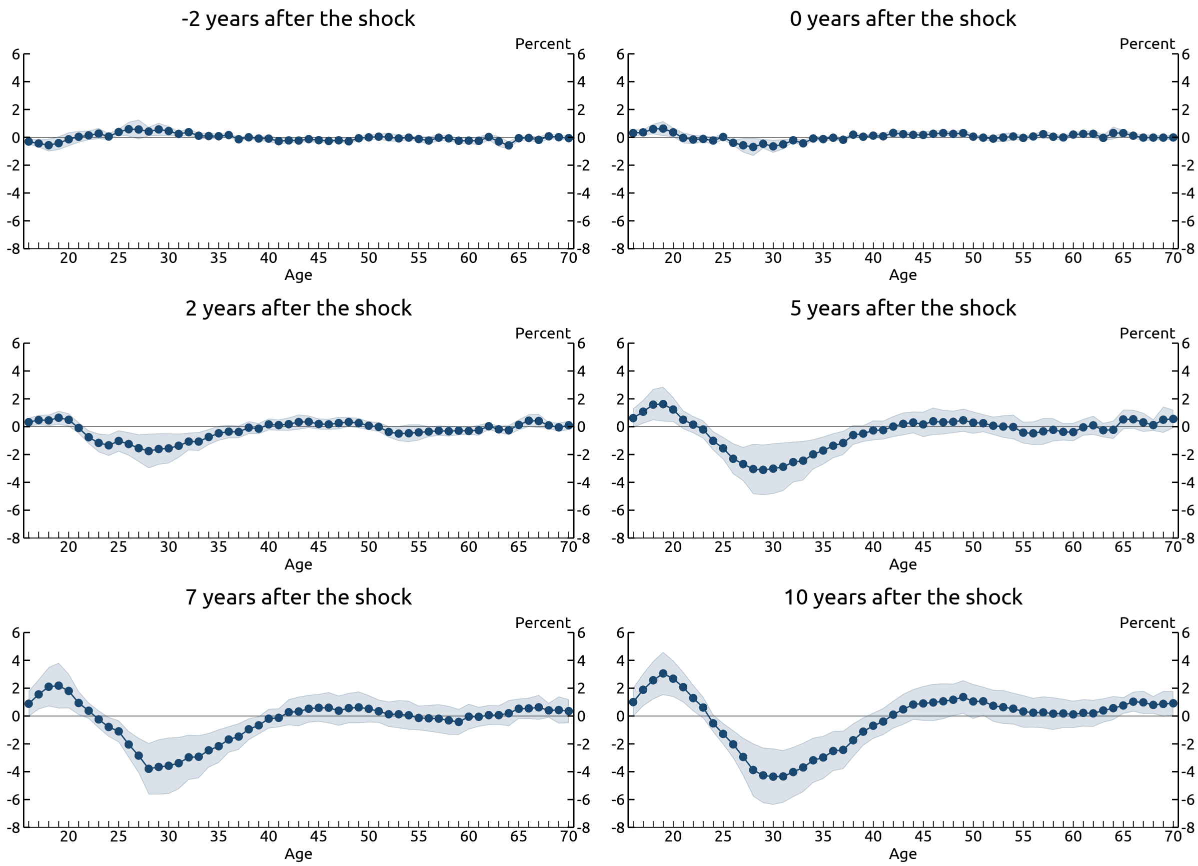
Note: Each line shows the estimated coefficients from Equation 1 for the associated labor market flow. Flows are measured as the share of the population experiencing the specified type of flow from the beginning of a 12-month period to the end. The bands around each line show a 95% confidence interval, based on standard errors clustered by state. Coefficients are normalized to show the effect of a temporary -1 percentage point shock to GSP growth in year 0. All outcomes are adjusted for changes in the age-by-sex composition of the population. F-statistic for $k = 0$: 12.9. Regressions control for state and year fixed effects and are weighted by population.
Source: BLS, BEA, and authors' calculations.

Figure 6: Cyclicality by Demographic Adjustment



Note: Each line shows the estimated coefficients from Equation 1 using the specified adjusted, unadjusted, and fitted-value LFPR as the outcome. The band around the orange solid line shows a 95% confidence interval, based on standard errors clustered by state. Coefficients are normalized to show the effect of a temporary -1 percentage point shock to GSP growth in year 0. F-statistic for $k = 0$: 12.9. Regressions control for state and year fixed effects and are weighted by population.
Source: BLS, BEA, own calculations.

Figure 7: Percent Change in Single-Age Population in Response to a Business Cycle Shock



Note: The dependent variable is the percent change in the population of a single-age group in period $t+k$ relative to period $t-1$. The bands around each line show a 95% confidence interval, based on standard errors clustered by state. Regressions are weighted by population.
Source: BLS, BEA, and authors' calculations.

FOR ONLINE PUBLICATION

Appendix A Differences in Long-Lived Cyclicality Across Groups

In this section, we examine how the cyclicality of the LFPR varies across the age, gender, education, and race/ethnicity distributions. Comparing young workers to older workers, men to women, and less-educated people to more-educated people, we find the LFPR for each former group is both more cyclical and features longer-lived cyclicality. These differences in long-lived cyclicality may create differential benefits for these groups from "running the economy hot" in years 5 to 7 after a shock, when the unemployment rate has fully recovered but the LFPR is still recovering (Aaronson et al., 2019).

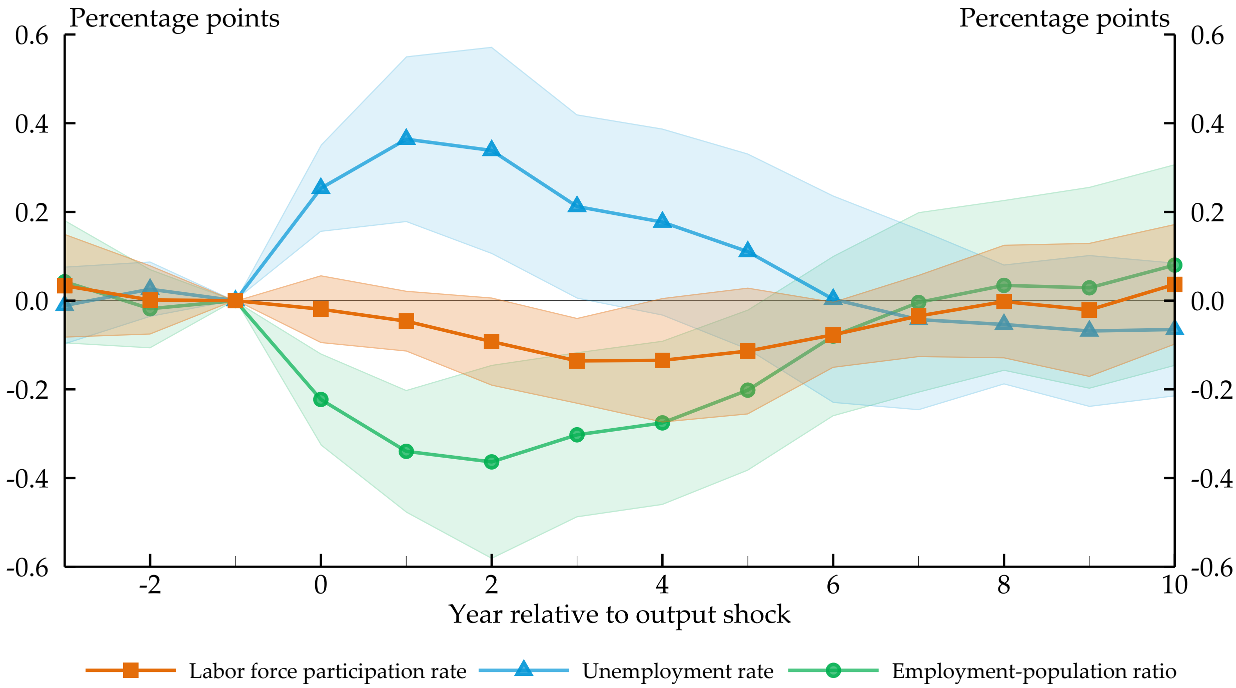
A.1 Age

The labor market performance of prime-age people (ages 25 to 54) is often used as a benchmark for the cyclical state of the labor market as a whole. Understanding the cyclical response for the prime-age group is of considerable interest, as prime-age people make up about 50 percent of the 16 and over civilian non-institutional population and roughly 60 percent of the labor force. Further, much work has focused on the structural factors contributing to the long-run and steady decline of the trend prime-age LFPR and EPOP (see, for example, Abraham and Kearney (2020) and Coglianese (2018)), but there has been relatively less work on identifying the cyclical response of those variables from their long-run declining trends.[1]

The cyclical response of the prime-age LFPR is similar to the overall response, albeit a bit smaller in magnitude. Appendix Figure A.1 shows the estimated impulse response

[1] Although the main purpose of Aaronson et al. (2014) and Montes (2018) is to build a forecasting model of the overall LFPR, both papers provide some evidence on the cyclicality of prime-age LFPR. Our work complements those papers in that we establish a causal response to output shocks, whereas those estimates were largely based on correlations with changes in the unemployment rate.

Appendix Figure A.1: Cyclicality for Ages 25 to 54



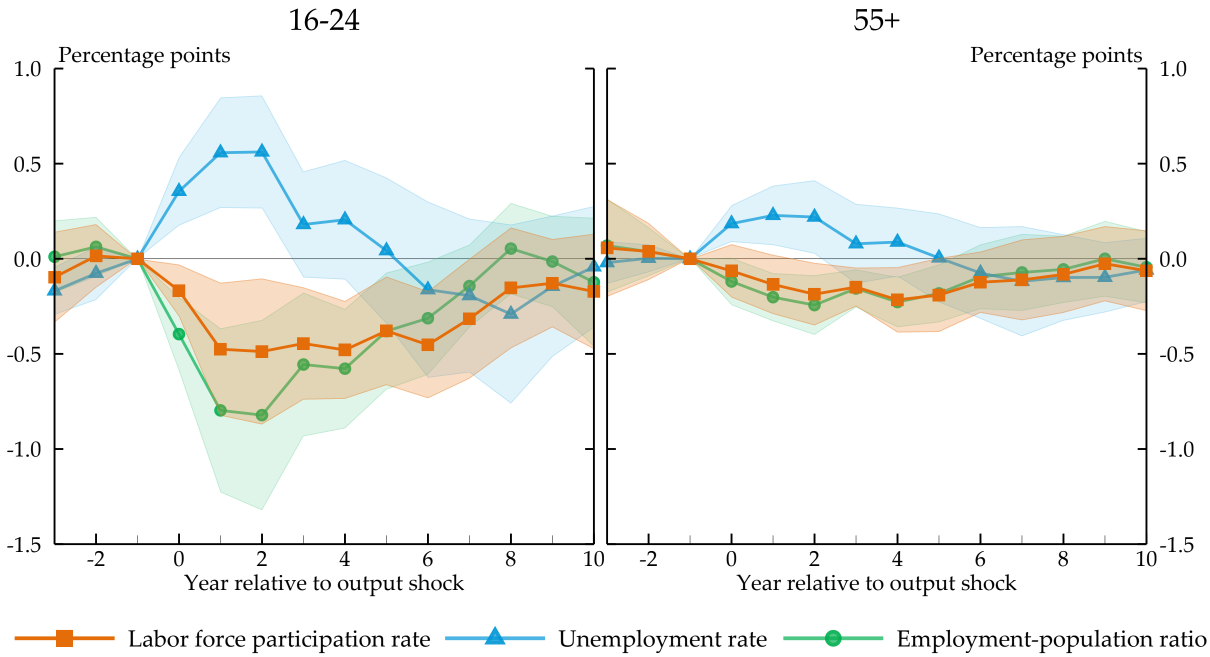
Note: Each line shows the estimated coefficients from Equation 1 for the associated labor market outcome. The bands around each line show a 95% confidence interval, based on standard errors clustered by state. Coefficients are normalized to show the effect of a temporary -1 percentage point shock to GSP growth in year 0. F-statistic for $k = 0$: 12.4. Regressions control for state and year fixed effects and are weighted by population.
Source: BLS, BEA, own calculations.

for the prime-age LFPR, along with unemployment rate and EPOP.[2] The LFPR declines steadily after the shock until it reaches its trough four years after the initial shock—well after the unemployment rate peaks—at about 0.14 percentage point below its pre-shock level, before gradually recovering and reaching its pre-shock level in year eight.

Compared to prime-age people, the LFPR for younger people (ages 16 to 24) responds more quickly and with a larger amplitude—reaching a trough of about -0.5 percentage point—but remains near its trough for many years and begins recovering later (Appendix Figure A.2, left panel). The point estimate of the LFPR of younger people never fully

[2]Unlike our baseline results, we do not use age-sex-adjusted participation rates for these subgroups. However, the results are very similar if we age-sex-adjust the LFPRs within each age range. This is a consequence of the fact that changes in the demographic composition of the population mainly reflect changes across these age groups, rather than changes within them.

Appendix Figure A.2: Cyclicality for Ages 16-24 and 55+



Note: Each line shows the estimated coefficients from Equation 1 for the associated labor market outcome. The bands around each line show a 95% confidence interval, based on standard errors clustered by state. Coefficients are normalized to show the effect of a temporary -1 percentage point shock to GSP growth in year 0. F-statistic for $k = 0$: 14.8 for 16–24, 12.6 for 55+. Regressions control for state and year fixed effects and are weighted by population.
Source: BLS, BEA, own calculations.

recovers, as it settles at about 0.2 percentage point below its pre-shock value, although the upper end of the confidence interval suggests we cannot rule out a full recovery. The delayed recovery of the LFPR for younger people likely reflects the increase in time spent in schooling documented in Section 6.

The LFPR response for older people is similar to the response of the overall population, reaching its trough at about 0.2 percentage point four years after the shock (Appendix Figure A.2, right panel). The LFPR for older people then begins to steadily recover 5 years after the shock and does not fully recovery until 9 years after the shock. For this age group, the shortfall of participation at its trough is likely due to higher rates of illness and disability, with no increase in retirements.

A.2 Gender

Digging deeper into the prime-age LFPR responses, our results suggest that while both men and women have strong cyclicality, the magnitudes and timing of their responses are quite different. For men, the initial point estimate response shown in the left panel of Appendix Figure A.3 is small, and subsequent year-over-year declines are also small. However, even though those yearly declines are small, they compound for many years after the shock, cumulating to a total decline in the LFPR of about 0.15 percentage point at its trough 6 years after the shock. Although the confidence bands around those estimates are large due the smaller sample sizes from splitting the prime-age group by gender, the decline in the prime-age LFPR for men is large enough in year 6 for the confidence band to not include zero.

The response of LFPR for prime-age women is considerably delayed. In fact, the LFPR of prime-age women does not start to decline until 2 years after the shock and reaches its trough 3 to 4 years after the shock at about 0.1 percentage point below its initial value. This rate fully recovers by about 6 years after the shock and settles at rate slightly above its pre-shock value. Of course, the confidence bands around the estimates for prime-age women are quite large, possibly due to large non-cyclical variation in the LFPR for prime-age women, and so one cannot reject the possibility that the LFPR of prime-age women does not respond to the shock at all.

A.3 Education

Labor market outcomes over at least the past 40 years have been quite different for less- and more-educated people. Indeed, the levels of the unemployment rates, LFPRs, and EPOPs for prime-age workers vary significantly across levels of educational attainment for both men and women. Additionally, the prime-age LFPR and EPOP for less-educated people have been declining steadily over the past several decades, while the LFPR and

Appendix Figure A.3: Cyclicality for Ages 25 to 54 by Sex

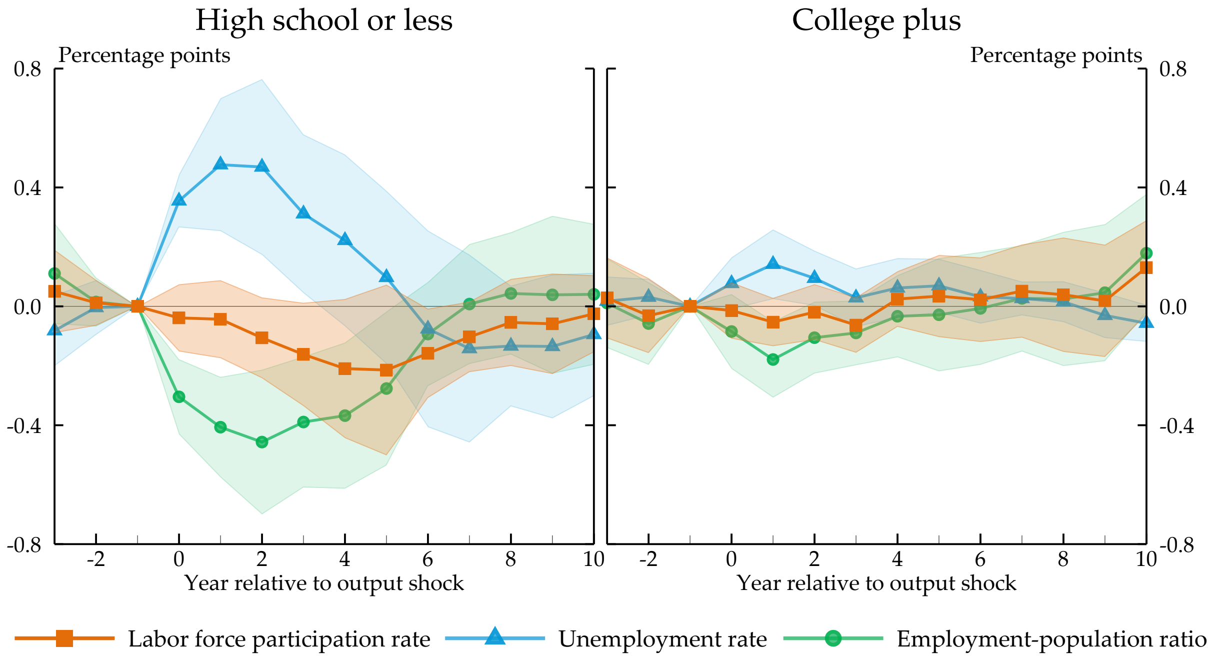
Note: Each line shows the estimated coefficients from Equation 1 for the associated labor market outcome. The bands around each line show a 95% confidence interval, based on standard errors clustered by state. Coefficients are normalized to show the effect of a temporary -1 percentage point shock to GSP growth in year 0. F-statistic for $k = 0$: 12.4 for men, 12.4 for women. Regressions control for state and year fixed effects and are weighted by population.
Source: BLS, BEA, own calculations.

EPOP for more-educated prime-age people were relatively flat. Those trends have led to a growing divergence in labor market outcomes between the most and least educated individuals.

This divergence may, at least in part, be due to a long-term decline in the demand for lower-educated workers that is unrelated to the business cycle and caused, perhaps, by changes in technology and globalization. Thus, to isolate cyclicality one needs to control for these long-term structural declines. Our approach using state-level business cycles and controlling for these national and international trends is well suited to isolate the effects of the business cycle and explore how they differ across education groups.

We find a starkly different evolution of the LFPR after a shock for less-educated prime-age workers compared to those with at least college degrees, as shown in Appendix Fig-

Appendix Figure A.4: Cyclicality for Ages 25 to 54 by Education



Note: Each line shows the estimated coefficients from Equation 1 for the associated labor market outcome. The bands around each line show a 95% confidence interval, based on standard errors clustered by state. Coefficients are normalized to show the effect of a temporary -1 percentage point shock to GSP growth in year 0. F-statistic for $k = 0$: 14.6 for high school degree or less, 9.8 for college degree or more. Individuals with some college but less than a four year degree are omitted. Regressions control for state and year fixed effects and are weighted by population.
Source: BLS, BEA, own calculations.

ure A.4. For workers with a high school degree or less, the shock leads to a slow decline of the LFPR for about 5 years, reaching a trough of about 0.25 percentage point, before recovering subsequently. In contrast, workers with a college degree experience essentially no variation in LFPR following a shock.[3] This disparity is also found in the responses of the unemployment rate and EPOP, each of which respond substantially among the less-educated group but barely at all among the more-educated group.

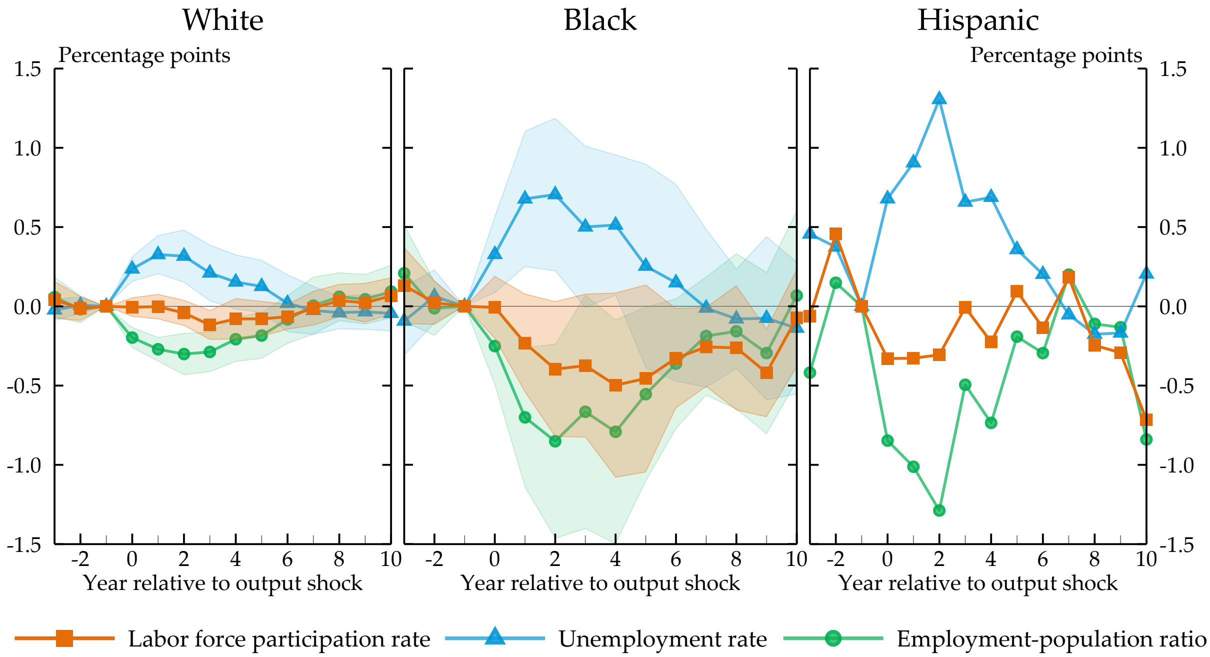
[3]We omit workers with some college but less than a four year degree for ease of comparison. The labor market response of this group falls in between the two groups shown here, closer to the less-educated group than to the more-educated group.

A.4 Race and Ethnicity

We also investigate the inequality of long-lived LFPR cyclicality across race and ethnicity. As has been noted by Cajner et al. (2017) and others, business cycles are more costly for minority groups. We divide prime-age people in the CPS into racial and ethnic groups and estimate Equation 1 for each group, showing the results in Appendix Figure A.5.

We find that shocks lead to larger and more long-lived declines in LFPR among minority groups. While the white LFPR falls by only 0.1 percentage point after a shock, the Black LFPR falls by 0.5 percentage point. The Black LFPR remains depressed for substantially longer, and only fully recovers ten years after the shock, well after the white LFPR has recovered. The responses for Hispanic workers are also large, although our results for this group are much noisier due to a lower-powered instrument when weighting states by the Hispanic population.

Appendix Figure A.5: Cyclicality for Ages 25 to 54 by Race/Ethnicity



Note: Each line shows the estimated coefficients from Equation 1 for the associated labor market outcome. The bands around each line show a 95% confidence interval, based on standard errors clustered by state. Individuals not reporting either white, Black, or Hispanic are omitted. Coefficients are normalized to show the effect of a temporary -1 percentage point shock to GSP growth in year 0. F-statistic for $k = 0$: 18.3 for white, 7.3 for Black, 0.7 for Hispanic. Confidence interval for Hispanic not shown due to low F-statistic. Regressions control for state and year fixed effects and are weighted by population.

Source: BLS, BEA, own calculations.

Appendix B What Drives the Shocks?

We examine the driving forces behind our output shock. We find similar responses to contractionary and expansionary shocks, suggesting that our effects are not being driven by asymmetries. More of the variation in our shocks comes from the pre-1994 period, with estimates using only post-1994 data being similar overall but substantially noisier. The variation in the shift-share instrument is driven primarily by a handful of industries including motor vehicle production, oil and gas extraction, securities and commodities brokers, and farms, but our estimated effects are similar if these industries are excluded. Overall, we find that our results are not being driven by a single source of variation, and instead reflect common responses to shocks in a wide variety of environments.

B.1 Expansions vs. Contractions

Our estimated impulse responses are an average of the effects of expansionary and contractionary shocks, which may not be informative if these effects are starkly different. To examine whether expansionary and contractionary shocks have different effects, we divide the distribution of shocks into thirds and estimate the impulse responses separately for each third. In the left panel of Appendix Figure B.1, we present the effects of expansionary shocks (top third) and contractionary shocks (bottom third), normalizing both to show the effect of a negative 1 percentage point shock. Both impulse responses have similar patterns, and we cannot reject that the two are the same. This result suggests that our baseline estimates, which combine the response of both expansionary and contractionary shocks, are a reasonable guide for a wide range of shocks.

B.2 Differences over Time

Our instrument also combines variation over time, including periods with different macroeconomic dynamics. Business cycles since 1990 have been characterized by jobless recoveries (Jaimovich and Siu, 2020), while earlier periods included more rapid recoveries in the labor market. Additionally, our CPS sample includes data both before and after the 1994 redesign, which substantially changed how the survey was collected.

To test whether the cyclicality of the LFPR has changed over time, we divide our sample into pre- and post-1994 periods. For each period, we separately estimate the impulse response and plot these estimates in the right panel of Appendix Figure B.1. Although the post-1994 estimates are substantially noisier, the two point estimates are similar and we cannot rule out that the two are the same. This suggests that most of the variation in the instrument in our baseline estimates comes from the earlier period, but it does not exclusively drive our estimates.

B.3 Decomposing the Shift-Share Instrument

To further examine where the variation in our shift-share instrument comes from, we decompose the variation using the approach of Goldsmith-Pinkham, Sorkin and Swift (2020). For simplicity, we focus on the response of the LFPR four years after the shock, which is the point that it reaches its trough in our main estimates. To compute the Rotemberg weights for each industry-year pair, we compute

$$\hat{\alpha}_{kt} = \frac{g_{kt}\zeta'_{kt}\Delta GSP^{\perp}_{t,t-1}}{\sum_{k'}\sum_{t'} g_{k't'}\zeta'_{k't'}\Delta GSP^{\perp}_{t,t-1}}, \quad \hat{\beta}_{kt} = \frac{\zeta'_{kt}\Delta \text{LFPR}^{\perp}_{t+4,t-1}}{\zeta'_{kt}\Delta GSP^{\perp}_{t,t-1}}, \quad \hat{\beta} = \sum_k \sum_t \hat{\alpha}_{kt}\hat{\beta}_{kt} \tag{10}$$

where $\Delta GSP^{\perp}_{t,t-1}$ is GSP growth and $\Delta \text{LFPR}^{\perp}_{t+4,t-1}$ is the cumulative change in LFPR by four years after shock, both residualized on state and year fixed effects, ζ'_{kt} is the lagged industry share for industry k in year t, and g_{kt} is the national growth rate of industry k in year t. We depart from our baseline specification in using the national growth rate for g_{kt}, instead of the leave-one-out growth rate, in order to align with the calculation of Rotemberg weights.[4]

Importantly, we treat each industry and year as a distinct instrument, using the variation from the shares to identify each effect. Our baseline estimate is a weighted average of these effects, where the weights are the Rotemberg weights outlined above.

Much of the variation in the shift-share instrument comes from a small number of industry-year instruments. Panel (a) of Appendix Table B.1 shows the top 10 industry-year instruments, along with their weights $\hat{\alpha}_{kt}$ and estimated effects $\hat{\beta}_{kt}$. The instruments

[4]Our baseline results are little changed using an instrument constructed from the national growth rate for each industry instead of the leave-one-out growth rate.

contributing the most weight include shocks to oil & gas extraction during the 1980s, as well as shocks to motor vehicle production and securities during recessions. Collectively, the top 10 instruments account for about 62 percent of the total weight. Most of the shocks have estimated βs close to our main estimate, including the total of shocks outside the top 10. In this way, no single shock drives our result.

We also aggregate the weights to show the most important industries, pooling across time periods, and the most important time periods, pooling across industries. Panel (b) of Appendix Table B.1 shows that 3/4 of the shift-share instrument variation comes from just four industries—motor vehicle production, oil & gas extraction, securities & commodities brokers, and farms. Nonetheless, these industries do not exclusively drive our result, as the estimated effect pooling across all other industries is 0.18, very close to our baseline estimate. Panel (c) of Appendix Table B.1 shows that our instrument derives a substantial amount of variation from recessions, with the top 10 years including at least one year from each of the five national recessions that took place during our sample period, but also includes variation from non-recessionary years. Almost all years have coefficients close to our baseline estimate, indicating that our estimates are not being driven by a single year or recession.

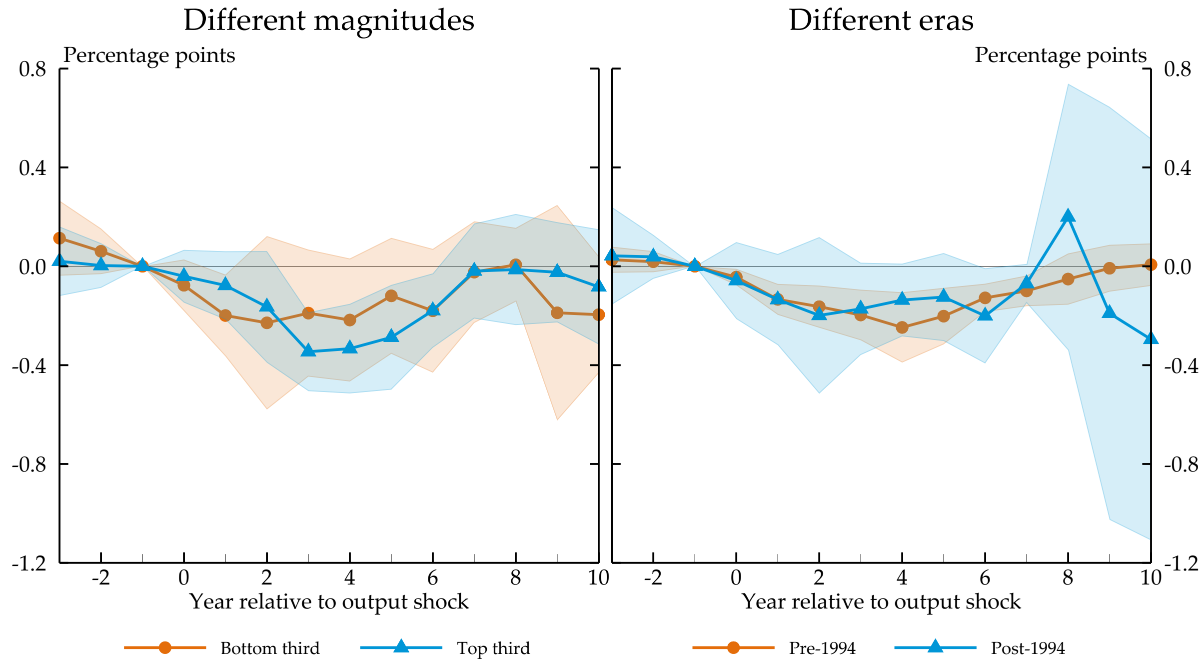
Appendix C Robustness

In this section, we show several robustness checks for our methodology.

C.1 Lead-Lag Exogeneity

One of the conditions required for our research design to identify the impulse response of the LFPR is that the instrument satisfies lead-lag exogeneity, as laid out in Equation 6 (Stock and Watson, 2018). A necessary, though not sufficient, condition for lead-lag exogeneity is that the instrument should be uncorrelated with leads and lags of itself, which

Appendix Figure B.1: Cyclical Responses to Different Types of Shocks



Note: Each line shows the estimated coefficients from Equation 1 for the LFPR, using only the specified sample of shocks. The bands around each line show a 95% confidence interval, based on standard errors clustered by state. Coefficients are normalized to show the effect of a temporary -1 percentage point shock to GSP growth in year 0. In all specifications, the LFPR is adjusted for changes in the age-by-sex composition of the population. F-statistic for $k = 0$: 14.1 (bottom-third), 4.7 (top-third), 21.2 (pre-1994), 2.6 (post-1994). Regressions control for state and year fixed effects and are weighted by population.
Source: BLS, BEA, and authors' calculations.

we can test empirically. Given that our instrument is based on industry growth rates and shares, which can be persistent over time, there is some potential for the instrument to be correlated with leads and lags of itself.

To examine whether our instrument is correlated with its leads and lags, we estimate Equation 1 using our shift-share instrument as the outcome variable. This impulse response is reported in the panel (a) of Appendix Figure C.1. The coefficient in period 0, 2.71, is the inverse of our first stage coefficient, γ, and is highly statistically significant as a result. Importantly, though, all of the other coefficients are close to zero and almost all of them are statistically indistinguishable from zero.[5]

[5] Although our instrument is constructed from industry shares and growth rates, which can be highly

Appendix Table B.1: Rotemberg weights in GSP Shift-Share Instrument

(a) By Industry/Year

Industry	Year	α_{kt}	β_{kt}
Oil & gas	1986	0.13	0.40
Oil & gas	1980	0.12	0.24
Securities	2009	0.08	0.10
Motor vehicles	2010	0.07	0.07
Motor vehicles	1980	0.05	0.09
Oil & gas	1981	0.04	0.35
Motor vehicles	2009	0.03	-0.05
Motor vehicles	1983	0.03	0.07
Oil & gas	1983	0.03	0.21
Motor vehicles	1992	0.02	0.45
All other	All other	0.38	0.16

(b) By Industry

Industry	α_k	β_k
Motor vehicles	0.30	0.09
Oil & gas	0.28	0.38
Securities	0.12	0.11
Farms	0.06	0.13
Primary metals	0.03	-0.19
Computers & electronics	0.02	0.04
Trans. eq. excl. motor veh.	0.02	0.20
Federal govt. - military	0.02	0.60
State & local govt.	0.02	0.29
Chemicals	0.01	0.11
All other	0.12	0.18

(c) By Year

Year	α_t	β_t
1980	0.22	0.16
1986	0.17	0.35
1983	0.10	0.20
2009	0.10	-0.02
2010	0.07	0.12
1982	0.07	0.14
1992	0.04	0.18
2001	0.04	0.50
1994	0.03	0.04
1981	0.03	0.21
All other	0.14	0.22

Note: Tables show the Rotemberg weights for the GSP shift-share instrument used in our main estimates. Each panel shows the top 10 Rotemberg weights in each category, along with the total among all non-top-10 entries. Outcome is the change in the LFPR four years after the shock; the total effect is equal to 0.19 in our main specification using the non-leave-one-out version of the instrument.
Source: BLS, BEA, and authors' calculations.

C.2 Placebo

We cluster our standard errors at the state level in our baseline estimates, but Adão, Kolesár and Morales (2019) point out that this may be insufficient in some circumstances. Our instrument exploits variation across places with different industry exposure, and the residuals for states with similar industry exposure may be correlated. Our clustering approach does not exactly capture this structure, raising a concern that our standard errors may be incorrect.

We examine the relevance of this critique for our setting using a placebo exercise similar to the one proposed by Adão, Kolesár and Morales (2019). In place of our shift-share instrument, we estimate the reduced-form version of our main specification using a placebo shift-share instrument, where the national growth rates of each industry have been replaced with random draws from a normal distribution with the same mean and variance as the observed growth rates. We repeat this procedure 100 times, obtaining a

persistent unconditionally, the fact that we are controlling for state and year fixed effects means that our variation comes from residual variation in industry shares and growth rates conditional on these fixed effects, which does not feature the same persistence. In particular, the inclusion of year fixed effects removes business cycle variation from industry growth rates, which can be more persistent than idiosyncratic industry-level shocks.

placebo estimate for each, and report the distribution of these placebo estimates along with our baseline in section I in panel (b) of Appendix Figure C.1. Unlike the cases examined by Adão, Kolesár and Morales (2019), we find that the spread of placebo estimates is similar to or a bit smaller than the confidence intervals obtained from standard errors clustered at the state level. This result suggests that our approach to inference is valid, and if anything is a bit conservative.

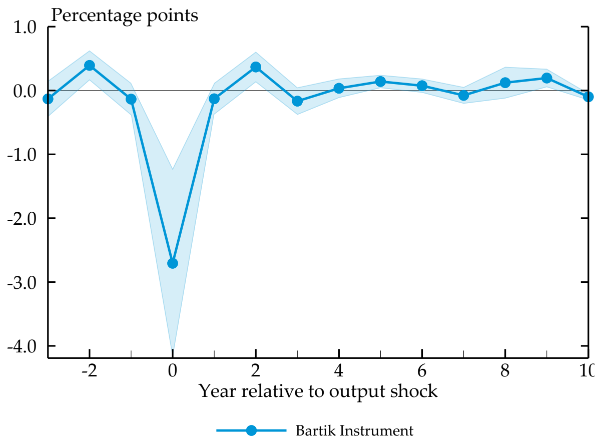
C.3 Local Projections vs. VAR

A key departure of our approach from the literature is the use of local projections regressions instead of a VAR to estimate impulse response functions. Both Blanchard and Katz (1992) and Dao, Furceri and Loungani (2017) use VAR methods to estimate impulse responses and find roughly similar cyclical timing for the unemployment rate and LFPR. However, VAR methods can fail to identify the correct impulse responses even when the instrument conditions are met if the impulse responses are not invertible, but local projections do not require this assumption for identification (Stock and Watson, 2018).

To test whether VAR methods are appropriate for our setting, we conduct a test of invertibility following Stock and Watson (2018). This is a Hausman (1978)-type test, where, under the null hypothesis of invertibility, both methods should deliver similar estimates but with VAR estimates more efficient, while under the alternative they would return different estimates. We report the test statistic in section II in panel (b) of Appendix Figure C.1 along with the associated p-value. We are able to strongly reject the null hypothesis of invertibility, implying that local projections are the only suitable method for examining the cyclicality of LFPR with our approach.

Appendix Figure C.1: Robustness Checks

(a) Leads/lags of instrument



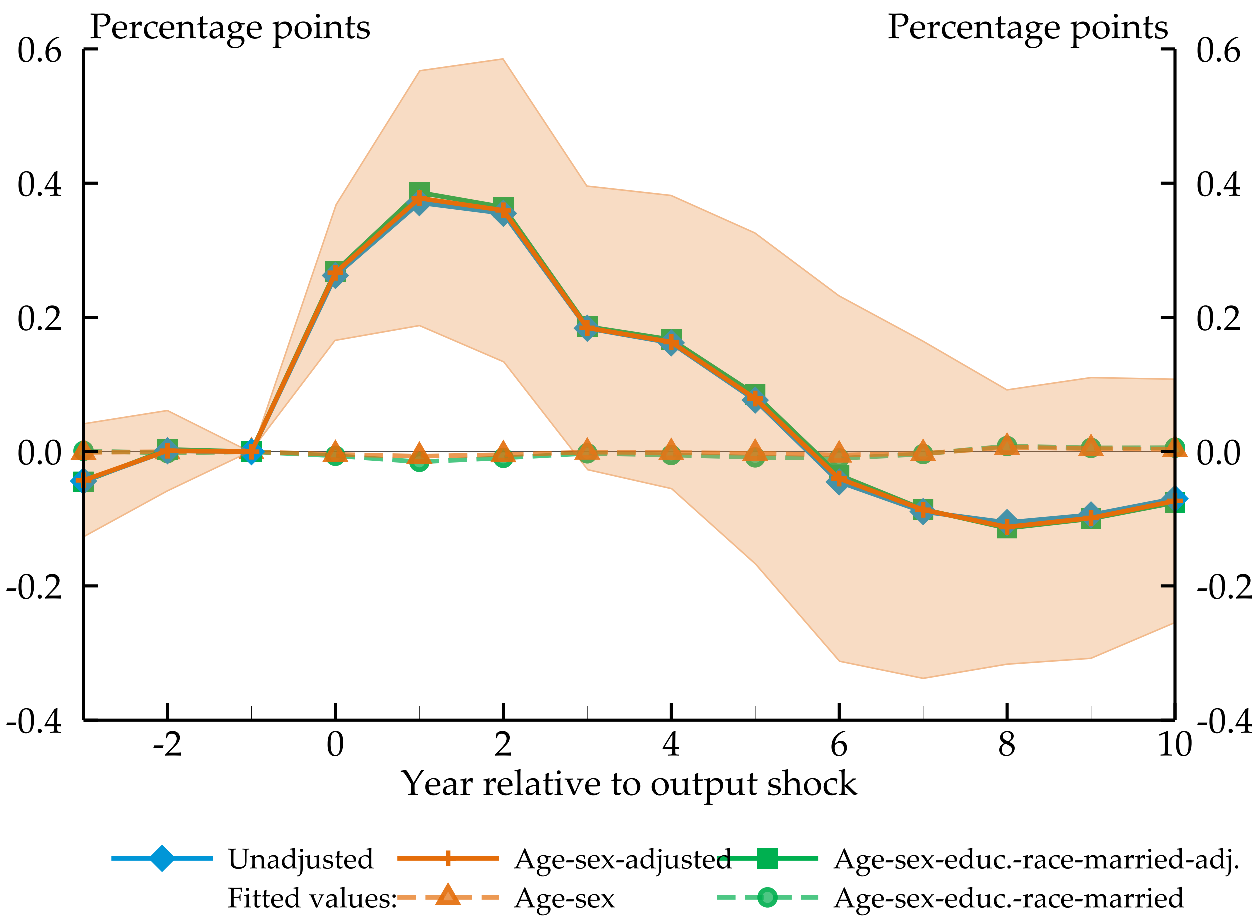
(b) Additional robustness checks

Section I - Placebo	
Main estimate (reduced form)	0.080
	(0.014)
	[0.052,0.108]
Placebo	0.0098
	(0.012)
	[-0.022,0.023]
Section II - Test of invertibility	
Test statistic	750.8
p-value	0

Note: In the left panel, the line shows the estimated coefficients from Equation 1 using the shift-share instrument as the outcome, and the band around the line shows a 95% confidence interval, based on standard errors clustered by state. Panel (b), section I, shows the estimated response of the age-sex-adjusted LFPR four years after a shock that uses the placebo shift-share instrument described in Appendix C.2. The 95% confidence interval is shown in brackets; for the placebo specification this is the empirical confidence interval taken from the 2.5th percentile to the 97.5th percentile across placebo estimates. Standard errors clustered by state are shown in parentheses. Panel (b), section II, shows the results of the Stock and Watson (2018) test of invertibility. Regressions control for state and year fixed effects and are weighted by population.
Source: BLS, BEA, FHFA, and authors' calculations.

Appendix D Additional Results

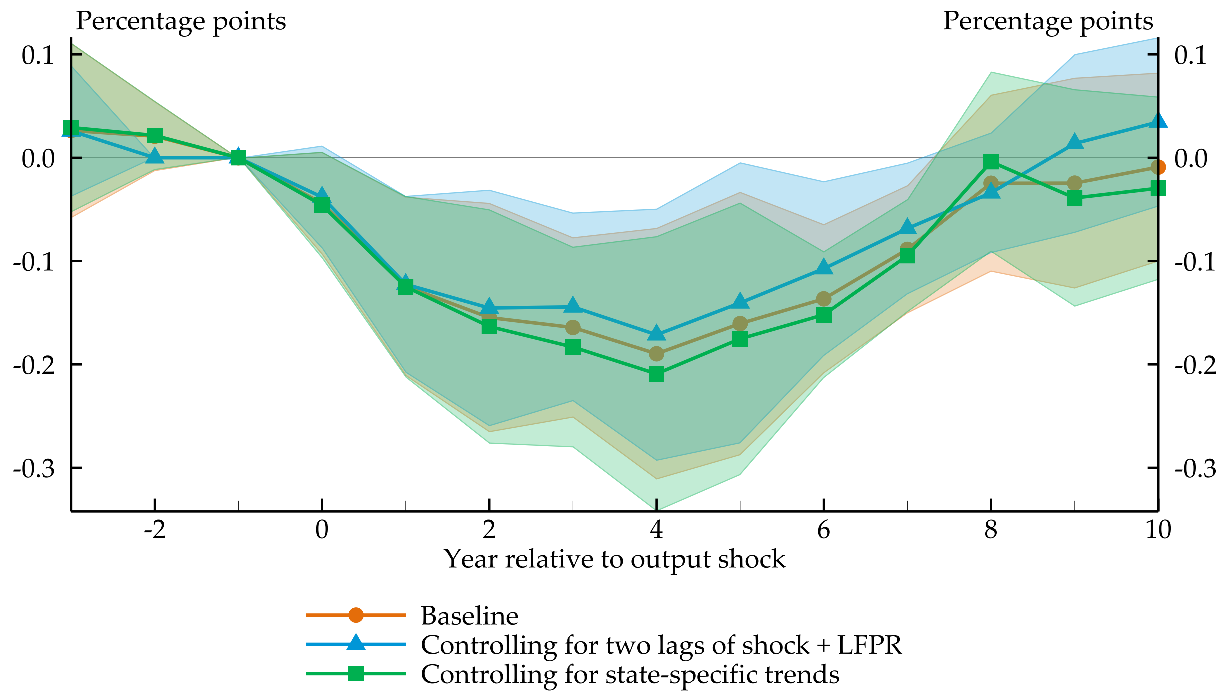
Appendix Figure D.1: Unemployment Rate Cyclicality by Demographic Adjustment



Note: Each line shows the estimated coefficients from Equation 1 using the specified adjusted/unadjusted LFPR or fitted values as the outcome. The band around the orange solid line shows a 95% confidence interval, based on standard errors clustered by state. Coefficients are normalized to show the effect of a temporary -1 percentage point shock to GSP growth in year 0. F-statistic for $k = 0$: 12.9. Regressions control for state and year fixed effects and are weighted by population.

Source: BLS, BEA, own calculations.

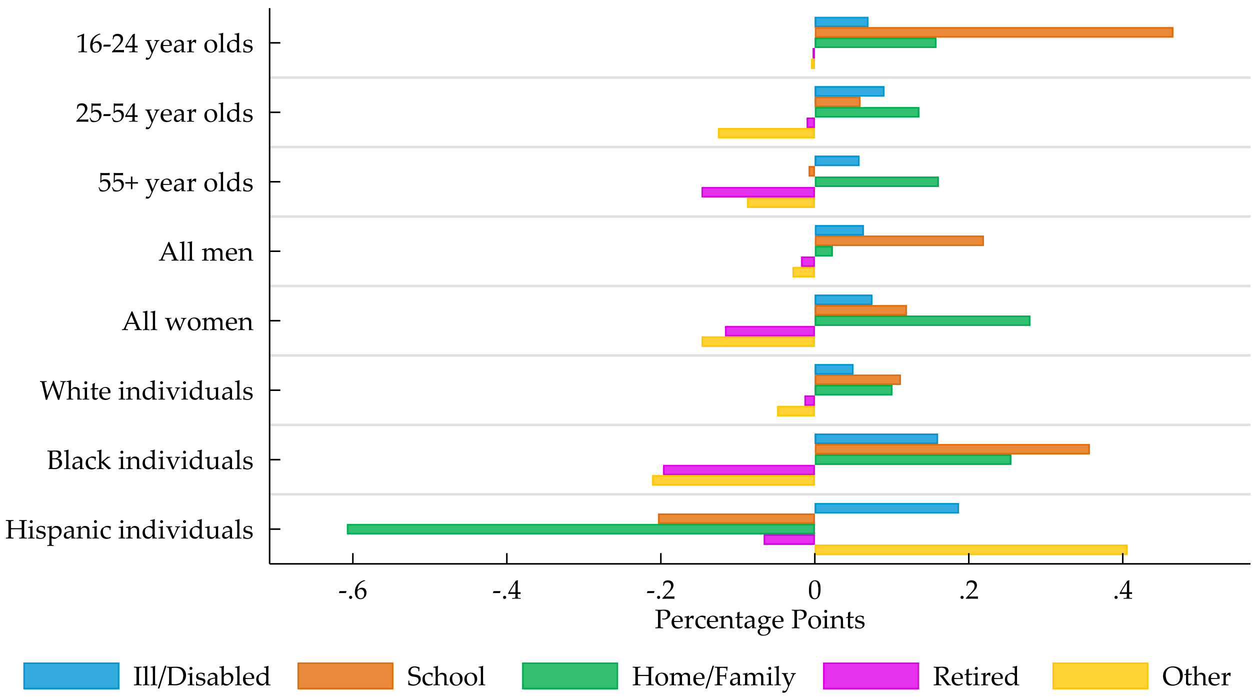
Appendix Figure D.2: Robustness to Additional Controls



Note: The orange line shows the estimated coefficients from our baseline specification of Equation 1. The blue line shows estimated coefficients from a specification where we additionally control for two lags of the instrument and two lags of the residualized LFPR. The green line shows estimated coefficients from a specification where we instead add controls for state-specific linear time trends. The bands around each line show 95% confidence intervals, based on standard errors clustered by state. Coefficients are normalized to show the effect of a temporary -1 percentage point shock to GSP growth in year 0. All regressions control for state and year fixed effects and are weighted by population.

Source: BLS, BEA, own calculations.

Appendix Figure D.3: Change in reasons for nonparticipation at $k = 2$ by demographic group



Note: Each bar shows the estimated effect of the output shock on the share of the population out of the labor force and reporting one of the five reasons categories (ill/disabled, school, home/family, retired, other) two years following the shock.

Source: BLS, BEA, and authors' calculations.

Appendix Table D.1: Kleibergen-Paap F Statistics

	KP F stat
-2	12.89012
-1	.
0	12.90944
1	14.0798
2	12.65996
3	14.57457
4	14.8723
5	14.96208
6	15.57704
7	18.30004
8	14.86393
9	20.74206
10	20.83183

Note: Each row reports the Kleibergen-Paap (2006) F statistic for each horizon in the baseline local projections specification of Equation 1. The relevant Stock-Yogo critical value for 15% maximal size is 8.96.

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