SPATIAL DYNAMICS AND HETEROGENEITY IN THE CYCLICALITY OF REAL WAGES

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Abstract—Neither the issue of how local and aggregate labor markets interact over time—nor the issue of how heterogeneity by education, race, and other factors interacts with these spatial dynamics—has previously been explored in the literature on the cyclicality of real wages. This study investigates how real wages respond to local and aggregate unemployment rates over time, and explores possible heterogeneities in the responses. Results, based upon data from the Panel Study of Income Dynamics, indicate that real wages move procyclically with both aggregate and local markets, but that the response to local changes occurs with a lag; that rates of return to education are procyclical overall for aggregate labor markets, but tend to be countercyclical for blacks; and that wages of union, manufacturing, blue-collar, and black workers tend to be less procyclical, even countercyclical for black college graduates. Overall, we find substantial spatial dynamics and heterogeneity in the cyclicality of real wages.

I. Introduction

REAL-WAGE adjustment has been a central issue for macroeconomic models since at least the classical models of the last century. Most recently, real-wage cyclicality has been a prominent feature of models of the real-business cycle, efficiency wages, and other macroeconomic models of the labor market. Even so, issues of spatial dynamics (i.e., the connection between local and aggregate labor markets over time) and of how heterogeneity by union status, education, race, and the like interacts with these spatial dynamics have been neglected in the related empirical literature on the cyclicality of real wages.[1] For example, are real wages more responsive to local or aggregate labor-market changes? Is the persistence in the response to aggregate changes dependent upon the responses at the local level? To what extent do the answers depend upon union status, education, race, or other factors? Indeed, in a recent survey on real wages and the business cycle, Abraham and Haltiwanger (1995) argue that ''in discussing real wage cyclicality, it may be most appropriate to characterize the response of local real wages for a particular type of worker to changes in local and aggregate conditions for that type of worker.'' Our unique contribution here is to estimate how real wages respond to local and aggregate unemployment changes over time, and to explore the significance of possible heterogeneities in the spatial responses.

We rely upon longitudinal microdata for prime working-age males from the Panel Study of Income Dynamics for the years 1971 through 1990. Solon et al. (1994) have shown that estimates of real-wage cyclicality from aggregate data are subject to a countercyclical composition bias: the proportion of low-wage workers in the workforce rises during expansions and falls during contractions. In fact, real wages taken from longitudinal microdata, for which the composition can be held constant, are strongly procyclical. In addition, longitudinal microdata permit estimates of a variety of potentially heterogeneous responses. For purposes of comparison with previous studies, we estimate both the first-difference and fixed-effect transformations, each of which provides consistent estimates of model parameters.

Our results, presented in section III, pose a puzzle. In a standard (static) first-difference specification, the aggregate unemployment rate enters significantly, while the local unemployment rate does not. That is, real wages respond to aggregate, not local, unemployment changes. In a standard (static), fixed-effect specification, however, both the aggregate and local unemployment rates enter significantly. In a well-specified model, one would expect comparable results across both specifications. The apparent anomaly turns out to be due both to the omission of dynamics and to measurement error in the local unemployment rate. Specifically, instrumenting the mismeasured local rate and introducing lags yield significant results for local and aggregate unemployment rates across both specifications; that is, real wages move procyclically with both the aggregate and local cycles. However, the timing of labor-market changes differs across space; the aggregate rate affects real wages contemporaneously while the local rate has only a lagged impact. The aggregate rate likely has a rapid impact on wages because it is common across all local economies. However, to the extent that it is more costly to gather information that a purely local change has occurred—and that the underlying structure of local economies differ, as argued by Davis et al. (1997)—then it is plausible that wages adjust only sluggishly to a change in local unemployment rates. In contrast with Blanchflower and Oswald (1994), who argue that wages are set primarily in local labor markets, we find that responses to the aggregate market are also important.

Most previous studies find little heterogeneity in the cyclicality of real wages by union status (Solon et al., 1994), education level (Bils, 1985; Keane & Prasad, 1993; Solon et

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[1] Most recently, Bils (1985), Keane et al. (1988), Blank (1990), and Solon et al. (1994) examine individual real-wages responses to aggregate labor-market changes; Raisian (1983), Tremblay (1990), and Blanchflower and Oswald (1994) examine real-wage responses to local (and/or industry) markets. Blanchflower and Oswald (1994, pp. 212–213) examine local and aggregate markets simultaneously, but only in a static model of wage levels.

al., 1994), or race (Bils, 1985; Blank, 1990; Tremblay, 1990). In contrast, we find substantial heterogeneity once spatial dynamics are introduced and returns to education are allowed to vary by race. Real wages of black workers tend to be acyclical in response to aggregate unemployment. Furthermore, rates of return to schooling are procyclical for both aggregate and local changes for whites, but tend to be countercyclical for blacks. Real wages of union members are less procyclical than those of other workers, which is consistent with the view that union wages tend to be less flexible in response to market changes. The response of real wages by broad industrial sector (manufacturing versus nonmanufacturing) or occupation group (blue-collar versus white-collar) also tends to be less procyclical, at least in response to aggregate changes in unemployment. Overall, these findings suggest substantial heterogeneity by race, education, and union status in the response to changes in both local and aggregate labor markets.

II. Empirical Specifications and Data

A. Empirical Specifications

The empirical model we employ to test for spatial differences and heterogeneity in real-wage cyclicality is a human-capital wage equation augmented with business-cycle controls. The static form is

$$w_{it} = \gamma_{1i} + \gamma_2 UR_t + \gamma_3 CUR_{it} + \gamma_4 \mathrm{t} + \gamma_5 t^2 + \gamma'_6 X_{it} + \gamma'_7 Z_i + \epsilon_{it}, \quad i = 1, \ldots, N; \quad t = 1, \ldots, T \tag{1}$$

where w_{it} is the natural log of the real wage for person i in time t, UR_t is the aggregate unemployment rate, CUR_{it} is the deviation in the unemployment rate in person i's county of residence from the aggregate rate, t and t^2 is a trend and its square, X_{it} is a $k \times 1$ vector of time-varying demographics such as experience and its square, Z_i is a $g \times 1$ vector of time-invariant demographics such as race and education, γ_{1i} is a time-invariant individual fixed effect assumed to be correlated with X_{it} and Z_i, ϵ_{it} is a mean-zero random error permitted to be conditionally heteroskedastic and autocorrelated. The inclusion of the county unemployment rate (CUR_{it}) captures spatial differences in real-wage cyclicality. Moreover, the inclusion of time-varying and time-invariant demographics is intended to control for composition changes in the sample over time. These controls for observed and unobserved heterogeneity should purge our tests of the countercyclical composition bias found in aggregate studies (Solon et al., 1994).

Because of the nuisance parameters, γ_{1i}, least-squares estimates of equation (1) are inconsistent. Consequently, we consider two methods to estimate consistently the model parameters, first-differences, and fixed effects. The static model in first differences is

$$\Delta w_{it} = \beta_1 + \beta_2 \Delta UR_t + \beta_3 \Delta CUR_{it} + \beta_4 t + \beta'_5 \Delta X_{it} + \Delta\epsilon_{it}, \tag{2}$$

where $\beta_1 = \gamma_4 + \gamma_{61} - \gamma_5$, $\beta_2 = \gamma_2$, $\beta_3 = \gamma_3$, $\beta_4 = 2\gamma_5$, $\beta_5 = 2\gamma_{62} + \gamma_{6j}$ ($j = 3, \ldots, k$). The parameters γ_{61} and γ_{62} refer to the coefficients on potential experience and its square, respectively, where the effects of linear time and experience are not identified separately. The business-cycle parameters, β_2 and β_3, now reflect the effect of the aggregate and local cycles on wage growth, as opposed to wage levels in equation (1). This specification is most similar to that used by Bils (1985) and Solon et al. (1994).

An alternative approach to eliminate the latent heterogeneity is the fixed-effects estimator used by Keane et al. (1988), Keane and Prasad (1993), and Tremblay (1990). The static form is

$$\tilde{w}_{it} = \delta_1 \tilde{UR}_t + \delta_2 \tilde{CUR}_{it} + \delta_3 \tilde{t} + \delta_4 \tilde{t}^2 + \delta'_5 \tilde{X}_{it} + \tilde{\epsilon}_{it} \tag{3}$$

where

$$\tilde{w}_{it} = w_{it} - (1/T)\sum_t w_{it}, \quad \tilde{UR}_t = UR_t - (1/T)\sum_t UR_t,$$
$$\tilde{CUR}_{it} = CUR_{it} - (1/T)\sum_t CUR_{it},$$
$$\tilde{t} = t - (1/T)\sum_t t, \quad \tilde{t}^2 = t^2 - (1/T)\sum_t t^2, \quad \text{and}$$
$$\tilde{X}_{it} = X_{it} - (1/T)\sum_t X_{it}, \quad \tilde{\epsilon}_{it} = \epsilon_{it} - (1/T)\sum_t \epsilon_{it}$$

are the variables listed in their deviations from individual time-series means. In this case, $\delta_1 = \gamma_2$, $\delta_2 = \gamma_3$, $\delta_3 = \gamma_4 + \gamma_{61}$, $\delta_4 = \gamma_5$, and $\delta_5 = \gamma_{6j}$ ($j = 2, \ldots, k$). As in the first-differences model, we are not able to separately identify the effects of linear time and potential experience, and thus their effects are collapsed into δ_3. The business-cycle effects now reflect the impact on wage deviations rather than either wage levels or wage growth; however, the coefficients on UR_t and CUR_{it} are identical in equations (2) and (3).

We note that the first-difference and fixed-effects estimators are equivalent asymptotically if the model is well specified. However, the parameter estimates could diverge between the two methods either in small samples or if the model is misspecified. For example, a possible advantage of the first-difference specification is that, if there is a nonstationarity in ϵ_{it}, then differencing makes the process difference stationary, whereas the deviations from time-series means transformation does not. Additionally, a common hazard for the first-difference specification is that it tends to exacerbate any errors-in-variables problems and thus may exaggerate both the attenuation bias in the coefficients (in the case of random errors) and inefficiency of the estimated standard errors (Griliches & Hausman, 1986). We present

TABLE 1.—SPATIAL AND TIMING DIFFERENCES IN REAL-WAGE CYCLICALITY

	First Differences			Fixed Effects		
	(1)	(2)	(3)	(1)	(2)	(3)
UR(t)	−1.15[a]	−1.15[a]	−1.08[a]	−1.31[a]	−1.24[a]	−1.12[a]
	(0.23)	(0.23)	(0.24)	(0.22)	(0.24)	(0.24)
UR($t-1$)			0.14			0.13
			(0.24)			(0.21)
CUR(t)		−0.04	−0.18		−0.80[a]	−0.44[a]
		(0.13)	(0.14)		(0.12)	(0.12)
CUR($t-1$)			−0.40[a]			−0.79[a]
			(0.13)			(0.12)
Wald1			22.51			22.40
[df, p-value]			[2,0.00]			[2,0.00]
Wald2			9.69			54.34
[df, p-value]			[2,0.01]			[2,0.00]
Error Sum of Squares	1029.05	1029.04	926.48	829.78	826.89	668.72
Number of observations	12,108	12,108	11,272	12,944	12,944	12,108

Notes: Heteroskedasticity and autocorrelation consistent standard errors reported in parentheses. The first-difference regressions control for a constant, experience, and a trend, while the fixed-effects regressions control for experience, experience squared, and trend squared. Wald1 is a test for the joint significance of UR(t) and UR($t-1$), while Wald2 is a test for the joint significance of CUR(t) and CUR($t-1$). See the text for details.
[a] Significant at the 1% level.

both first-difference and fixed-effect estimates for comparison, and then explore the potential importance of nonstationarity, misspecified dynamics, and measurement error.

B. Data Issues

The data used in estimation are from the Panel Study of Income Dynamics (PSID), waves IV through XXV (1971–1990). We assemble an unbalanced panel of men that includes ‘‘split-off’’ households. A *split-off* is defined as an individual who ‘‘splits off’’ from a sample family after 1971 to form a separate household. Appendix A describes in detail our sample selection process along with variable descriptions and summary statistics. To summarize, we include only black and white male heads of households, aged at least 20 and less than 45 in 1971, who are in the labor force and have worked at some point in each of the included years, and who have completed schooling. The sample is selected to avoid several potential endogeneity problems such as the endogeneity of women's wages and labor-market experience (Kim & Polachek, 1994), the endogeneity of the retirement decision, and the endogeneity of wages and schooling (Card, 1994). In addition, we exclude self-employed individuals and farmers because their wages are not well defined. If information on an explanatory variable for an individual is not available in a particular year, then that person-year is excluded from the data. We obtain 12,944 observations, comprising 836 individuals (603 white men, and 233 black men).[2]

The dependent variable, w_{it}, is defined as the log ratio of annual earnings to annual hours, deflated by the 1987 personal consumption expenditure deflator. We use annual average hourly earnings, whereas other authors (e.g., Bils, 1985; Keane et al., 1988) have argued for the point-in-time survey week wage. As noted in Abraham and Haltiwanger (1995, p. 1,295), average hourly earnings is the more-preferred wage measure because it is less likely to suffer from sample-selection bias because most workers, particularly our sample of prime-age men, likely work at some point in the year.

III. Empirical Results

We begin our empirical investigation by examining spatial and timing differences in real-wage cyclicality, and then test for heterogeneity in aggregate and local business-cycle responses across union status, industrial sector, occupational group, educational levels, and racial groups. In addition to the business-cycle controls, each of the first-difference regressions has as explanatory variables a constant, potential experience, and a trend, while each of the fixed-effects regressions contains potential experience and its square, along with a squared trend.

A. Spatial and Timing Differences in Real-Wage Cyclicality

Table 1 presents OLS estimates of equations (2) and (3) for three different specifications. For a benchmark comparison with previous studies, we begin with specification (1), where the only business-cycle control is the aggregate unemployment rate. Our estimates of −1.15 from the first-difference regression and −1.31 from the fixed-effect regression are similar, if slightly smaller, to the first-difference estimates of −1.59 from Bils (1985) and −1.40 from Solon et al. (1994). Abraham and Haltiwanger (1995) note that estimates of real-wage cyclicality are sensitive to sample period, and this is the most likely source of our difference from others in the literature.

Our story begins with specification (2), where we permit spatial differences in real-wage cyclicality by controlling for both the contemporaneous aggregate and local unemploy-

[2] In relation to Solon et al. (1994), the sample here is larger than their balanced panel but smaller than the unbalanced panel, because they include younger male household heads and those who have not completed schooling. However, their estimates of real-wage cyclicality were not sensitive to sample size (cf., balanced and unbalanced samples in table II).

ment rates. A puzzle emerges in comparing the first-difference and fixed-effect results. The static first-difference specification suggests that the aggregate unemployment rate enters significantly, while the local unemployment rate does not. On the contrary, the fixed-effects results suggest that both the aggregate and local unemployment rate enter significantly. The implication of the first-difference results is that wages are unresponsive to changes in local labor markets, whereas the fixed-effect results imply that wages are responsive to local as well as national labor markets. Interestingly, the first-difference results are at odds with Blanchflower and Oswald (1994, p. 213) who estimate a static model of wage levels (similar to equation (1)) and conclude that real wages are set primarily in regional labor markets. While the fixed-effects estimates indicate that wages do respond to local labor-market conditions, the effect is smaller than that arising from the aggregate market.

Specification Tests The conflicting results may arise from model misspecification. As noted earlier, nonstationarity, misspecified dynamics, or measurement error can lead to differences in the first-difference and fixed-effects estimators.[3] The first avenue of potential misspecification we explore is nonstationarity. As noted earlier, the first-difference estimator will remove a unit root, if present, rendering a difference-stationary process, but a fixed-effects estimator will not. To investigate this issue, we compute the autocovariance function for the fixed-effects residuals. If there is strong persistence in the autocovariances, then nonstationarity is a potential source of discrepancy between the two estimators. We find that the fixed-effects autocovariances go to zero after two periods; i.e., they equal 0.07 at the second lag and -0.004 at the third lag. Hence, there does not appear to be a nonstationarity in the conditional error variance, so this potential problem does not explain the difference in the first-difference and fixed-effects estimates for aggregate and local cycles. However, the autocovariances do suggest that we correct the standard errors for an MA(2) in fixed-effects and an MA(1) in first-differences.

Dynamics Because the static models in equations (2) and (3) may not fully capture the adjustment of wages to changes in unemployment, the next avenue we pursue is to test for possible misspecification of the model dynamics. The Schwarz criterion indicates the choice of only one lag of the aggregate and local unemployment rates in the first-difference specification, and (marginally) indicates the choice of two lags in fixed effects.[4] To retain comparable specifications for the first-difference and fixed-effect models, and because lags from t-2 are economically small, we employ one lag for both aggregate and local unemployment in the two specifications.[5]

With the inclusion of lagged values of the aggregate and local unemployment rates in specification (3), the first-difference and fixed-effects results are now roughly in accord, indicating that the differences in specification (2) appear to be due in part to misspecified dynamics. This is confirmed by the small p-values from Wald tests of the joint significance of UR_t and UR_{t-1} and of CUR_{it} and CUR_{it-1}. The results suggest that real wages are responsive to both local and aggregate labor-market conditions; however, the timing of the impact differs across space. The aggregate rate has a contemporaneous impact, but no additional lagged effect; alternatively, the local rate has a relatively weak contemporaneous effect but a strong lagged effect. That is, local changes in unemployment must persist longer to have effects on the real wage that approach (and perhaps eventually exceed) those arising from changes in aggregate unemployment.

Perhaps surprisingly, the omission of lagged unemployment rates leads to opposite effects in the first-difference and fixed-effect specifications, as illustrated by the incongruous results for specification (2). The conflicting results arise primarily because the first-order partial autocorrelation in unemployment rates is negative in first differences, but positive in deviations from the mean. The negative autocorrelation in first differences causes the negative coefficient on CUR_{it-1} to offset the negative coefficient on CUR_{it}, resulting in an insignificant coefficient in specification (2). In contrast, the positive autocorrelation in the fixed-effect specification causes just the reverse: the negative coefficient on CUR_{it-1} now reinforces the negative coefficient on CUR_{it}.

Measurement Error While incorporating dynamic adjustment of wages to the local and national unemployment rates goes a long way in reconciling the results across first-difference and fixed-effects specifications, a neglected concern up to now is the possibility of measurement error in the local unemployment rate. County unemployment rates are imputed by using the so-called ''handbook method,'' which relies on data from the Current Population Survey, UI data on insured unemployment, and other establishment-level data (U.S. Bureau of Labor Statistics, 1992). Because the rates are estimated from disparate sources, significant error may arise in their construction. In addition, as described in the data appendix, the county unemployment rate is re-

[3] Another source of difference could be the differential effect of common shocks on the estimated standard errors across the models. In general, ignoring these shocks attenuates downward the estimated standard errors. In results not reported we estimate some two-step models along the lines suggested by Solon et al. (1994) and find that the standard errors are not significantly affected.

[4] We also compute the partial autocorrelations for the aggregate and local unemployment rates. The first-, second-, and third-order autocorrelations for UR_t (standard errors in parentheses) are 0.67 (0.18), -0.42 (0.24), and -0.09 (0.24), respectively. The comparable estimates for CUR_{it} are 0.58 (0.20), -0.40 (0.24), and -0.07 (0.27). For both variables, only the first-order partial autocorrelation is statistically significant at the 5% level.

[5] Wage adjustment could also be modeled with the inclusion of the lagged dependent variable. To test for this adjustment pattern, we estimated the models in table 1, inclusive of the lagged dependent variable, by instrumental variables (IV). The coefficients on the aggregate and local unemployment rates are not significantly affected, although the adjustment is slightly more protracted in fixed effects.

TABLE 2.—IV ESTIMATES OF SPATIAL AND TIMING DIFFERENCES IN REAL-WAGE CYCLICALITY

	First Differences				Fixed Effects			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
UR(t)	−1.14[a]	−1.15[a]	−0.72[b]	−0.79[b]	−1.20[a]	−1.20[a]	−0.94[a]	−0.96[a]
	(0.23)	(0.23)	(0.30)	(0.28)	(0.22)	(0.22)	(0.25)	(0.25)
UR($t-1$)			0.18	0.15			0.20	0.17
			(0.25)	(0.24)			(0.22)	(0.22)
CUR(t)	−1.61[b]	−1.02	0.62	0.18	−1.39[a]	−1.43[a]	0.41	0.11
	(0.75)	(0.61)	(0.46)	(0.44)	(0.34)	(0.33)	(0.41)	(0.42)
CUR($t-1$)			−2.51[b]	−2.10[b]			−2.27[a]	−1.96[a]
			(0.97)	(0.83)			(0.42)	(0.43)
Wald1			5.84	8.06			14.29	15.37
[df, p-value]			[2,0.05]	[2,0.02]			[2,0.00]	[2,0.00]
Wald2			6.89	6.51			36.69	33.22
[df, p-value]			[2,0.03]	[2,0.04]			[2,0.00]	[2,0.00]
Wald3	82.29	180.79			2,080.66	2,378.28		
[df, p-value]	[9,0.00]	[18,0.00]			[9,0.00]	[18,0.00]		
Can. Corr.			0.36	0.38			0.44	0.46
OID	26.12	38.271	17.82	3.20	5.11	14.97	10.58	30.21
[df, p-value]	[8,0.00]	[17,0.00]	[16,0.33]	[34,1.0]	[8,0.74]	[17,0.60]	[16,0.83]	[34,0.65]
Number of observations	12,108	12,108	11,272	11,272	12,944	12,944	12,108	12,108

Notes: Heteroskedasticity and autocorrelation consistent standard errors reported in parentheses. The first-difference regressions control for a constant, experience, and a trend, while the fixed-effects regressions control for experience, experience squared, and trend squared. Wald1 is a test for the joint significance of UR(t) and UR($t-1$), while Wald2 is a test for the joint significance of CUR(t) and CUR($t-1$). Wald3 is a first-stage test of joint significance of instruments. Can. Corr. is the canonical correlation between the instruments and CUR(t) and CUR($t-1$). OID is an overidentifying restrictions test of the validity of the instrument set. See the text for details.

[a] Significant at the 1% level.

[b] Significant at the 5% level.

corded by the PSID as a categorical variable through 1980 (i.e., equal to 1 if $0 < CUR_{it} \leq 2$, equal to 2 if $2 < CUR_{it} \leq 3.9$, etc.), whereas, from 1981 onward, the PSID records the actual unemployment rate. To construct a continuous variable for all twenty years, we set the county unemployment rate equal to the midpoint of each group for all person-years prior to 1981. Consequently, measurement error in the local unemployment rate prior to 1981 may be exacerbated. If so, then measurement error may explain the attenuation and insignificance of the coefficient for the local unemployment rate in the first-difference estimates.

In table 2, we reestimate specifications (2) and (3) from table 1 with instrumental variables. The base-case instrument set in specification (1) of table 2 consists of the deviation in the state unemployment rate from the national rate, the deviation in the natural log of gross state product from federal gross domestic product, the deviation in the natural log of state personal income from national income, and interactions of the latter three instruments with dummy variables indicating whether the worker lives in a large city or a small town. Specification (2) appends to the base-case instrument set a full interaction of those instruments with a dummy variable equal to 1 for survey years after 1980. This interaction is meant to permit a different relationship between the instruments and the local unemployment rate depending on whether the local rate is measured categorically or continuously.[6] Specifications (3) and (4) rely on the instruments sets in (1) and (2), respectively, except that both current and one-period lagged values of the instruments are used. The first-stage Wald tests (Wald3) and canonical correlations (Can. Corr.) suggest that the instrument sets in specifications (2) and (4) are the preferred sets, and thus we focus our discussion on these models.[7]

Comparing specification (2) in table 2 to the same model from table (1), we find an economically and statistically significant change in the estimated effect of the local unemployment rate on real wages. The coefficient on CUR_{it} increases (in absolute value) from −0.04 to −1.02 in first differences, and from −0.80 to −1.43 in fixed effects. Given the large change in the effects of the local rate, it is important to gauge whether these estimated coefficients are plausible. Griliches and Hausman (1986, p. 95) provide a formula for the consistent estimation of a mismeasured coefficient based on the asymptotic plims of the first-difference and fixed-effects estimators. Under the assumption that the measurement error is stationary and uncorrelated over time, their formula suggests the coefficient on CUR_{it} ranges from −1.25 to −1.75, depending on the number of years a person is in our sample. Hence, the estimates in table 2 do seem plausible, and thus it appears that measurement error in the local unemployment rate is substantial. Moreover, correcting for measurement error brings the first-difference and fixed-effects estimates largely in accord relative to the OLS

[6] We estimate models using the post-1980 data alone, and the results are qualitatively similar. Not surprisingly, the small sample results in a substantial loss in efficiency, especially with the national rate because it has only ten years of data and one business cycle to identify its impact.

[7] We use Wald tests rather than F-tests because of conditional heteroskedasticity and autocorrelation. Under the null of i.i.d. errors, the first-difference F-tests are no smaller than 12, while the fixed-effects tests are no smaller than 140, both of which exceed the value of 10 that may signal weak instruments (Staiger & Stock, 1997). The canonical correlation, rather than the F- or Wald test, is recommended as a first-stage test in the presence of multiple endogenous regressors (Bowden & Turkington, 1984). The objective of this measure is to maximize the correlation between the endogenous regressors and the instruments.

estimates, even without resorting to dynamic wage adjustment.

Given that instrumenting the local unemployment rate brings the two estimators into general agreement, are there still interesting unemployment dynamics to explore? The answer from specifications (3) and (4) seems to be yes. The Wald tests (Wald1 and Wald2) reject the null hypothesis that the current and one-period lagged unemployment rates are jointly zero. More important, though, are the spatial differences in the timing of the effects of unemployment changes on real wages. The aggregate rate, similar to table 1, has a contemporaneous impact, but no independent lagged effect. Alternatively, with the use of instruments, the contemporaneous effect of the local rate is zero in both fixed effects and first differences, but the lagged effect is significantly negative (with a large coefficient of approximately -2). Hence, an increase of one percentage point in local unemployment that lasts two years results in an approximately 1.9% reduction (the sum of the current and lagged coefficients) in the real wage, with almost all the effect occurring with a lag.

As Abraham and Haltiwanger (1995, p. 1,261) note, the connection between the national and local labor markets has not been addressed in the empirical real-wage/business-cycle literature. In addressing this issue, we find that real wages respond contemporaneously to changes in the aggregate labor market, but respond with a lag to changes in the local labor market. The aggregate rate likely has a rapid impact on wages because it is common across all local economies. However, to the extent that it is more costly to gather information that a purely local change has occurred, and that the underlying structure of local economies differ, as argued by Davis et al. (1997), then it is plausible that wages adjust only sluggishly to a change in local unemployment rates. Unlike Blanchflower and Oswald (1994), who find that real wages are set primarily in regional labor markets, we find that the aggregate business cycle is also important for real wages, although its timing differs from the local level.

B. *Heterogeneity in Real-Wage Cyclicality*

The empirical literature on the cyclicality of real wages has also focused on testing for heterogeneous responses in real wages over the cycle by various demographic groups such as union status (Solon et al., 1994), education level (Bils, 1985; Keane & Prasad, 1993; Solon et al., 1994), or race (Bils, 1985; Blank, 1990; Tremblay, 1990). The consensus of these tests is that there is little heterogeneity in cyclical wage responses for any of these groups. We reexamine the issues of heterogeneity for each of these groups, along with those classified by broad manufacturing sectors and occupational groups, based upon our wage models corrected for measurement error and augmented with spatial dynamics, i.e., with contemporaneous and lagged aggregate and local unemployment rates (specification (4) of table 2). The instrument set is the same as in table 2, column (4), with the addition of a full interaction of the demographic indicator with the instruments. This results in 68 overidentifying restrictions. Based on the large efficiency gain relative to first differences, we rely at this point on estimates from the fixed-effects estimator to explore the potential importance of heterogeneities. However, the coefficients from first-differences are qualitatively similar.

Union, Industry, and Occupational Heterogeneity We first test for heterogeneity in the cyclicality of real wages for workers belonging to a union, workers in the manufacturing sector, and for workers in blue-collar occupations. We report the results of our tests in table 3. The IV fixed-effect estimates suggest that the wages of union members are less procyclical in response to both aggregate and local conditions than those of nonunion workers. This is consistent with the view that union wages tend to be less flexible in response to market conditions than those of other workers, possibly because of fixed-term contracts or because of inflexible wages even between contracts.

In addition, we find differences in individual coefficients for workers classified by broad occupational groups. Real wages for blue-collar workers, as compared to those of other workers, are significantly less procyclical in response to changes in the aggregate unemployment rate, but appear to be as procyclical at the local level. This pattern suggests that the relevant market for blue-collar workers is the local labor market. Likewise, real wages for manufacturing workers are also substantially and significantly less procyclical than those of other workers, a conclusion roughly consistent with Keane (1993, p. 160), who concludes that interindustry wage differentials are largely acyclical.[8]

Skill Heterogeneity One might expect real-wage responses to differ by skill or education level. For example, higher levels of education should broaden the scope of the relevant market. If so, then, in the absence of other intervening factors, the procyclicality of real wages would increase with the level of education for aggregate changes, but not necessarily for local changes. In this case, the real wages of college graduates would be more responsive to the broader national market than those of high-school graduates, and the real wages of high-school graduates more responsive than those of nonhigh-school graduates.

Column (4) of table 3 presents estimates of differences in real-wage cyclicality by education level. The regression variables are now scaled by the overall sample mean. Therefore, the primary (i.e., noninteracted) coefficients on the business-cycle indicators represent the effect of business cycles on real wages for workers with the mean level of

[8] The overidentifying restrictions are rejected for the specifications with manufacturing, blue collar, and race. However, there is evidence in Brown and Newey (1995), Hall and Horowitz (1996), and Ziliak (1997) that the OID test has a tendency to over-reject when the instrument set is relatively large, as in table 3. We also estimate models with more parsimonious instrument sets with no qualitative change in the coefficients, but a loss in efficiency.

TABLE 3.—IV FIXED-EFFECTS ESTIMATES OF REAL-WAGE CYCLICALITY BY SELECTED DEMOGRAPHICS

X_t:	Union	Manufacturing	Blue Collar	Education	Race
UR(t)	-1.14^a	-1.11^a	-1.51^a	-0.95^a	-0.91^a
	(0.27)	(0.29)	(0.33)	(0.25)	(0.28)
UR($t-1$)	0.06	−0.08	−0.07	0.19	−0.15
	(0.22)	(0.22)	(0.23)	(0.22)	(0.24)
CUR(t)	−0.12	0.15	0.18	0.09	−0.05
	(0.43)	(0.47)	(0.49)	(0.39)	(0.44)
CUR($t-1$)	-2.44^a	-2.02^a	-2.29^a	-2.06^a	-1.95^a
	(0.46)	(0.51)	(0.52)	(0.41)	(0.44)
$X_t *$ UR(t)	0.27	0.18	0.91^b	−0.10	−0.12
	(0.38)	(0.36)	(0.37)	(0.08)	(0.50)
$X_t *$ UR($t-1$)	0.49^a	0.76^a	0.42^a	0.02	1.15^b
	(0.15)	(0.14)	(0.14)	(0.09)	(0.55)
$X_t *$ CUR(t)	0.37	−0.03	−0.29	−0.09	−0.31
	(0.53)	(0.56)	(0.53)	(0.18)	(0.90)
$X_t *$ CUR($t-1$)	1.48^a	0.66	0.67	0.13	0.25
	(0.55)	(0.54)	(0.56)	(0.17)	(0.95)
Wald	19.52	31.25	17.75	2.77	6.08
[df, p-value]	[4,0.00]	[4,0.00]	[4,0.00]	[4,0.59]	[4,0.19]
OID	94.36	163.38	128.07	91.77	126.72
[df, p-value]	[68,0.02]	[68,0.00]	[68,0.00]	[68,0.03]	[68,0.00]
Number of observations	12,108	12,108	12,108	12,108	12,108

Notes: Heteroskedasticity and autocorrelation consistent standard errors reported in parentheses. The fixed-effects regressions control for experience, experience squared, and trend squared. The Wald test is of the null hypothesis that the interactions of union status, manufacturing sector, occupation, education level, or race with the business cycle indicators are jointly zero. OID is an overidentifying restrictions test of the validity of the instrument set. See the text for details.
[a] Significant at the 1% level.
[b] Significant at the 5% level.

education (12.2 years of education). The interacted education coefficients measure the marginal impact of business cycles on wages for workers with more or less education than the mean. Although the interacted coefficient between education and the aggregate rate has the expected negative sign, it (along with the other interactions) is jointly insignificant.

Racial Heterogeneity Wages for black and white male workers might differ over the business cycle for a variety of reasons, including differences in human-capital investment or possible labor-market wage discrimination. For example, Becker's (1971) ''taste for discrimination'' model postulates that, in tight labor markets, discriminating employers increase their relative demand for black workers, and thus the wages of black workers should be more procyclical than whites.

Column (5) of table 3 presents differences in real-wage cyclicality for black and white men. Unlike most previous studies, which find little difference between white and black workers in this regard, we find that real wages of black workers tend to be relatively acyclical for changes in aggregate unemployment (in the sense that the sum of all the aggregate coefficients is near zero for blacks). Although the Wald test does not reject the null that all four of the interacted coefficients are jointly zero, it does reject for the subset of aggregate rates with a p-value of 0.06. Similar to the case of blue-collar workers, that wages for black workers are not very responsive to the aggregate rate suggests that markets are localized for the average black worker.

Race-Skill Interactions Substantive differences between white and black workers might still exist at the local level but may be obscured by the simple interactions in table 3. For example, there may be racial heterogeneity in both aggregate and local real-wage cyclicality across skill levels, such as education. Indeed, there is no reason to expect the predictions of Becker's model for the effects of discrimination on real-wage cyclicality to be invariant to the level of education. In table 4, we present racial differences in real-wage cyclicality by education level. We estimate the model separately for blacks and whites, and present marginal effects of a change in the labor-market indicator evaluated at three levels of education: the individual cell mean (10.8 for blacks, 12.7 for whites), the overall sample mean (12.2, about high school), and college (16 years).[9] The last column of table 4 presents p-values from t-tests of the null hypothesis that the individual marginal effects are the same across racial groups. With differences in returns to education by race permitted, evidence of racial heterogeneity in the cyclicality of real wages is now apparent in several cases, as noted by the statistically significant difference (10% level or better) between whites and blacks for five of the twelve marginal effects. The significance of several of the results in table 4, as compared to the pooled estimates in table 3, suggests that the common finding of no heterogeneity in business-cycle responses by education or race is due, at least in part, to the inappropriate pooling of white and black workers by education levels.

The most striking differences between white and black workers in table 4 appear to be for college graduates. These differences are driven by the fact that the rate-of-return to

[9] We relax the linear assumption on education and estimated models with dummy variables for less than high school and more than high school. The results are qualitatively similar, but less efficient due to the large number of interactions.

TABLE 4.—RACIAL DIFFERENCES IN REAL-WAGE CYCLICALITY BY EDUCATION

	Whites	Blacks	*P*-value of Difference
Cell Average:			
UR(t)	−0.64[b]	−0.85[c]	0.72
	(0.30)	(0.49)	
UR(t − 1)	−0.31	0.77	0.05
	(0.25)	(0.49)	
CUR(t)	−0.29	−0.60	0.75
	(0.43)	(0.86)	
CUR(t − 1)	−1.80[a]	−0.08	0.09
	(0.43)	(0.94)	
Sample Average:			
UR(t)	−0.47	−0.24	0.73
	(0.32)	(0.57)	
UR(t − 1)	−0.43	0.45	0.20
	(0.27)	(0.62)	
CUR(t)	−0.38	−1.12	0.55
	(0.45)	(1.14)	
CUR(t − 1)	−1.74[a]	1.05	0.03
	(0.45)	(1.23)	
College:			
UR(t)	−1.75[a]	1.38	0.00
	(0.34)	(1.00)	
UR(t − 1)	0.44	−0.39	0.51
	(0.32)	(1.22)	
CUR(t)	0.27	−2.50	0.23
	(0.65)	(2.21)	
CUR(t − 1)	−2.18[a]	4.07	0.01
	(0.60)	(2.25)	
Number of observations	8,877	3,231	

Notes: Heteroskedasticity and autocorrelation consistent standard errors reported in parentheses. The cell average education level is 10.8 for blacks, and 12.73 for whites. The sample average education level is 12.214. The college education level is 16.

[a] Significant at the 1% level.
[b] Significant at the 5% level.
[c] Significant at the 10% level.

schooling is procyclical for whites, but countercyclical for black workers.[10] Indeed, for contemporaneous changes in aggregate unemployment, real wages of black college graduates are weakly countercyclical (1.38, s.e. = 1.00), but real wages for white college graduates are strongly procyclical (−1.75, s.e. = 0.34). The countercyclical response for the wages of black college graduates is also apparent at the lagged local level (4.07, s.e. = 2.25) relative to the procyclical response for white graduates (−2.18, s.e. = 0.60). Moreover, the wages of average black workers are acyclical at the local level and significantly different from white workers.

These patterns indicate that for blacks the wages of highly educated workers increase relative to those of less-educated workers in economic downturns and decrease during upturns, whereas the reverse is true for whites. For white college graduates, the finding that wages increase relative to those of less-educated workers during upturns and decrease during downturns has an expedient interpretation: wages of white workers with college degrees are more responsive to the broader national market. For black college graduates, the interpretation is less obvious. One plausible interpretation, however, is that less-educated black workers are the most-marginalized workers (as compared to highly educated black workers and all white workers), and, hence, they benefit most from economic growth and suffer the most from declines. This interpretation is supported, for example, by the fact that the rate of return to education for black workers is also countercyclical at the local level, which suggests that the market-broadening effects of education—which for whites makes the wages of highly educated workers more procyclical—are dominated in the case of black workers by the relative gains (losses) of highly educated blacks during downturns (upturns), whether in response to aggregate or local labor-market changes.

[10] Kniesner et al. (1978, 1980), with data from the National Longitudinal Survey, find procylical wages for blacks, but nearly acyclical rates of return to education for young white and black men in annual earnings (level) regressions. Our results are not directly comparable because Kniesner et al. use a less heterogeneous sample and only four years of data, assume that the unobserved heterogeneity is uncorrelated with other regressors, and estimate an earnings equation with annual weeks worked as a regressor, which is likely to be endogenous.

IV. Conclusions

Overall, in contrast to results based upon aggregate data, but consistent with the recent results of Solon et al. (1994), our findings indicate that real wages are strongly procyclical. Our focus, however, is on how local and aggregate markets interact over time, and on how these spatial responses may differ by education, race, and other factors. We find that the static first-difference and fixed-effects estimators give divergent results in longitudinal microdata with regard to the relative responsiveness of real wages to aggregate and local labor markets. However, with correction for measurement error in the local unemployment rate and incorporating spatial dynamics, the results from first-differences and fixed-effects are in accord. Specifically, real wages tend to move procyclically with both aggregate and local cycles, but the response to the aggregate rate is contemporaneous while the response to the local rate occurs with a lag.

In addition, we find substantial heterogeneity across broad demographic groups in wage responses to business-cycle conditions. Rates of return to education for blacks are countercyclical for both aggregate and local cycles (perhaps indicating that less-educated black workers are so marginalized that they fare relatively best during economic expansions, and worse during contractions). Wages of union members (and to a lesser extent, those of manufacturing and blue-collar workers) are less procyclical in response to either aggregate or local conditions, indicating that their wages are less flexible in response to changes in market conditions than those of other workers.

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APPENDIX A

In this appendix, we summarize both the sample-selection procedure for our PSID data and the construction of the variables. In the sample, we include only black and white male heads of households between the ages of 20 and 45 in 1971. This makes the oldest sample observation 64 years old in 1990 and permits us to abstract from endogenous retirement decisions. Each sample member must have worked at some point in each of the included years; thus, we exclude individuals who are unemployed due to retirement, permanent disabilities, or student status. In addition, we exclude self-employed individuals and farmers because their wages are not well defined. If information on an explanatory variable for an individual is not available in a particular year, or if the individual has a wage of zero, then that person-year is excluded from the data. Because we are working with prime-age men, sample-selection bias is not likely to be of major concern as these individuals are likely to work at some point in the year. In fact, after imposing all other sample-selection rules described above, we have only 53 person-years out of 12,997 possible in the sample reporting no annual earnings. However, as black men are over-represented (45%) in the group with missing wages, we test for sample-selection bias by constructing the inverse of Mill's ratio from a reduced-form probit, labor-force participation equation (nonlabor income and number of children in the household are identifying regressors). The selection-correction term in the estimated wage equation is statistically zero and does not affect the other coefficients. Consequently, the time-varying and time-invariant demographics, along with fixed effects, seem to be sufficient controls for sample composition.

The variable for which missing values is most acute is the county unemployment rate variable. We attempted to impute the missing county unemployment rates by actually replacing the values by hand using the identifier for county-of-residence. However, beginning in 1989, the PSID stopped releasing the identifier for county-of-residence for confidentiality purposes. This information is available only after signing an unlimited liability contract, which the State of Oregon will not underwrite. We were unsuccessful after several attempts at obtaining the county identifiers, and so we unfortunately exclude the missing person-years. After these deletions, we obtain 12,944 observations.

Numerous coding inconsistencies over the sample period require us to manually alter the data. The county unemployment rate is coded categorically (1 = unemployment less than 2%, 2 = unemployment between 2% and 3.9%, etc.) until 1981 when the actual rate is reported. Therefore, we choose the midpoint of each group as the county unemployment rate for each person-year until 1981. The union variable is problematic because data for this variable was not collected in 1973. Consequently, we infer the most-likely union status for each individual based on their union status in 1972 and 1974, and their occupation and industry reported in 1973. If the individual is a union member in 1972 and 1974, then we assume they are a union member in 1973. If an individual is a union member in 1972 and has the same occupation in 1972 and 1973, then we assume they are a union member in 1973. There are a small number of individuals (23) who change union status and their occupation in 1972, 1973, and 1974. In these cases, we make a best guess as to their union status on an individual basis, by reviewing their occupation and industry in 1973 and analyzing their union status in other years in which they report the same occupation and industry. Chowdury and Nickell (1985) test extensively the measurement error in the PSID's union variable, and their results suggest that the estimated union premium estimated with PSID data is biased downward, but they also show that ignoring measurement error in the union variable does not distort the other parameter estimates, e.g., the business-cycle variables. This measurement error implies that the estimated union-status heterogeneity over the business cycle reported in table 3 is likely understated.

The education variable is defined as years of schooling, where ''17 years'' is actually 17 or more years of education. Fortunately, we are able to determine whether an individual has a graduate degree through alternate data. However, we do not know whether the graduate degree is a Masters, Ph.D., or other degree. Therefore, we add a top-code for the education variable at 19 years for those individuals who have graduate degrees. Table A.1 summarizes the variable descriptions, and table A.2 presents the descriptive statistics.

TABLE A.1.—VARIABLE DESCRIPTIONS

Dependent Variable:	
Wage:	Natural log of the real wage defined as the log ratio of annual earnings to annual hours deflated by the 1987 personal consumption expenditure deflator.
Time-Varying Regressors (X):	
Experience:	Age-Education-6
Occupation:	Dummy variable equal to 1 if blue-collar worker
Industry:	Dummy variable equal to 1 if work in manufacturing industry
Union:	Dummy variable equal to 1 if union member
UR:	Aggregate unemployment rate
CUR:	County unemployment rate
Trend:	Time trend equal to 1, . . . , 20
Time-Invariant Regressors (Z):	
Black:	Dummy variable equal to 1 if black
Education:	Number of years completed schooling (1–19)

TABLE A.2.—SUMMARY STATISTICS FOR THE UNBALANCED PANEL OF MEN FROM THE PSID OVER THE PERIOD 1971–1990

Variable	Whole Sample	Blacks	Whites
Wage	2.458	2.156	2.568
	(0.525)	(0.539)	(0.474)
Black	0.267	1	0
	(0.442)	(0)	(0)
Occupation	0.610	0.777	0.549
	(0.488)	(0.416)	(0.498)
Industry	0.363	0.385	0.355
	(0.481)	(0.487)	(0.478)
Experience	18.724	19.517	18.435
	(10.078)	(10.723)	(9.817)
Experience^2	452.140	495.877	436.221
	(442.675)	(500.381)	(418.604)
Union	0.32	0.333	0.316
	(0.467)	(0.471)	(0.465)
Education	12.214	10.800	12.728
	(2.703)	(2.690)	(2.517)
CUR	0.064	0.067	0.062
	(0.028)	(0.027)	(0.028)
UR	0.068	0.068	0.068
	(0.014)	(0.014)	(0.014)
Trend	11.276	11.484	11.201
	(5.677)	(5.625)	(5.695)
Number of observations	12,944	3,464	9,480

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