Excess Persistence in Employment of Disadvantaged Workers*

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We examine persistence in employment-to-population ratios among less-educated individuals in excess of that implied by persistence in aggregate labor market conditions, using state-level data for the United States. Dynamic panel regressions indicate only a moderate degree of excess persistence, which dissipates within three years. We find no significant asymmetry between the excess persistence of high versus low employment rates. The cumulative effect of excess persistence in the business cycle surrounding the 2001 recession was mildly positive, while the effect in the cycle surrounding the 2008–09 recession was decidedly negative. Simulations suggest that the lasting employment benefits of temporarily running a "high-pressure" economy are small.

JEL Codes: E24, J21, J24.

1. Introduction

The relationship between current employment experience and future employment outcomes, especially for disadvantaged workers, has long interested both researchers and policymakers. Notably, during the expansion of the 2010s policymakers asked whether temporarily running a "high-pressure economy," with robust aggregate demand

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and a tight labor market, might produce long-run benefits to workers with weak workforce attachment even after the economy as a whole returns to a more "normal" state (Stockhammer and Sturn 2012; Ball 2015; Reifschneider, Wascher, and Wilcox 2015; Yellen, 2016, 2019).[1]

We address this question by estimating excess persistence in employment among less-educated individuals using state-level data for the United States. We define excess persistence in employment as a lasting effect of past employment conditional on macroeconomic conditions, as in Okun (1973).

We find a moderate but ephemeral degree of excess persistence: For the group with the greatest excess persistence among those we examine—prime-age men with no more than a high school education—the effects of past employment rates on subsequent employment rates can be substantial early on but essentially dissipate within three years. Furthermore, we find little evidence for asymmetric effects of high or low past employment on present employment. Our estimates imply that the cumulative effect of excess persistence in the business cycle surrounding the 2001 recession was mildly positive, while the effect in the cycle surrounding the 2008–09 recession was decidedly negative. Our simulations suggest that, despite large contemporaneous benefits, the lasting employment benefits of temporarily running a "high-pressure" economy are small.

Microeconomic evidence seems to support the notion of excess persistence in employment. This evidence includes findings that macroeconomic conditions at the time a person completes his or her education and starts a career have lasting effects on relative individual earnings, that the state of the labor market earlier in one's tenure at an employer influences one's subsequent wage rate at that employer, and that a person's early employment experience may affect her later employment (see von Wachter 2020 for a recent discussion).

[1] A related literature addresses persistence in aggregate conditions themselves. This literature has found that in at least some countries, loose labor markets appear to have had adverse long-run effects (e.g., Blanchard and Summers 1986; Ball 2009).

However, such evidence on how conditions at an early point in one's labor market experience affect individual outcomes does not establish the existence of excess persistence in aggregate employment, for two reasons.

First, the effects on those who, say, initially enter the labor force during a tight labor market are measured relative to the effects on those who enter during a slack labor market. This form of comparative excess persistence at the individual level does not imply excess persistence at an aggregate level: More employment in my history may enhance my chances of being employed today at the expense of reducing the chances of a competing person (with less employment in his history) being employed today.[2]

Second, given the great heterogeneity across jobs and persons and the multiplicity of mechanisms through which employment experience may affect future employment probabilities, the dynamic effects of employment at the microeconomic level may depend on the source of the variation in employment. That is, the microeconomic evidence on the dynamic effects of more employment in general does not imply that greater employment achieved through tighter macroeconomic conditions, as opposed to other causes, will have lasting effects on overall employment rates.

In addition, the microeconomic literature has mostly concentrated on excess persistence in wage rates or earnings, which need not imply excess persistence in employment. Indeed, depending on the mechanism at work, persistence in wage rates may work against persistence in employment. For example, Schmieder and von Wachter (2010) find that lower unemployment rates during a worker's job spell, which are associated with higher wage premiums, significantly increase the probability of job loss.

That said, past labor market conditions may affect subsequent employment outcomes at the aggregate level even conditional on

[2]In the context of trade policy, Abraham and Kearney (2018, p. 8) write, "as Pierce and Schott (2016) acknowledge, their difference-in-differences identification strategy precludes an estimate of the effect of the policy change on overall U.S. employment. This is because the estimated effects are all about relative job losses and there is not an obvious way to translate their findings into an estimate of overall absolute job losses." Similarly, see Gautier et al. (2018) for an example of how microeconomic welfare evaluation of job search assistance may differ from aggregate evaluation.

subsequent macroeconomic conditions because they affect employment experience. In particular, experience provides human and market capital that enhance future employability, such as "soft" skills (Almlund et al. 2011) and job contacts that facilitate employment after job loss (Cingano and Rosolia 2012; Glitz 2017) or improve match quality (Dustmann et al. 2017).

Unfortunately, previous research directly addressing the question of excess persistence in aggregate employment in the United States is thin. We follow the general approach of Fleischman and Gallin (2001) and Fleischman, Gallin, and Smith (2018), who estimate a dynamic model to extract the persistence of the employment-to-population ratio (e/p) in excess of that implied by the persistence of the macroeconomic conditions themselves, as measured by overall labor market tightness. Their evidence is consistent with our results. They also do not find a large degree of persistence in cohort-level e/p in response to fluctuations in macroeconomic conditions. They use variation among synthetic birth cohorts over time to identify possible excess employment persistence in the national data, as opposed to variation among states over time that we exploit.

Hotchkiss and Moore (2018) use state-level variation to compare individual outcomes in recessions following expansions of varying intensities. They find that, for some demographic groups, a person is likely to experience better outcomes during a period of high unemployment if that period was preceded by a tighter labor market. Yagan (2019) and Hershbein and Stuart (2020) find a large amount of persistence in local e/p ratios following recessions. However, Hershbein and Stuart (2020) find that this relationship is likely driven by persistent declines in overall labor demand, rather than the result of excess persistence in employment that is our interest here. Using employer survey data from the 1990s, Holzer, Raphael, and Stoll (2006) find that the relative demand for disadvantaged workers rose and racial discrimination likely declined during that expansion. Unfortunately, their data cover only the period 1992 to 2001 and so cannot separate the contemporaneous implications of cyclical conditions from their longer-term effects.

To measure the excess persistence in aggregate employment, we estimate a dynamic panel model in the detrended employment-to-population ratio (e/p) of disadvantaged workers, while controlling for aggregate labor market conditions using the unemployment rate (UR) gap among all workers.[3] We use variation among states over time for identification in these regressions.

The validity of the policy implications from our reduced-form model requires two assumptions. The first is that the variations in e/p under consideration be driven only by variations in overall labor market conditions as represented by the UR gap. This means that the phenomena (including economic policies) that drive the UR gap have no direct effect on the cyclical component of the e/p of the disadvantaged group, or that any direct effect is highly correlated with the UR gap (see Section 3.2.1). The second is that the degree of excess persistence identified by the state panel regressions is applicable to the aggregate level. We argue in Section 3.2.2 that this assumption is appropriate in our application.

The paper proceeds as follows. Section 2 describes our data. Section 3 describes the dynamic panel model. Section 4 presents our baseline estimates. Section 5 presents robustness exercises, including various detrending methods for the e/p of disadvantaged workers and instrumenting for the UR gap. Section 6 considers various definitions of the disadvantaged group. Section 7 investigates whether the degree of excess persistence in employment differs between high and low employment rates. Section 8 simulates the implications of our estimates of excess persistence for employment over the business cycle, and their implications for temporarily running a tight labor market. Section 9 concludes.

2. Data and Definitions

2.1 Baseline Sample

We focus our analysis on individuals with no more than a high school education, for four reasons. First, this education group has seen its relative earnings (Acemoglu and Autor 2011) and employment (Juhn 1992; Council of Economic Advisers 2017) decline markedly since the 1970s, which has made it a frequent focus of concern. Second, the mechanisms mentioned above for possible excess persistence at the

[3] We do not address the possibility of persistence generated by *long-term* unemployment, as in Song and von Wachter (2014) and Kallenberg and von Wachter (2017).

aggregate level would seem to be more important for this population, whose lower employment rates in general mean that they may benefit less from households and neighborhoods that provide human and market capital independent of an individual's own employment history (Conley and Topa 2002). Third, the employment of these populations tends to be more procyclical, so any change in overall labor market conditions can be expected to have a larger effect on their employment (Devereux 2002; Hoynes, Miller, and Schaller 2012; Aaronson et al. 2019), making any degree of excess persistence more important for this group. Fourth, Blacks and Hispanics are more likely to be less educated (Stoops 2004) and if these groups face discrimination in the labor market, higher levels of employment among the less educated mean greater direct exposure of employers to this group, which may reduce discrimination (Boisjoly et al. 2006; Miller 2017).

While these factors apply equally to women and to men, in this paper we focus on men because of practical difficulties in detrending the employment rates of women (see Section 2.3).

We further concentrate on prime-age men, ages 25 to 54, in order to abstract from most education and ordinary retirement decisions. Also for practical reasons we mostly examine all races together. However, we explore excess persistence among alternative education, race, and age groups in Section 6, and find qualitatively similar results as for our baseline group.

2.2 Data

Our analysis uses U.S. annual data for the e/p and URs at the state level. We include only the 50 states, omitting Washington DC and territories. We calculate the e/p for particular demographic groups for 1978–2018 from individual data in the basic monthly Current Population Survey (CPS).[4] We use published data on state URs. We measure labor market tightness by the UR gap, the difference between the overall UR in the state and an estimate of the state's

[4]CPS state-level data (National Bureau of Economic Research 2019) are also available for 1976 and 1977, but due to confidentiality restrictions some states are not identified in those years.

trend UR. For our baseline specification we use estimates of state-level trend URs from Fallick and Tasci (2020) (henceforth FT).

2.3 Detrending e/p

There are secular trends in the e/p of all groups of workers that we study. We isolate the cyclical component of e/p of each group in each state using the method recommended by Hamilton (2018). This method derives the trend of a variable as the predicted value from a regression of that variable at date $t+h$ on the d most recent values as of date t. We set the horizon parameter, h, at five years, and d at four, but our results are not sensitive to other reasonable choices. Except where noted, all of the results reported below use these detrended e/p.[5] In order to minimize end-of-sample bias, when estimating the trend we augment the e/p series on both ends with univariate forecasts (Kaiser and Maravall 1999; Stock and Watson 1999a).[6]

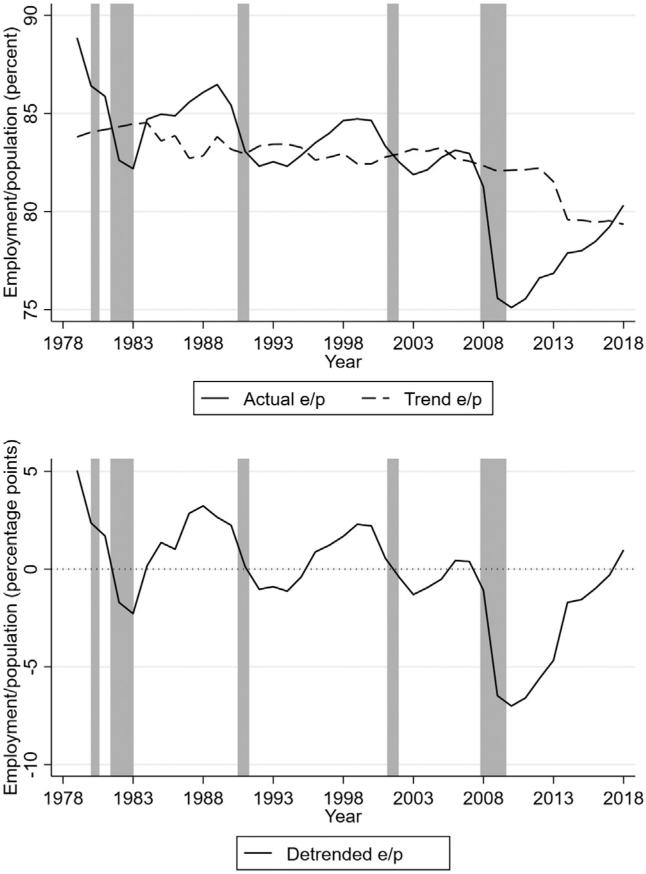
In addition to the advantages proposed by Hamilton (2018), this detrending method is backward looking. Alternative detrending methods that use subsequent data are not suitable for our purposes, as they may include the effects of excess persistence in the estimates of trend, thereby understating the amount of excess persistence in the data. We investigate alternative detrending methods (including no detrending) in Section 5.1.

The upper panel of Figure 1 shows the actual e/p and trend e/p for prime-age men with no more than a high school education

[5]Removing the trend in the e/p allows us to concentrate on persistence stemming from cyclical fluctuations. Notice that this detrended e/p will move fairly closely with (the negative of) the unemployment-population ratio in each state. This is because $e/p = L/p - u/p$, in which L denotes the size of the labor force and u denotes the number of unemployed workers, and removing the trend in the e/p primarily removes the secular movements in the labor force participation rate. However, movements in the participation rate caused by cyclical fluctuations ought to remain in the detrended e/p.

[6]We use second-order autoregressive models for this purpose, similar to Clark and Kozicki (2005) and Mise, Kim, and Newbold (2005), to extend the e/p series 10 years backward and forward from the beginning and end of our sample period, respectively.

Figure 1. Actual e/p and Trend e/p of Disadvantaged Group, Aggregated



Note: State-level actual and trend e/p for prime-age men with no more than a high school education aggregated to the national level. Trend e/p is calculated separately for each state using the method in Hamilton (2018). The dotted horizontal line in the lower panel denotes zero.

(the disadvantaged group in our baseline results), aggregated from the state to the national level for ease of display.[7] The lower panel shows the detrended e/p. Unfortunately, we were unable to estimate

[7]We aggregate by weighting within year by the number of observations in our CPS data for the baseline sample for each state in that year.

Table 1. Summary Statistics for e/p of Baseline Sample, State-Level Data

	Mean	Std. Dev.	Min.	Max.
Actual e/p$_{s,t}$ (%)	82.4	5.1	62.6	94.4
Detrended e/p$_{s,t}$ (pp)	–0.3	3.4	–14.0	9.7

Note: Summary statistics for prime-age men with no more than a high school education for the years 1978 to 2018. Mean and standard deviation are weighted by the population of the state. "Actual e/p$_{st}$" is the e/p of prime-age men with no more than a high school education in state s at time t. "Detrended e/p$_{st}$" is "Actual e/p$_{st}$" less the estimated trend for each state and is measured in percentage points (pp). Trend e/p is calculated using the method in Hamilton (2018). The detrended e/p will be the dependent variable in our baseline specification (Section 3).

reasonable trends for the like group of women, so we confine our attention to men.[8]

Table 1 provides summary statistics for the state-level (actual and detrended) e/p used in the regression analysis for our baseline group. Not surprisingly, there is more variation in the state-level data than is evident in the aggregate data in Figure 1.[9]

2.4 Disjoint Samples

Measuring employment status in the CPS is subject to measurement error from at least two sources. The first is sampling error, which makes any particular sample imperfectly representative of the population. The second is misreporting, due to misunderstanding, proxy responses, etc. (Poterba and Summers 1986; Elsby, Hobijn, and Şahin 2015). Sampling error, in particular, is positively correlated

[8]We suspect that the substantial changes in the trajectory of women's labor force participation in the 1970s and 1990s (e.g., Aaronson et al. 2014), and thus e/p, pose difficulties for univariate methods of estimating trends without longer time series than we have available for prime-age women with no more than a high school education.

[9]This trend differs from a suitably lagged moving average because the coefficients (effectively weights) on the various lags for each state are estimated, not imposed. The estimated coefficients are neither uniform across lags nor uniform across states, nor do they sum to 1 within any state (indeed, in every state the coefficients on the lags sum to less than 1). All in all, the estimated trends are smoother than moving averages.

over time due to the repeated sampling of individuals in the monthly CPS (Tiller 1992), which would bias up our estimates of excess employment persistence.

To avoid this bias, we use "disjoint" samples from one year to the next. That is, we calculate the e/p in state s in a given year for use on the left-hand side (LHS) of our regression equation (Equation (1) in Section 3 below) from a sample of individuals who are distinct from those used to calculate the e/p in previous years for use on the right-hand side (RHS) of this equation.[10] Since the disjoint samples still provide an unbiased estimate of the population e/p in each year, we obtain consistent estimates of excess employment persistence.

Such disjoint samples could be constructed in a number of ways. For simplicity and to balance the sample sizes used for the LHS and RHS measures, we choose to calculate the LHS e/p from a sample that includes only observations in the CPS that are in rotation groups 1 to 4, and the RHS e/p from a sample that includes only observations in rotation groups 5 to 8. These samples are disjoint because an individual in rotation groups 5 to 8 in year $t-1$ (or $t-2$) cannot be in rotation groups 1 to 4 in year t. There are other schemes that would provide slightly larger samples, but they would involve more complicated interactions between rotation group and calendar year. Summary statistics similar to those in Figure 1 and Table 1 for the full sample, but for our disjoint samples, are provided in Appendix A.

3. Dynamic Panel Methodology

3.1 Estimating Equation

Equation (1) is our baseline estimating equation, in which e/p is the detrended employment-to-population ratio, DA denotes the

[10] Indeed, as expected, estimation with the full sample suggests a larger amount of excess persistence than with the disjoint samples, although our conclusions in Section 8, in which we use simulations to assess the magnitude of our estimates, are not materially affected. We recognize that the smaller estimates from the disjoint samples could be due to attenuation bias from the smaller sizes of the disjoint samples. However, experimentation with random subsamples of the full sample that mimic the size of our disjoint samples indicates that attenuation bias is not a serious concern in this case.

disadvantaged group, s denotes state, t denotes year, the α are state fixed effects, the γ are year fixed effects, $Ugap$ is the UR gap, β and δ are coefficients, and ϵ is an error term:

$$(e/p)^{DA}_{s,t} = \alpha_s + \gamma_t + \beta_1(e/p)^{DA}_{s,t-1} + \beta_2(e/p)^{DA}_{s,t-2} + \delta_0 Ugap_{s,t} + \delta_1 Ugap_{s,t-1} + \delta_2 Ugap_{s,t-2} + \epsilon_{s,t}. \quad (1)$$

As described in Section 2.4, the e/p's on the left-hand and right-hand sides of Equation (1) are derived from disjoint samples.

This specification for $(e/p)^{DA}_{s,t}$ accounts for factors that differ across states but are constant over time (α_s) and for aggregate factors that change over time but are constant across states (γ_t). Thus, the estimation uses variation that is left over after removing within-state and within-year variation.

The coefficients β on the lagged detrended e/p terms capture persistence in the e/p *in excess of* that implied by the persistence in the overall UR gap.[11] Our approach for obtaining estimates of β_1 and β_2 in Equation (1) is equivalent to the following two-step procedure. First, regress $(e/p)^{DA}_{s,t}$ on state and year fixed effects and the UR gap and its two lags and obtain residuals, $\xi_{s,t}$. In this first step, we use two lags because the annual UR gap obtained with the FT approach is well approximated by an AR(2).[12] Second, obtain the two β coefficients from regressing $\xi_{s,t}$ on two own lags, $\xi_{s,t-1}$ and $\xi_{s,t-2}$. This second step captures the excess persistence in e/p after controlling for the effects of aggregate labor market conditions, as measured by the overall UR gap. In Appendix B.1 we show that these β parameters are a function of both individual effects on a person's employment, such as human capital accumulation and depreciation, as well as cross-individual effects, such as employment networks and competition.[13]

Of course, we are concerned about the endogeneity of the overall UR gap with respect to the detrended e/p for the disadvantaged

[11]This estimation strategy depends, of course, on there being sufficient cyclical variation in the UR gap to identify the relationships. If there were complete hysteresis in UR, for example, the strategy would fail.

[12]We investigated various lag lengths and, based on both formal tests and to avoid overfitting, settled on this lag structure.

[13]Our dynamic panel approach bears some resemblance to Blanchard and Katz (1992), which we discuss in Appendix B.2.

group, if for no other reason than that the disadvantaged group make up a sizable proportion of the labor force. In Section 5.2 we use several instruments for the UR gap and show that they do not change our conclusions.

A regression of squared residuals on the inverse of the number of observations, as suggested by Solon, Haider, and Wooldridge (2015), indicates significant heteroskedasticity in our data. Therefore we weight the regressions by the number of observations in the disadvantaged group in each state in each year.

It is well known that estimating dynamic panels with fixed effects may lead to biased estimates if the panel is short. Arellano (2003) argues that if the number of periods is at least 10, then this bias is likely small. Nickell (1981) shows that with reasonably long panels, the bias is around order $-(1+\beta)/T$, in which T is the length of the panel. As our data effectively span 38 years, if there is excess persistence ($\beta > 0$), the downward bias in the coefficient is likely small. Note, however, that Hershbein and Stuart (2020) argue otherwise.

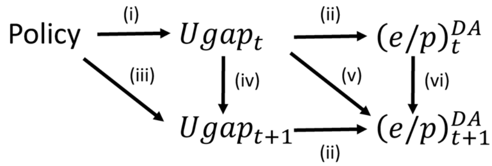
We are also not concerned that the size of our cross-section (N) induces bias. Monte Carlo simulations by Nerlove (1967), in which $N = 25$, suggest that the approximate formula for bias in Nickell (1981) is more or less exact when β is not too large. We have 50 cross-section observations in our baseline sample and our estimates of β are well below unity.

Although our data for the e/p go back to 1978, our estimates for trend UR from the FT model begin only in 1979. Between this constraint and the lag structure in Equation (1), the sample period for our baseline regressions is 1981 to 2018 (38 years), which, with 50 states, yields a total of 1,900 observations.

3.2 Identifying Aggregate Excess Persistence with Equation (1)

Below in Section 8 we will use the estimates from the reduced-form Equation (1) to simulate the implications of excess persistence for employment of the disadvantaged group over the business cycle. The validity of these inferences depends upon two assumptions. The first is that the variation in e/p under consideration is driven by variation in overall labor market conditions as represented by the UR gap, and that the degree of excess persistence in e/p is invariant to

Figure 2. Effects of Policy on the UR Gap and e/p



Note: We call channel (vi) excess persistence in e/p. We assume that variation in the UR gap is the only source of variation in detrended e/p that is relevant to generating excess persistence. See Section 3.2.1 for a discussion.

the source of that variation in the UR gap. The second is that the degree of excess persistence identified by the state panel regressions is applicable to the aggregate level. We discuss each of these in turn.

3.2.1 The UR Gap as a Sufficient Statistic for Labor Market Conditions

Figure 2 describes the causal relationship between economic policy actions, overall labor market conditions, and the (detrended) e/p of the disadvantaged group. A given policy action affects contemporaneous overall labor market conditions as represented by the UR gap (channel i), which, in turn, affects the e/p ratio of the disadvantaged group (channel ii). The e/p in year t further affects the e/p in year $t + 1$ (channel vi) conditional on the contemporaneous and lagged influence of the UR gap (channels ii and v). It is this channel (vi) that we call excess persistence in e/p and estimate with the coefficients β in Equation (1). (For ease of exposition, the diagram includes only one lag of UR gap and of e/p, although our empirical model includes two lags. We also omit the s subscript.)

The appropriateness of β for the simulations in Section 8 depends importantly on the assumption that variation in overall labor market conditions represented by the UR gap is the only source of variation in detrended e/p that is relevant to generating excess persistence. This means that the phenomena (including economic policies) that drive the UR gap have no direct effect on the cyclical component of the e/p (i.e., the detrended e/p) of the disadvantaged group, or, more precisely, that any direct effect is highly correlated with the

UR gap. This assumption would seem to be approximately appropriate for monetary policy actions as contemplated by Yellen (2016, 2019). However, our data doubtless include variations in the UR gap driven by other phenomena. We depend upon these other phenomena being sufficiently acyclical that their persistent effects on e/p are captured in the estimated trend.

An alternative would be to identify particular types of policy shocks and estimate excess persistence in the response of e/p to those shocks. If the degree of excess persistence varies by the type of policy, contrary to our assumption that the policies we contemplate operate on e/p only through overall labor market tightness, that would have the value of allowing differentiated policy simulations. Of course, the validity of such estimates depends upon proper identification of the policy shocks, which is often controversial (Nakamura and Steinsson 2018, p. 61). In addition, the number of incidents of any particular policy intervention in the sample period are not large, which may pose a challenge for estimation.

Our approach does not rely on identification of particular shocks, but if the degree of excess persistence does, in fact, vary by the type of policy, then our estimates are an average across different true coefficients. In that case, they may not be valid for any particular policy intervention, but instead provide a general sense of magnitudes.

3.2.2 Aggregate Inference from State-Level Variation

Although we are ultimately interested in excess employment persistence at the national level, we use state-level variation in our estimation to improve identification. We then assume that the estimates from our state-level panel regressions can be used to simulate excess persistence at the national level in Section 8 below.

However, Beraja, Hurst, and Ospina (2019), among others, note several impediments to extrapolating state-level coefficients to the national level.[14] There are at least three ways to address these impediments. First, instead of applying the coefficients from the state-level equations directly, one could use them to discipline structural models. Second, one could obtain aggregate estimates

[14] Other examples include Nakamura and Steinsson (2014), Charles, Hurst, and Schwartz (2019), and Adao, Arkolakis, and Esposito (2020).

by embedding the state-level equation within an explicitly spatial model, as in Adao, Arkolakis, and Esposito (2020). Third, one could concentrate on situations in which the impediments are, as a practical matter, minor. We adopt this third solution. For this solution to be valid, our application needs to satisfy three conditions.

The first condition is that the sources of change, and mechanisms through which those changes operate, be the same at the state and national level. As noted above, our model assumes that changes in overall labor market conditions as represented by the UR gap are the only contemporaneous drivers of changes in detrended e/p, so the proximate source of change is the same at the state and national levels. Furthermore, the mechanisms posited to produce excess persistence—accumulation of human capital and market capital—operate at the individual level, and so are the same in national as in state-level data.

The second condition is that there be sufficient variation across states over time in the right-hand side variables to identify the coefficients.[15] In our case, it is well recognized that both the size and timing of business cycle changes in unemployment and employment rates vary substantially across states (e.g., Owyang, Piger, and Wall 2005). The quantity of state-specific variation in these variables need not be large relative to the common variation to obtain reliable estimates. But the assumption that the phenomena that drive the movements in the UR gap have no direct effect on detrended e/p is important here. As emphasized by Nakamura and Steinsson (2014) and Chodorow-Reich (2019), if nationally uniform policies that move the unemployment rate also directly affected the detrended e/p, these would be absorbed by the year indicators in Equation (1) and our β coefficients would not capture that effect.

The third condition is that the change in one state not affect the outcome in another state in ways that are not accounted for in the econometric model. In our application, the main concern in this regard is interstate migration in response to differences across states in overall labor market conditions. In work not shown, we found that over our sample period the interstate migration of our

[15] Static cross-sectional variation will be captured by the state fixed effects, while movements over time that are common to all states will be captured by the time fixed effects.

Table 2. Baseline Estimates

	Coefficient (Std. Err.)
$(e/p)_{s,t-1}$	0.25*** (0.02)
$(e/p)_{s,t-2}$	0.14*** (0.03)
$Ugap_{s,t}$	–1.24*** (0.09)
$Ugap_{s,t-1}$	0.36*** (0.12)
$Ugap_{s,t-2}$	0.42*** (0.11)
Observations	1,900
Within R-squared	0.29

Note: The degree of excess persistence among prime-age men with no more than a high school education is moderate. These are the estimated coefficients from Equation (1). The dependent variable is the detrended e/p of disadvantaged workers, $(e/p)_{s,t}$. $Ugap_{s,t}$ is the UR gap in state s at time t. Weighted by number of observations of the disadvantaged group. Driscoll-Kraay standard errors in parentheses. ***p $<$ 0.01, **p $<$ 0.05, *p $<$ 0.1. See Section 3 for the specification and Section 4 for a discussion of the results.

baseline demographic group did not respond in an economically or statistically significant degree to such differences—findings that are consistent with those of Bound and Holzer (2000) and Notowidigdo (2020).[16]

If these conditions are met, then the coefficient β estimated from state-level variation in UR gaps is a valid estimate of the degree of excess persistence at the national level.

4. Baseline Estimates

We find a moderate degree of excess persistence in employment among disadvantaged men. The estimates are shown in Table 2.[17] The coefficients on the lagged detrended e/p are significantly positive, indicating some excess persistence. However, these coefficients,

[16]Details are available from the authors.

[17]Throughout the paper, we show Driscoll-Kraay standard errors, with a lag length of 3, to allow for both spatial and temporal dependence in our state-panel regressions (Driscoll and Kraay 1998). As an alternative, we have also estimated standard errors clustered on year and state. There was no consistent pattern across the coefficients of which method yielded larger estimates of the standard errors, and our conclusions are not sensitive to this choice.

as well as the results in Section 8, indicate that within three years the effect of the lagged e/p has virtually no effect on the current e/p.

Note that the coefficients on $Ugap_{s,t}$ and its lags are of opposite signs in Table 2. This is similar to the results in Fleischman and Gallin (2001) and Fleischman, Gallin, and Smith (2018), in which the coefficients on the GDP gap and the lagged GDP gap have opposite signs. One interpretation of this is that changes in the UR gap, in addition to the level, have a short-run effect on the detrended e/p of the disadvantaged group, as is common in models of wage growth (Blanchard and Galí 2010). As we will see in Section 8.2, this property leads to the e/p "overshooting" its trend in some simulations.

5. Robustness Exercises

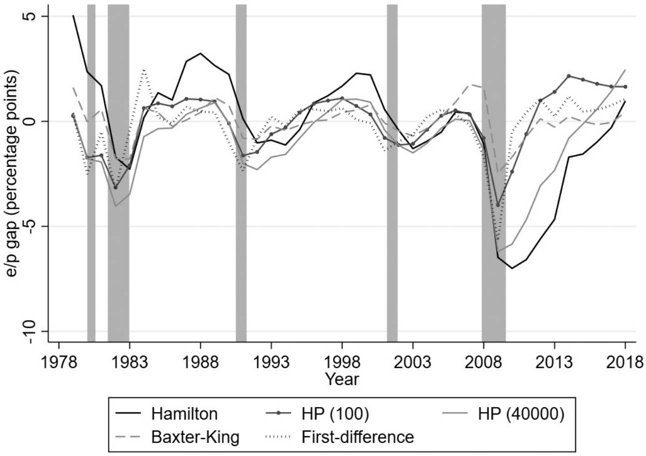
5.1 Detrending Methods for e/p

In this section we show that our results are robust to various methods of addressing trends in the e/p of disadvantaged workers.

As described above, our baseline estimates use the detrended e/p of the disadvantaged group in Equation (1) to concentrate on persistence stemming from cyclical fluctuations instead of lower-frequency (structural) phenomena. The choice of detrending method may therefore be important for identifying these cyclical movements, and in turn for the estimates of excess persistence. We try several methods for detrending state e/p, including one-sided filters, two-sided filters, and simple parametric time trends, as well as no attempt to account for trends. We find that these alternatives all imply similar or smaller estimates of excess persistence than our baseline approach.

Beginning with the filtering methods, in addition to our baseline approach that uses Hamilton (2018), we use the one-sided Hodrick-Prescott (HP) filter (Stock and Watson 1999b) with two smoothing parameters (100 and 40,000); first differencing; and a Baxter-King band-pass filter with a period of two to eight years and three-year smoothing. These filters amplify various frequencies of a series. The cyclical component from the Hamilton filter with a five-year horizon parameter recovers the spectral density function of white noise, as shown in Appendix C.1. The remaining filters remove lower-frequency components of the time series and pass through higher-frequency components, to a greater or lesser extent.

Figure 3. Estimates of State Detrended e/p, Aggregated



Note: State-level detrended e/p estimated by five different approaches, aggregated to the national level. See Section 5.1 for details.

The cyclical component of each filter gives a qualitatively similar account of the national detrended e/p since 1978, as shown in Figure 3. In particular, the deviations of e/p relative to trend were largest during the 2008–09 recession, less severe during the 1980s recession, and smallest during the 1991 and 2001 recessions. All the filters suggest that e/p was above trend in 2018. The detrended e/p from the Hamilton filter, reproduced from the lower panel of Figure 1, is the most procyclical out of our chosen filters. For example, during the 2008–09 recession, the Hamilton filter suggests that e/p fell over 6 percentage points relative to trend. (The cyclical components of the disjoint samples are similar to the full sample and shown in Appendix C.2.)

Our estimates of Equation (1) using the various detrending methods are shown in Table 3. Column 1 repeats our baseline specification with the detrended e/p using the approach in Hamilton (2018). Columns 2 through 5 present the results using the Baxter-King filter, the first-difference approach, and HP filter with 100 and 40,000 smoothing parameters, respectively. Column 6 uses the Hamilton method to detrend both the e/p ratio and the UR, to examine

Table 3. Excess Persistence Using Different Estimates of e/p Trends

	Baseline (Ham.) (1)	BK (2)	First Diff. (3)	HP (100) (4)	HP (40,000) (5)	Ham. for e/p and UR (6)
$(e/p)_{s,t-1}$	0.25***	–0.08***	0.27***	0.07***	0.21***	0.22***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
$(e/p)_{s,t-2}$	0.14***	–0.04**	0.18***	0.04**	0.15***	0.12***
	(0.03)	(0.02)	(0.01)	(0.02)	(0.02)	(0.03)
$Ugap_{s,t}$	–1.24***	–0.53***	–0.04	–0.94***	–1.22***	–1.10***
	(0.09)	(0.04)	(0.1)	(0.05)	(0.09)	(0.17)
$Ugap_{s,t-1}$	0.36***	0.29***	–1.25***	0.56***	0.56***	–0.10
	(0.12)	(0.07)	(0.15)	(0.07)	(0.10)	(0.19)
$Ugap_{s,t-2}$	0.42***	0.04	0.76***	0.41***	0.39***	0.45***
	(0.11)	(0.06)	(0.09)	(0.07)	(0.10)	(0.12)
Obs.	1,900	1,900	1,900	1,900	1,900	1,900
Within R2	0.29	0.06	0.32	0.19	0.25	0.30

Note: Using different estimates of state trend e/p implies similar or smaller estimates of excess persistence to our baseline approach (column 1). These are the estimated coefficients from Equation (1), in which we use different methods to detrend e/p. The disadvantaged group is prime-age men with no more than a high school education. The dependent variable is the detrended e/p of disadvantaged workers. $Ugap_{s,t}$ is the UR gap in state s at time t. In columns 1 through 5 we estimate the UR gap using the model developed in Fallick and Tasci (2020) (see Section 2). In column 6 we use the method described in Hamilton (2018) to detrend the UR. Weighted by number of observations of the disadvantaged group. Driscoll-Kraay standard errors in parentheses. ***p < 0.01, **p < 0.05, *p < 0.1. See Section 5.1 for details.

whether using different methods for estimating trends for these two quantities in the baseline specification is driving our results.

Using different estimates of state trend e/p implies similar or smaller estimates of excess persistence to our baseline approach. The first-difference and HP (40,000) filters imply a similar amount of excess persistence to our baseline approach. Other filters imply smaller estimates of excess persistence. Using the Hamilton (2018) method for detrending both e/p and UR—instead of the Hamilton method for e/p and the FT method for UR—yields similar results to our baseline approach, as shown in column 6. The detrended e/p from the Baxter-King and HP (100) filters are less procyclical than the other methods, consistent with the aggregated results in Figure 3.

Despite the robustness to various filtering methods, one may be concerned that if excess persistence is sufficiently long-lived, then any of these methods may attribute some of the excess persistence to the trend. This may be of particular concern if some secular movements have their origins in cyclical phenomena, as may be the case, for example, for the number of persons receiving disability payments (Aaronson et al. 2014). We therefore also estimate Equation (1) with no detrending at all, which should provide an upper bound on the degree of excess persistence, and with simple parametric time trends, which should be less susceptible to mistaking persistent cyclical movements for structural trends.

We present these results in Table 4. Column 1 repeats our baseline specification (with the detrended e/p). Columns 2 through 4 instead use actual e/p on both sides of Equation (1). Column 2 makes no attempt to account for trends in e/p. Column 3 includes linear time trends in e/p and column 4 includes quadratic time trends.

As expected, the coefficients on the lagged e/p in column 2 are larger than in the baseline. However, the differences are small. The inclusion of the parametric time trends results in smaller estimates of excess persistence than in the baseline.

5.2 Instrumenting for the UR Gap

The overall UR gap may be endogenous with respect to the detrended e/p for the disadvantaged group, if for no other reason

Table 4. No Time Trend and State-Specific Time Trends

	Baseline (1)	No Trends (2)	Linear Trends (3)	Quadratic Trends (4)
$(e/p)_{s,t-1}$	0.25***	0.27***	0.14***	0.10***
	(0.02)	(0.02)	(0.03)	(0.03)
$(e/p)_{s,t-2}$	0.14***	0.19***	0.08***	0.05*
	(0.03)	(0.02)	(0.03)	(0.03)
$Ugap_{s,t}$	–1.24***	–1.29***	–1.36***	–1.42***
	(0.09)	(0.11)	(0.09)	(0.09)
$Ugap_{s,t-1}$	0.36***	0.59***	0.43***	0.42***
	(0.12)	(0.11)	(0.08)	(0.08)
$Ugap_{s,t-2}$	0.42***	0.20**	0.021	–0.12
	(0.11)	(0.09)	(0.08)	(0.09)
Observations	1,900	1,900	1,900	1,900
R-squared	0.29	0.32	0.28	0.27

Note: Without detrending, estimates of excess persistence are slightly larger than in our baseline and using state-specific time trends reduces the estimates of excess persistence. These are the estimated coefficients from Equation (1), in which we include no detrending, as well as linear and quadratic state-specific time trends. The dependent variable is the actual employment-to-population ratio of disadvantaged workers. The disadvantaged group is prime-age men with no more than a high school education. $Ugap_{s,t}$ is the UR gap in state s at time t, in which we estimate the trend UR gap using the model developed in Fallick and Tasci (2020) (see Section 2). Column 1 produces the baseline OLS regression from Table 2. Column 2 includes no detrending of actual e/p. Columns 3 and 4 include linear and quadratic trends, respectively. Weighted by number of observations of the disadvantaged group. Driscoll-Kraay standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. See Section 5.1 for details.

than that the disadvantaged group in a particular state make up a sizable proportion of the labor force.

To address this potential endogeneity, we use three approaches to instrument for state UR gaps as estimated by the FT model. First, we instrument with state GDP (U.S. Bureau of Economic Analysis 2019) gaps because state GDP gaps are not mechanically related to the detrended e/p for the disadvantaged group. Second, we use the “leave-out” mean of the UR gap in the state’s region because the UR gaps of other states in a state’s region reflect similar demand conditions but should be approximately exogenous to the e/p in the state in question. Third, we use the detrended insured unemployment rate (IUR), defined as the number of individuals receiving UI benefits over all covered employment. The IUR is correlated with a state's UR but should be exogenous to a state's e/p because the IUR reflects the level of labor demand, benefits being designed to be paid only to individuals who lose a job through no fault of their own (e.g., laid off or position abolished), and not to individuals who quit or are fired for cause (U.S. Department of Labor 2018).

To obtain state GDP gaps we detrend state GDP using the procedure suggested by Hamilton (2018), with a five-year horizon parameter. To give this filter a running start ahead of our sample period, we estimate the filter from 1970 onward. However, data on state GDP are available beginning only with 1977. In order to minimize end-of-sample bias, we augment the GDP series on both ends, as we did for e/p in Section 2.3. Column 1 of Table 5 repeats our baseline ordinary least squared (OLS) regression. Column 2 shows the estimates using this instrument.

To obtain the "leave-out" mean of the UR gap in a state's region (the "regional Ugap"), we use the eight clusters of the 48 contiguous states identified by Crone (2005) as having similar business cycles. To adjust the baseline for comparison to this instrument, column 3 of Table 5 presents the estimated OLS coefficients from Equation (1) when using only the 48 contiguous states. Column 4 presents the results when instrumenting the UR gap with the regional UR gap, in which all UR gaps are estimated with the FT approach (as in the baseline).

To detrend the IURs we use an HP filter with a smoothing parameter of 1,600.[18] State-level insured unemployment data only start in 1986, so for comparison, in column 5 we rerun the baseline OLS regression for that shorter date range. Column 6 presents the results using the detrended IUR as an instrument.

[18]As with GDP, we augment the insured unemployment data on both ends using second-order regressive models to reduce endpoint bias.

Table 5. Instrumenting for UR Gaps with GDP Gaps, Regional UR Gaps, and IUR

	OLS Baseline (1)	**IV GDP (2)**	**OLS Excluding AK and HI (3)**	**IV Regional Ugap (4)**	**OLS 1986–2018 (5)**	**IV IUR 1986–2018 (6)**
$(e/p)_{s,t-1}$	0.25***	0.17***	0.25***	0.14***	0.26***	0.16***
	(0.02)	(0.05)	(0.02)	(0.03)	(0.02)	(0.03)
$(e/p)_{s,t-2}$	0.14***	0.14***	0.14***	0.08**	0.14***	0.06
	(0.03)	(0.02)	(0.03)	(0.04)	(0.03)	(0.04)
$Ugap_{s,t}$	–1.24***	–1.85***	–1.22***	–1.66***	–1.33***	–1.83***
	(0.10)	(0.8)	(0.10)	(0.32)	(0.10)	(0.29)
$Ugap_{s,t-1}$	0.36***	0.22	0.34***	–0.10	0.26*	0.10
	(0.12)	(1.4)	(0.11)	(0.52)	(0.14)	(0.50)
$Ugap_{s,t-2}$	0.42***	0.69	0.40***	0.53*	0.53***	0.22
	(0.11)	(0.75)	(0.10)	(0.32)	(0.13)	(0.35)
FS F-stat		942		164		60
Obs.	1,900	1,900	1,824	1,824	1,650	1,650
Within R2	0.29	0.23	0.28	0.18	0.28	0.17

Note: For each instrument, the estimated excess employment persistence is no greater than in the OLS baseline. These are the estimated coefficients from Equation (1) with instruments for the overall UR in state s at time t and its lags. The dependent variable is the detrended e/p of disadvantaged workers, $(e/p)_{s,t}$. The disadvantaged group is prime-age men with no more than a high school education. $Ugap_{s,t}$ is the UR gap in state s at time t, in which the trend is estimated using the Fallick-Tasci approach (Section 2). Column 1 reproduces the baseline OLS regression from Table 2. In column 2 we instrument for the UR gap with state GDP gaps. In column 3 we restrict the sample for the OLS regression to the 48 contiguous states. In column 4 we instrument for the UR gap with the average UR of the other states in a state's region as defined by Crone (2005). In column 5 we restrict the sample for the OLS regression to years when the IUR is available. Column 6 instruments for the UR gap with the IUR gap. Weighted by number of observations of the disadvantaged group. "FS" stands for "First Stage." Driscoll-Kraay standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. See Section 5.2 for details.

In each case, the estimated excess employment persistence when we instrument for the UR gap is no greater than in the OLS baseline.[19]

6. Different Definitions of the Disadvantaged Population

The definition of the disadvantaged group is necessarily somewhat arbitrary. We have so far defined the disadvantaged group as prime-age men with no more than a high school education because, among men, this group has seen substantial deterioration in relative earnings and employment in recent decades, has generally lower employment rates than other education groups, has more procyclical employment rates, and its members are more likely to be Black or Hispanic. However, these characterizations are all the more apt for persons with less than a high school education, and to Blacks and Hispanics themselves. In addition, younger persons have had less opportunity for previous accumulation of human and market capital, and so may have more to gain from a bout of employment and more to lose by missing out on employment, while older persons may exhibit more excess persistence in employment because of age discrimination in hiring (Neumark, Burn, and Button 2019).

Table 6 explores these possibilities by varying the definition of the disadvantaged population. Column 1 repeats our baseline specification, which treats prime-age (25 to 54) men with no more than a high school diploma as the disadvantaged group. Column 2 narrows the baseline sample to prime-age men with less than a high school education. Column 3 narrows the baseline sample to Black men and Hispanic men ages 25 to 54 with no more than a high school education.[20] Column 4 narrows the baseline sample to men ages 18 to

[19]In addition to the estimated coefficients, Table 5 reports the first-stage F statistic to show that our instruments are all strongly correlated with the detrended e/p of the disadvantaged group. We report the statistic proposed by Cragg and Donald (1993). Our test statistics are above conventional critical values presented in Stock and Yogo (2005).

[20]We also tried samples of Black men and Hispanic men separately. Unfortunately, the samples in the CPS data were too small to allow reasonable estimation. For example, for Black men with no more than high school the smallest state averages just 6 observations, and limiting the sample to only states with even 75 observations eliminates half of the states.

Table 6. Different Definitions of Disadvantaged

	Baseline (25–54, $\leq$ HS) (1)	**Less Educ. (25–54, $<$ HS) (2)**	**Blacks & Hispanics (25–54, $\leq$ HS) (3)**	**Younger (18–34, $\leq$ HS) (4)**	**Older (45–64, $\leq$ HS) (5)**
$(e/p)_{s,t-1}$	0.25***	0.17***	0.026	0.21***	0.19***
	(0.02)	(0.02)	(0.04)	(0.03)	(0.02)
$(e/p)_{s,t-2}$	0.14***	0.091***	0.034	0.13***	0.096***
	(0.03)	(0.03)	(0.05)	(0.03)	(0.02)
$Ugap_{s,t}$	–1.24***	–1.15***	–0.91***	–1.76***	–0.56***
	(0.10)	(0.21)	(0.20)	(0.17)	(0.12)
$Ugap_{s,t-1}$	0.36***	0.37	0.27	0.54**	–0.064
	(0.12)	(0.28)	(0.46)	(0.25)	(0.18)
$Ugap_{s,t-2}$	0.42***	0.17	–0.21	0.42**	0.25**
	(0.11)	(0.23)	(0.32)	(0.18)	(0.12)
Obs.	1,900	1,900	1,900	1,900	1,900
Within R2	0.29	0.086	0.02	0.28	0.10

Note: Different definitions of disadvantaged do not suggest greater excess persistence in employment. These are the estimated coefficients from Equation (1) in which the dependent variable is the detrended e/p of various definitions of disadvantaged workers, $(e/p)_{s,t}$. $Ugap_{s,t}$ is the UR gap in state s at time t, in which we estimate the trend UR gap using the model developed in Fallick and Tasci (2020) (see Section 2). Weighted by number of observations of the disadvantaged group. Driscoll-Kraay standard errors in parentheses. ***p < 0.01, **p < 0.05, *p < 0.1. See Section 6 for details.

34 with no more than a high school education. Column 5 narrows the baseline sample to older men ages 45 to 64 with no more than a high school education.

In all cases, the coefficients indicate less excess persistence than in the baseline. We take these results with a grain of salt, because the smaller sizes of the samples in the CPS data may lead to noisier measures of the lagged e/p and therefore to attenuation of those coefficients.[21]

7. Asymmetries

High and low detrended e/p may have asymmetric effects on future employment outcomes. For example, skills may be slower to deteriorate through non-use than they are to accrue through use, while the formation of networks may display the opposite pattern. To allow for such asymmetry, we split the lagged detrended e/p term into two components: one for the e/p above its trend (positive detrended e/p) and one for the e/p below its trend (negative detrended e/p).

Column 1 of Table 7 repeats the baseline specification. Column 2 introduces asymmetric linear terms. The estimates do not indicate significant asymmetry. Although the point estimates for the second lag of e/p do show more persistence in the positive direction, F-tests (not shown) cannot reject that the coefficients on the positive and negative e/p are equal at conventional significance levels. In column 3 we add quadratic terms in each asymmetric detrended e/p to allow for the possibility that extremely high employment or extremely low employment has a larger marginal effect than smaller deviations from trend. Here, too, one cannot reject symmetry.

8. Simulations

In this section we provide simulations to help interpret the magnitude of our baseline estimates of employment persistence from

[21] Attenuation bias is a potential concern with the baseline definition as well, of course. However, the sample sizes for that group are large: The smallest state averages 775 observations. In contrast, for example, for the less-than-high-school group the smallest state averages 139.

Table 7. Asymmetry

	Baseline (1)	Linear Asymmetry (2)	Quadratic Asymmetry (3)
$(e/p)_{s,t-1}$	0.25***		
	(0.03)		
$(e/p)_{s,t-2}$	0.14***		
	(0.02)		
$(e/p\ positive)_{s,t-1}$		0.25***	0.21*
		(0.05)	(0.12)
$(e/p\ positive\ squared)_{s,t-1}$			0.01
			(0.02)
$(e/p\ negative)_{s,t-1}$		0.25***	0.21***
		(0.03)	(0.06)
$(e/p\ negative\ squared)_{s,t-1}$			–0.01
			(0.01)
$(e/p\ positive)_{s,t-2}$		0.19***	0.25**
		(0.04)	(0.10)
$(e/p\ positive\ squared)_{s,t-2}$			–0.01
			(0.01)
$(e/p\ negative)_{s,t-2}$		0.11**	0.14**
		(0.04)	(0.06)
$(e/p\ negative\ squared)_{s,t-2}$			0.00
			(0.01)
$Ugap_{s,t}$	–1.24***	–1.25***	–1.24***
	(0.09)	(0.09)	(0.09)
$Ugap_{s,t-1}$	0.36***	0.36**	0.37**
	(0.12)	(0.12)	(0.11)
$Ugap_{s,t-2}$	0.42***	0.41***	0.41***
	(0.11)	(0.11)	(0.11)
Obs.	1,900	1,900	1,900
Within R2	0.28	0.284	0.285

Note: Estimates do not indicate significant asymmetry. These are the estimated coefficients from versions of Equation (1) in which we split the lagged detrended e/p term into two components: above and below trend. The dependent variable is the detrended e/p ratio of disadvantaged workers, $(e/p)_{s,t}$. The disadvantaged group is prime-age men with no more than a high school education. $Ugap_{s,t}$ is the UR gap in state s at time t, in which we estimate the trend UR gap using the model developed in Fallick and Tasci (2020) (see Section 2). Weighted by number of observations of the disadvantaged group. Driscoll-Kraay standard errors in parentheses. ***p $<$ 0.01, **p $<$ 0.05, *p $<$ 0.1. See Section 7 for details.

Section 4 and their implications for policymakers. We focus on our baseline demographic group—prime-age men with no more than a high school education—because this group exhibited the largest amount of excess persistence in employment of the groups that we examined (see Section 6).

8.1 Historical Simulations

As noted above, policymakers have been interested in the possibility that the employment benefits of a high-pressure economy for disadvantaged groups may persist even after overall labor market conditions have normalized. This idea implies that there are mechanisms, such as the accumulation of human capital and network capital, that are distinct from those that generate the response of the e/p of the disadvantaged to overall labor demand, and so may persist after that overall demand has normalized. In motivating Equation (1), we argued similarly that the individual-level mechanisms that would generate excess employment persistence, and are reflected in coefficients β, are distinct from those that generate the direct relation between detrended e/p and the UR gap, which are reflected in coefficients δ (see Section 3 and Appendix B.1).[22] If this distinction is valid, then by setting the β coefficients to zero while leaving the δ coefficients at their estimated values, we can obtain a counterfactual e/p of the disadvantaged group that would obtain in the absence of the individual-level mechanisms underlying excess persistence. In this section we use such a counterfactual to quantify the cumulative effect of excess persistence since the mid-1990s.

First, we simulate e/p using the estimated coefficients from Equation (1) in Table 2. Second, we simulate e/p setting the coefficients on the lagged e/p terms to zero while leaving the coefficients on the UR gap and its lags as they are in Table 2. The cumulative difference between these two simulations is a measure of the contribution of excess persistence to the e/p of the disadvantaged group over this

[22]Put another way, if Equation (1) was the outcome of a microfounded model that included the relevant mechanisms, we are assuming that the structural parameters contributing to the coefficients on lagged e/p would be distinct from those contributing to the coefficients on the UR gap.

period, as explained above.[23] Comparing the two simulations, we find that the cumulative effect of excess persistence in the business cycle surrounding the 2001 recession was mildly positive, while the effect in the cycle surrounding the 2008–09 recession was decidedly negative.

The details for both simulations are as follows. We simulate the e/p of the disadvantaged group in each state for each year between 1987 and 2018. To do so, we set the UR gap to its observed value in each state and year 1985 to 2018. We set the detrended e/p of the disadvantaged group to zero in the two years (1985 and 1986) before the simulation commences.[24] In both simulations we set the δ coefficients and state and calendar-year effects to their estimated values. As just noted, in the first simulation we set the β coefficients to their estimated values, but in the second simulation we set the βs to zero. For ease of presentation, we then aggregate the two simulated e/p's from the state to the national level, and concentrate on the period beginning in 1996, a year in which the national UR was near the Congressional Budget Office's (CBO's) estimate of the natural rate of unemployment.

Before presenting our results, we note that the e/p simulated using our estimated coefficients follows a similar trajectory to the actual e/p over the 1996 to 2018 period, as shown in Figure 4.[25] This result suggests that our dynamic panel model (Equation (1)) accounts well for the cyclical variations in the actual e/p.

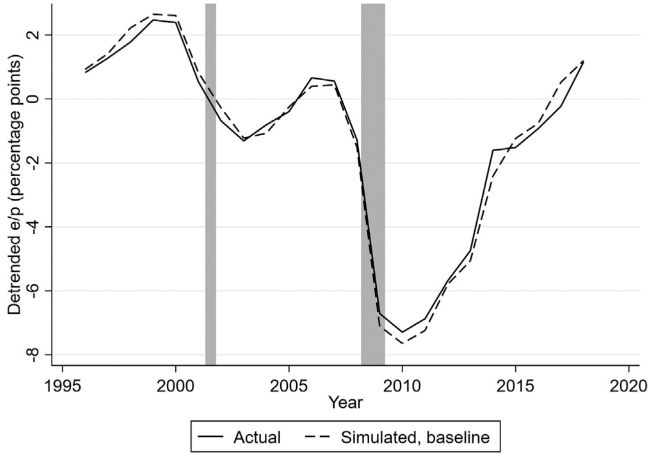
Figure 5 shows the estimated contribution of excess persistence to the e/p of the disadvantaged group. During the tight labor market

[23] An alternative would be to reestimate the equation imposing the restriction that the coefficients on the lagged e/p be zero, and use the coefficients on the *Ugap* terms from that regression in the counterfactual simulation. In that case, however, the coefficients on the *Ugap* terms would reflect excess persistence in the e/p to the extent that it is correlated with the persistence in *Ugap*. In this case the difference would not measure the contribution of excess persistence if some exists.

[24] The outcomes of interest are not sensitive to this choice of the initial e/p. Nor are they sensitive to the choice of starting year.

[25] To obtain the detrended actual e/p, we use the full sample of the CPS as opposed to the disjoint samples we used for estimating the amount of employment persistence. We take this approach because, while the disjoint samples minimize correlated measurement error for purposes of estimation (Section 2.4), the full sample provides the best estimate of the e/p for any given year.

Figure 4. Detrended e/p of Disadvantaged Group: Actual and Historical Simulation

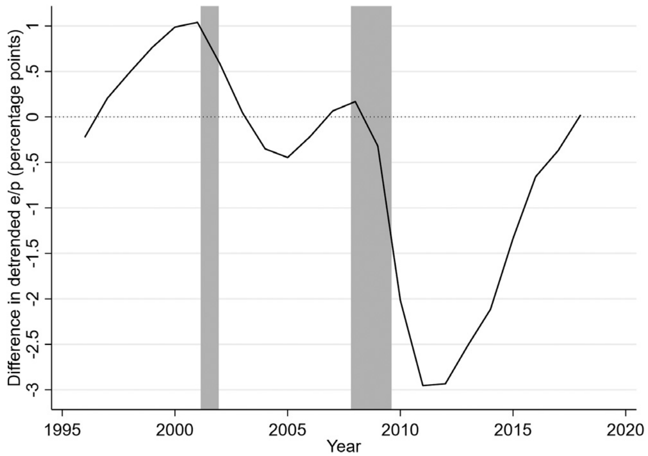


Note: The detrended e/p simulated using our estimated coefficients follows a similar trajectory to the detrended actual e/p over the 1996 to 2018 period. The figure shows the detrended actual e/p over the 1996 to 2018 period along with the one-step-ahead dynamic simulation of Equation (1) using our estimated baseline coefficients in Table 2. The simulation is done at the state level and then aggregated to the national level. For the UR gap we use the actual UR less an estimate using the model developed in Fallick and Tasci (2020) (see Section 2). See Section 8.1 for details.

toward the end of the 1990s expansion and before the 2001 recession, excess persistence served to buoy the e/p of the disadvantaged group by up to 1 percentage point at its height. Following the recession, however, excess persistence pulled in the opposite direction, weighing on the e/p of this group, with the contribution turning negative by 2004. Cumulatively, the former benefit outweighed the latter cost.

The situation is, unfortunately, quite different in the subsequent business cycle. The labor market was not as tight toward the end of the 2000s expansion as it was in the previous cycle, so the contribution of excess persistence barely moved into positive territory. The severity of the 2008–09 recession, however, meant that excess persistence weighed on the e/p of this group by almost 3 percentage points in 2011 and 2012, and only in 2018, when the national UR was

Figure 5. Contribution of Excess Persistence in Historical Simulation



Note: On net, excess persistence benefits disadvantaged workers during the business cycle around the 2001 recession but harms them during the cycle around the 2008–09 recession. This figure plots the difference between the simulated e/p from Equation (1) using all of the coefficients from Table 2 and the simulated e/p using the estimated coefficients for the *Ugap* terms but setting the coefficients on the lagged e/p terms to zero. This difference captures the contribution of excess persistence to the e/p of disadvantaged workers. The horizontal dotted line denotes zero.

0.7 percentage point below the CBO's natural rate of 4.6 percent, did excess persistence stop pushing down the e/p of disadvantaged workers.[26] Cumulatively, the costs of excess persistence during and after the 2008–09 recession far outweighed the benefits during the late stage of the previous expansion.

8.2 Policy Simulations

In this section we address policymakers' interest in the possible lasting employment benefits of a "high-pressure economy" (Ball 2015; Yellen 2016) for disadvantaged groups by providing a sense of the

[26]Replacing the baseline equation with an asymmetric specification from Section 7 does not qualitatively alter these conclusions.

likely magnitude of such benefits. These magnitudes are of particular interest if one is concerned that a high-pressure economy may increase the risk of subsequent recession, either because of high inflation and ensuing policy response (Lacker 2017; Bostic 2018) or other business cycle dynamics (Beaudry, Galizia, and Portier 2015, 2016; Feldstein 2018; Kiley 2018; Jackson and Tebaldi 2019). If this is the case, then the potential benefits of a high-pressure economy must be traded off against the possible costs.

We represent a high-pressure economy by the late stage of the 1990s expansion (we discuss this choice below). We convey a sense of the potential benefits from excess persistence by simulating the detrended e/p of the disadvantaged group from a UR that rises gradually from its low in 2000 to the natural rate in 2005 ("no-recession scenario"). We convey a sense of the potential costs from excess persistence by simulating the detrended e/p of the disadvantaged group from a UR that imitates the 2001 recession: rising to 6 percent in 2003 before falling back to the natural rate in 2005 ("recession scenario"). Not surprisingly given the modest degree of excess persistence implied by our estimated coefficients, we find that neither the lasting benefits nor the lasting costs are large, although the paths of employment are quite different in the two simulations.

In contrast to the historical simulations in Section 8.1, here we simulate directly at the national level in order to more easily specify historical paths for the UR.[27]

We chose the late stage of the 1990s expansion to represent a high-pressure labor market because in 2000 the national UR was as far below the CBO's estimate of its natural rate as occurred during the span of our data. Broad indexes of labor market conditions (KC LMCI 2021, for example) also suggest that the late stage of the 1990s expansion was the tightest labor market in that span.

To abstract from changes over time that are not due to the assumed paths for overall labor market conditions, we set the trend UR in every year of the simulations equal to the CBO's estimate of the long-run natural rate for 2005 (5.0 percent), and set all of the

[27] Because our empirical model assumes that the β and δ coefficients in Table 2 are constant across states, it would make little difference if we performed the simulations at the state level and aggregated to the national level.

year effects to the estimated year effect for 2005.[28] As in the historical simulations in Section 8.1, we begin the simulation in 1987 and set the detrended e/p of the disadvantaged group to zero in the two years before the simulations commence.

We show the hypothesized paths for the UR in the upper panel of Figure 6. In the no-recession scenario (gray line), we set the UR to the actual UR from the beginning of the simulation through the year 2000; from 2001 to 2005 we set the UR to rise linearly to the natural rate; and from 2005 on we hold the UR steady at the natural rate.

The recession scenario (black line) differs from the no-recession scenario only in the assumed path for the UR between 2000 and 2005 (the dashed lines in the upper panel of Figure 6 denote these two years). In this scenario, we set the UR to its actual value from 2001 (the year the recession commenced) through 2003 (the year in which the UR peaked during that cycle). We then set the UR to decline at a constant rate to trend in 2005. Thus this scenario includes something very much like the 2001 recession.

We show the simulated paths of detrended e/p in these two scenarios in the lower panel of Figure 6. For ease of exposition, we show the simulated detrended e/p as the deviations from the "steady-state" detrended e/p implied by our baseline coefficients.[29]

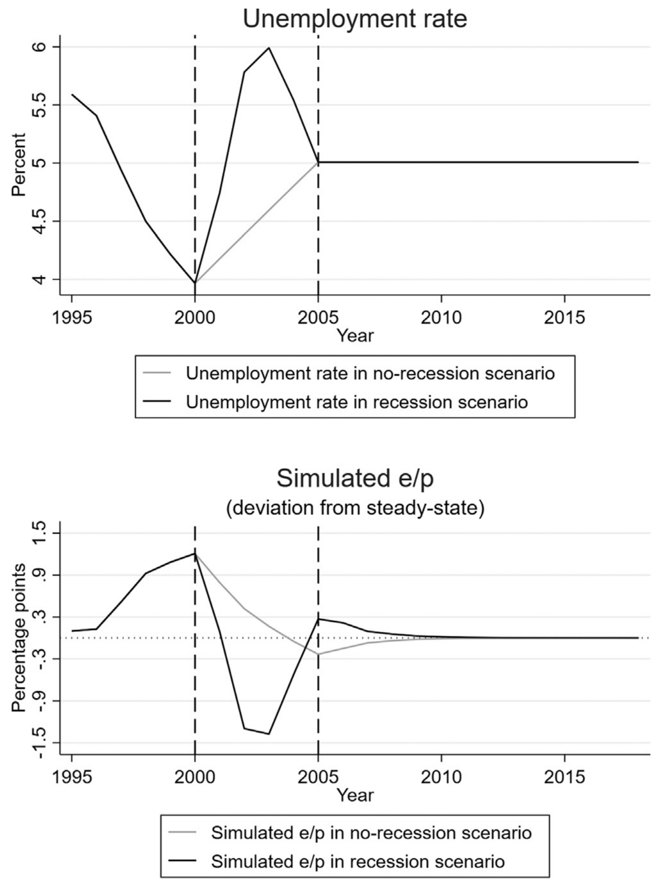
In the no-recession scenario, the lasting benefit from the tight labor market in 2000 is small. Naturally, the e/p rises above the steady state (positive in the graph) into the tight labor market of the late 1990s. The detrended e/p then falls toward the steady state as the UR reverts to trend. By 2005, when the UR returns to trend, the detrended e/p has essentially returned to its steady-state level, despite some small overshooting (see Section 4).

In the recession scenario, the lasting cost of the 2001 recession are also small. The detrended e/p falls during 2001 to 2003 as the UR rises, then rises as the UR falls. By 2005, when the UR has returned

[28]In 2005 the national UR was quite close to the CBO's estimate of the natural rate.

[29]We define a steady-state e/p as the solution for $(e/p)_t^{DA}$ in Equation (1) when $(e/p)_t^{DA} = (e/p)_{t-1}^{DA} = (e/p)_{t-2}^{DA}$, $Ugap_t = Ugap_{t-1} = Ugap_{t-2} = 0$, and $\gamma_t = \gamma_{2005}$. There are no s subscripts because these policy simulations are performed at the aggregate level.

Figure 6. No-Recession and Recession Scenarios in Policy Simulation



Note: The lasting employment benefits of temporarily running a "high-pressure" economy are small. The top panel shows the trajectory of the assumed UR for two scenarios: a "no-recession" scenario and a "recession" scenario. The lower panel shows the deviations of e/p from steady state in these two scenarios. After 2005, when the UR returns to trend, the lasting employment benefits and costs are small in the two scenarios. The time between the two vertical lines denotes the period over which the aggregate UR is assumed to be different between the two scenarios. The horizontal dotted line in the lower panel denotes zero.

to neutral, the detrended e/p has returned to its steady-state level except for a small amount of overshooting.[30]

In short, while the contemporaneous benefit for the e/p of disadvantaged workers of a high-pressure economy, and the contemporaneous cost should it be followed by a recession, are clear, neither has a significant lasting effect on the e/p of this group.[31]

These simulations used the coefficients estimated from our baseline sample. As noted in Section 6, this baseline group exhibits the largest amount of excess persistence among the groups we examined. At the other end of the spectrum, we found the least amount of excess persistence in the sample of Black or Hispanic men, ages 25 to 54, with no more than a high school education. Figure 7 repeats the simulation exercises using the coefficients estimated for this latter group, with the upper panel showing the historical simulations and the lower panel the policy simulations. Unsurprisingly, the contribution of excess persistence for this group in the historical exercise is minimal, and the policy simulations exhibit little difference between the two scenarios.

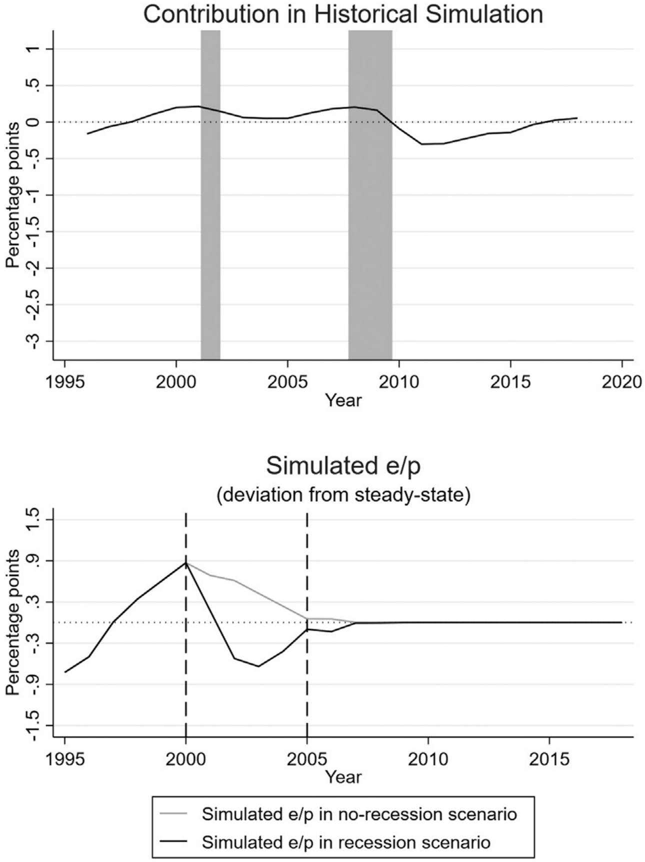
9. Conclusion

In this paper, we estimate a dynamic model on a panel of state-level data to quantify the persistence in the e/p of disadvantaged workers beyond that implied by the persistence of aggregate labor market conditions, which we call excess persistence in employment. We find that the e/p of less-educated prime-age males exhibits a moderate degree of excess persistence, which dissipates within three years. This finding is robust to a number of variations in sample and specification. Most notably, we find no indication of policy-relevant amounts of excess persistence for several definitions of disadvantaged populations that vary education levels, race, and age. In addition, we find no substantial asymmetry in the excess persistence of high

[30]Our historical simulation (Figure 5) suggests that the lasting cost of the 2008–09 recession would be larger. But even there the employment effects of excess persistence faded in just one year after UR returned to its natural rate in 2017.

[31]Replacing the baseline equation with an asymmetric specification from Section 7 does not qualitatively alter these conclusions.

Figure 7. Simulations for Group with Least Excess Persistence



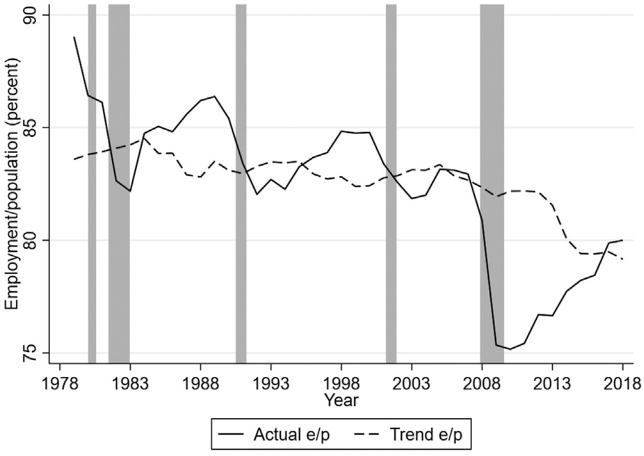
Note: For the demographic group with the smallest estimated amount of excess persistence (prime-age Black or Hispanic men with no more than a high school education), the contribution of that persistence in either historical or policy simulations is small. The time between the two vertical lines denotes the period over which the aggregate UR is assumed to be different between the two scenarios. The horizontal dotted line each panel denotes zero.

versus low employment rates. The cumulative effect of excess persistence in the business cycle surrounding the 2001 recession was mildly positive for our baseline group, while the effect in the cycle surrounding the 2008–09 recession was decidedly negative. Our simulations suggest that, despite large contemporaneous benefits, the lasting benefits to the employment rates of disadvantaged workers of temporarily running a "high-pressure" economy are small.

Appendix A. Summary Statistics for Disjoint Samples

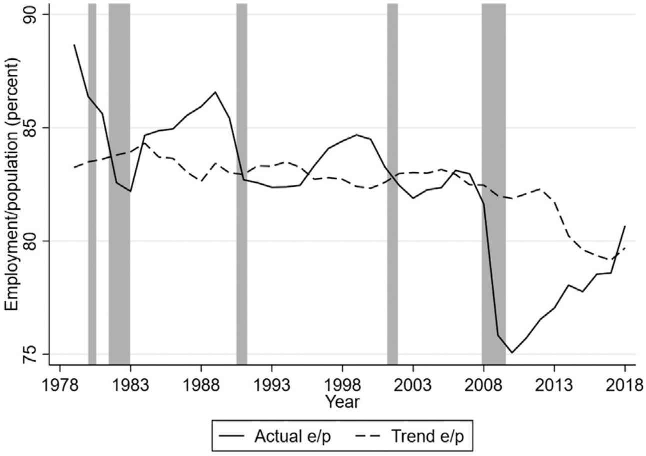
Figure 1 in the main text shows the full-sample estimates of the e/p and the trend e/p for our baseline sample of disadvantaged workers, in which both series have been aggregated from the state to the national level. These full-sample estimates provide the best estimates of the actual e/p and trend e/p in the economy. However, as described in Section 2.4, for the estimation we use disjoint samples. Figures A.1 and A.2 replicate Figure 1 for those disjoint samples. Tables A.1 and A.2 do the same for Table 1. On average the disjoint samples look much like the full sample, although there is more variability across states and years.

Figure A.1. Actual e/p and Trend e/p of Disadvantaged Group, Aggregated, LHS Sample



Note: State-level actual and trend e/p for prime-age men with no more than a high school education, aggregated to the national level. The trend e/p is calculated separately for each state using the method in Hamilton (2018). We use disjoint samples of the LHS and RHS e/p in the main analysis (Section 2.4). This presents the e/p ratios for the sample used in the LHS e/p. See Figure 1 for the e/p ratios using the full sample.

Figure A.2. Actual e/p and Trend e/p of Disadvantaged Group, Aggregated, RHS Sample



Note: State-level actual and trend e/p for prime-age men with no more than a high school education, aggregated to the national level. The trend e/p is calculated separately for each state using the method in Hamilton (2018). We use disjoint samples of the LHS and RHS e/p in the main analysis (Section 2.4). This presents the e/p ratios for the sample used in the RHS e/p. See Figure 1 for the e/p ratios using the full sample.

Table A.1. Summary Statistics for Baseline Group, State-Level Data (LHS sample)

	Mean	Std. Dev.	Min.	Max.
e/p$_{s,t}$, Actual (%)	82.4	5.2	61.4	95.6
e/p$_{s,t}$, Detrended (pp)	–0.3	3.6	–14.7	10.6

Note: Summary statistics for baseline samples for the years 1978 to 2018. "e/p$_{st}$, Actual" is the e/p of prime-age men with no more than a high school education in state s at time t. "e/p$_{st}$, Detrended" is "e/p$_{st}$, Actual" less the estimated trend for each state and is measured in percentage points (pp). The trend e/p is calculated using the method in Hamilton (2018). We use disjoint samples for the LHS and RHS e/p in the main analysis (Section 2.4). This table presents the summary statistics for the sample used in the LHS e/p. See Table 1 for the summary statistics using the full sample.

Table A.2. Summary Statistics for Baseline Group, State-Level Data (RHS Sample)

	Mean	Std. Dev.	Min.	Max.
e/p$_{s,t}$, Actual (%)	82.3	5.2	59.2	95.6
e/p$_{s,t}$, Detrended (pp)	–0.3	3.6	–16.5	10.3

Note: Summary statistics for baseline samples for the years 1978 to 2018. "e/p$_{st}$, Actual" is the e/p of prime-age men with no more than a high school education in state s at time t. "e/p$_{st}$, Detrended" is "e/p$_{st}$, Actual" less the estimated trend for each state and is measured in percentage points (pp). The trend e/p is calculated using the method in Hamilton (2018). We use disjoint samples for the LHS and RHS e/p in the main analysis (Section 2.4). This presents the summary statistics for the sample used in the RHS e/p. See Table 1 for the summary statistics using the full sample.

Appendix B. Our Specification and Blanchard and Katz (1992)

B.1 Motivating Our Baseline Equation

Our estimating Equation (1) can be thought of as the aggregated version of an individual-level equation. Ignoring some lags and the state subscripts for ease of exposition, the individual-level equation is

$$(e/p)_{i,t} = \alpha_i + \gamma_t + \phi(e/p)_{i,t-1} + \lambda \sum_{j \neq i} (e/p)_{j,t-1} + \delta U gap_t,$$

in which ϕ represents sources of persistence such as human capital accumulation and depreciation, and λ represents cross-individual effects of the sort we discussed in Section 1.

Summing across i,

$$\begin{aligned}
\sum_i (e/p)_{i,t} &= \sum_i \alpha_i + N\gamma_t + \phi \sum_i (e/p)_{i,t-1} \\
&\quad + \lambda \sum_i \sum_{j \neq i} (e/p)_{j,t-1} + N\delta U gap_t \\
&= \sum_i \alpha_i + N\gamma_t + \phi \sum_i (e/p)_{i,t-1} \\
&\quad + \lambda \sum_i \left[\sum_j (e/p)_{j,t-1} - (e/p)_{i,t-1} \right] + N\delta U gap_t.
\end{aligned}$$

Denote $(e/p)_t$ as the mean of $(e/p)_{i,t}$ across i to obtain

$$N(e/p)_t = \sum_i \alpha_i + N\gamma_t + N\phi(e/p)_{t-1} + \lambda \sum_i [N(e/p)_{t-1} - (e/p)_{i,t-1}] + N\delta Ugap_t,$$

and divide through by N to get

$$(e/p)_t = \frac{1}{N}\sum_i \alpha_i + \gamma_t + \phi(e/p)_{t-1} + \lambda[N(e/p)_{t-1} - (e/p)_{t-1}] + \delta Ugap_t$$

or

$$\begin{aligned}(e/p)_t &= \frac{1}{N}\sum_i \alpha_i + \gamma_t + [\phi + \lambda(N-1)](e/p)_{t-1} + \delta Ugap_t \\ &= \alpha + \gamma_t + \beta(e/p)_{t-1} + \delta Ugap_t,\end{aligned} \tag{2}$$

in which $\beta = \phi + \lambda(N-1)$. Equation (2) is our estimating equation, in which β is the object of primary interest.

B.2 Relation to Blanchard and Katz (1992)

Our analysis is similar to Blanchard and Katz (1992)—henceforth B&K—and Dao, Furceri, and Loungani (2017). Both those studies and ours use state-level labor market data in a VAR-type framework. In particular, B&K estimate a VAR in three state-level variables: the change in employment, the employment-to-labor-force rate (that is, one minus the UR), and the labor force participation rate (LFPR), and identify innovations in employment with shocks to labor demand.[32]

However, we and B&K address different questions. B&K are interested in how a state's labor market adjusts to unexpected

[32] B&K use defense spending and predicted growth rates of employment using state industry shares and national growth rates as two observable and plausibly exogenous demand shocks.

changes in labor demand that cause its overall labor market conditions (employment in particular) to differ from that of other states, while our study focuses on the persistence of employment among disadvantaged workers in excess of that implied by overall labor market conditions.

This difference between the research questions leads to three important distinctions between our setup and B&K's: First, given their focus on aggregate adjustment mechanisms, B&K estimate equations in the change in employment. Because we do not emphasize adjustment, the change in employment does not enter into our system. Rather, the dependent variable in Equation (1) is the level of the detrended e/p.[33] Second, we examine employment (e/p) of the disadvantaged group, rather than employment of the overall population. This allows us to examine the persistence of employment in this group in excess of the persistence in overall labor market conditions.[34] Third, since our focus is on the possible lasting effects of past employment of the disadvantaged group on their current employment *conditional* on overall labor market conditions, we take overall labor market conditions as given. We neither model them in a separate equation nor attempt to identify unexpected changes in those conditions.

Appendix C. Robustness Appendix

C.1 Spectral Analysis of e/p Filters

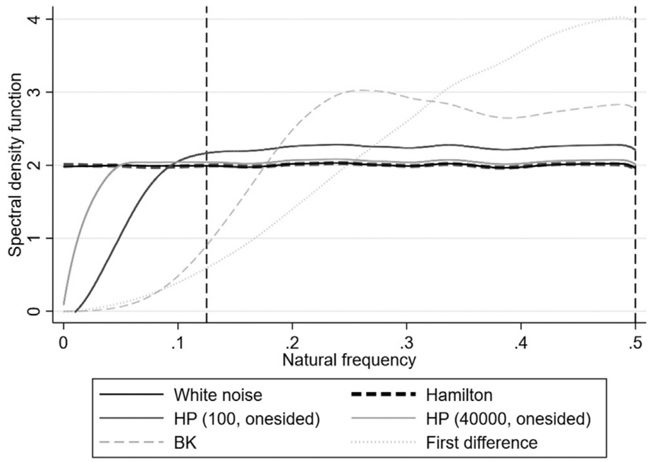
The filters in Section 5.1 amplify various frequencies. Because our qualitative results about excess persistence of e/p are robust to the choice of filter (Table 3), they are not driven by the particular frequencies passed by the filter we chose to feature.

To assess the gain of each filter, we first simulate standard normal white noise for 100,000 periods and compute its spectral density function and then pass this white noise through each of our filters

[33] We do not separately address the LFPR, which is consistent with B&K (footnote 35).

[34] Mechanically, focusing on the e/p of all workers in the B&K setup would mean we would be interested in the coefficient of the lagged e/p on the RHS, but would also include the employment-to-labor-force rate and the LFPR, which imply e/p.

Figure C.1. Spectral Density Function of Different Filters



Note: The filters we use pass through different frequencies of a standard normal white-noise series. The Hamilton approach recovers the spectral density function of the original series, and we use this approach as our baseline for detrending the state e/p of disadvantaged workers. The vertical dashed lines represent the 1/8 and 1/2 frequencies (eight- and two-year periodicities, respectively). See Appendix C.1 for more details.

(in turn) and compute the spectral density of the resulting time series.[35] The results are plotted in Figure C.1. The horizontal solid black line depicts the spectral density function of the original series, which is white noise and represents all the frequencies equally. The vertical dashed lines represent the 1/8 and 1/2 frequencies (eight- and two-year periodicities, respectively). The other lines represent the estimated gain from the various filters. We are reassured that our estimation method recovers the gains accurately for those cases in which the gains are well known (e.g., the first-difference and Baxter-King filters).

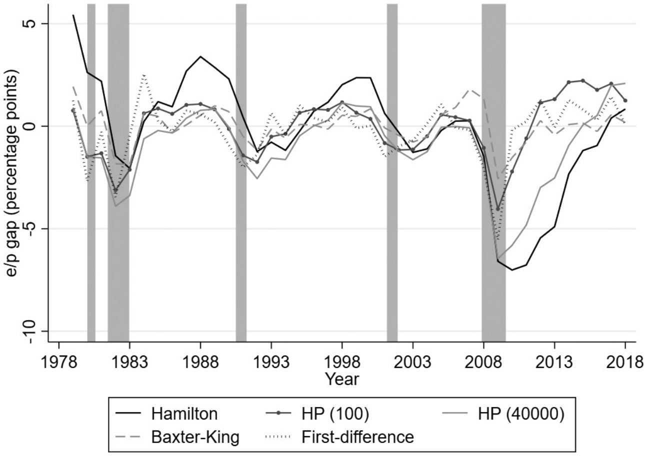
[35] Computing the spectral density function requires one to take a stand on a particular approach and some parameter values. Our approach was to compute the sample cumulative spectral-distribution function, and then compute the sample probability density function (pdf) using small differences. To smooth through the simulation error, we present the lowest-smoothed pdf using a small bandwidth (0.1).

The filters we use pass through different frequencies. The cyclical component from the Hamilton filter with a five-year horizon parameter recovers the spectral density function of the original series. As such, this filter does not amplify or mute any frequencies from the original series. The remaining filters remove lower-frequency components of the time series and pass through higher-frequency components, to a greater or lesser extent. The first-difference filter, as is well known, mostly passes through very high frequencies: it downplays frequencies below 0.25 (periodicities below $1/0.25 = 4$ periods) relative to higher frequencies, with the most weight on a natural frequency of 1/2. The remaining filters have qualitatively similar gain functions. Per design, the Baxter-King filter with a period of two to eight years and a three-year filter window puts less weight on frequencies below 0.18 (periodicities $1/0.18 = 5.6$ periods) and more weight on higher frequencies. The gain function of the HP filter depends on the smoothing parameter, with a higher smoothing parameter placing more weight on lower frequencies. As the HP smoothing parameter rises, the HP-filtered series approaches that of an ideal filter, whose gain would rise from 0 to its maximum level over an infinitesimally small frequency window at 0 frequency.

C.2 e/p Trends for Disjoint Samples

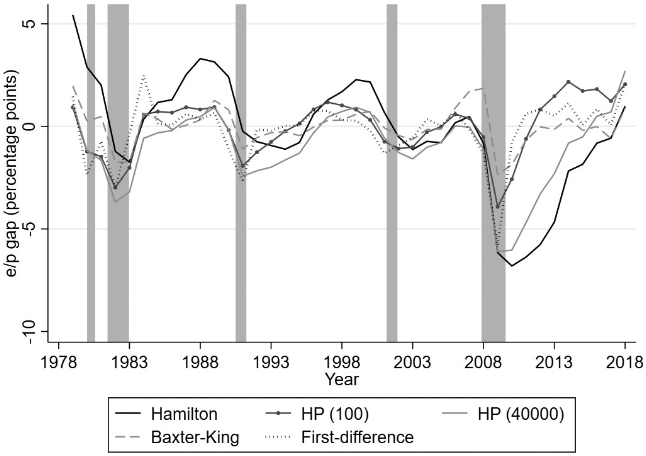
Figure 3 in the main text shows the full-sample estimates of the cyclical component of e/p using various filters, aggregated from the state to the national level. However, as described in Section 2.4, for the estimation we use disjoint samples. Figures C.2 and C.3 replicate Figure 3 for those disjoint samples. The disjoint samples look much like the full sample.

Figure C.2. Estimates of State Detrended e/p, Aggregated, LHS Sample



Note: Trends estimated by five different approaches. Aggregated to the national level. See Appendix C.2 for details.

Figure C.3. Estimates of State Detrended e/p, Aggregated, RHS Sample



Note: Trends estimated by five different approaches. Aggregated to the national level. See Appendix C.2 for details.

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