Entrepreneurship and the Policy Environment

Yannis Georgellis and Howard J. Wall

This paper uses a panel approach to examine the effect that the government-policy environment has on the level of entrepreneurship. Specifically, the authors investigate whether marginal income tax rates and bankruptcy exemptions influence rates of entrepreneurship. Whereas previous work in the literature finds that both policies are positively related to entrepreneurship, these results show non-monotonic relationships: a U-shaped relationship between marginal tax rates and entrepreneurship and an S-shaped relationship between bankruptcy exemptions and entrepreneurship.

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Entrepreneurship is thought to be an important factor in cultivating innovation, employment, and economic growth. Because the benefits flowing from entrepreneurship are not necessarily captured by the entrepreneurs themselves, but can be realized more generally, the case is often made that the level of entrepreneurship is below its social optimum and deserves some attention from policymakers. Despite the recognized importance of entrepreneurship, however, there has been relatively little empirical analysis of the role played by the government-policy environment.

Previous research on self-employment and entrepreneurship has examined the roles of various demographic, human capital, and financial considerations in a person's decision to become an entrepreneur. Typically, studies have indicated the importance of (i) the earnings differential between entrepreneurship and paid employment (Rees and Shah, 1986; Gill, 1988; and Hamilton, 2000); (ii) liquidity constraints (Evans and Jovanovic, 1989; Evans and Leighton, 1989; Holtz-Eakin, Joulfaian, and Rosen, 1994a,b; and Black and Strahan, 2002); (iii) satisfaction differentials (Taylor, 1996; Blanchflower and Oswald, 1998; and Blanchflower, 2000); (iv) macroeconomic conditions (Taylor, 1996; Parker, 1996; and Cowling and Mitchell, 1997); and (v) intergenerational human capital transfers (Dunn and Holtz-Eakin, 2000; and Hout and Rosen, 2000).[1]

Empirical studies that have considered the effects of the policy environment on entrepreneurship have focused on personal income tax rates, with the expectation that higher tax rates should suppress entrepreneurship. Nearly all studies, however, have found a positive relationship, whether it is between tax rates and aggregate rates of entrepreneurship (Long, 1982a; Evans and Leighton, 1989; Blau, 1987; Parker, 1996; Robson, 1998; and Bruce and Mohsin, 2003) or between tax rates and the likelihood that an individual will be an entrepreneur (Long, 1982b; Schuetze, 2000; and Fan and White, 2003).

The divergence between expectations and results with regard to the effects of the personal

[1] Le (1999) provides a fairly comprehensive survey of the empirical literature.

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income tax is usually attributed to the perception that, because of the nature of a tax system that relies on self-reporting, being an entrepreneur allows for relatively greater opportunities for tax evasion.[2] Cullen and Gordon (2002), however, argue that, because entrepreneurs decide whether or not to incorporate their business, and because personal income tax rates are higher than corporate rates, the tax system provides a net subsidy to risk-taking. This net subsidy arises because an entrepreneur facing losses would prefer to face personal income tax rates so that the deduction of the losses against other income would have greater tax-reducing value. All else equal, an increase in personal income tax rates makes this option more valuable, thereby increasing the likelihood that someone would choose to become an entrepreneur.

Other studies have begun to look at the question of taxes and entrepreneurship using more-complicated indicators of the tax system. Robson and Wren (1999) separate the effects of average and marginal tax rates, suggesting that the former represents the incentive for tax evasion while the latter represents the disincentive.[3] Bruce (2000) looks at the differential tax treatment of self-employment and wage-and-salary earnings, finding that marginal and average tax rates on self-employment earnings are negatively related to the probability of becoming self-employed. Gentry and Hubbard (2000) find that the more progressive a tax system is, the less likely it is that an individual will enter self-employment. Bruce, Deskins, and Mohsin (2004) look at state-level differences in a variety of tax policies, including rates of sales taxes and personal and corporate income taxes, along with whether states allow combined reporting and limited liability corporations.

A recently opened line of inquiry into the effects of the policy environment on entrepreneurship has raised the question of whether or not bankruptcy laws affect the number of entrepreneurs (Berkowitz and White, 2004; Fan and White, 2003; and White, 2001). Briefly, U.S. bankruptcy laws allow individuals filing for personal bankruptcy to exempt some of their assets and income from distribution to their creditors. The exemptions, which differ a great deal across states, can include some or all of the value of a person's home (the homestead exemption), pension holdings, and an assortment of other assets.[4]

The direct effect of these exemptions is to provide a sort of wealth insurance in the event that an entrepreneurial venture fails. Thus, through this wealth-insurance effect, higher exemption levels should lead to more entrepreneurs. Less direct than the wealth-insurance effect is a credit-access effect, which works in the opposite direction. It arises because banks and other credit providers adjust their actions in response to changes in bankruptcy exemptions. As a result, the higher the exemption level, the less credit will be available at a given interest rate.[5] These two opposing effects of bankruptcy exemptions on entrepreneurship mean that the sign of the total effect is ambiguous in general. However, Fan and White (2003) find that the wealth-insurance effect dominates the credit-access effect for all levels of the exemption. In fact, they find that homeowners in states with an unlimited homestead exemption are 35 percent more likely to be self-employed than equivalent homeowners in states with low exemption levels.

In an attempt to resolve the discrepancies in estimating the effects of taxes and to enhance the modeling of bankruptcy exemptions, we estimate the effects of government policies on entrepreneurship in a different way. Specifically, following Georgellis and Wall (2000a), we create a state-level panel dataset that pools observations over space and time.[6] This allows us to look at the effects of

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[2] Robson and Wren (1999) is an exception that finds a negative relationship between tax rates and entrepreneurship. The authors also have a theoretical model of tax evasion and the entrepreneurial decision.

[3] Their theoretical model separates the tax effects into pure marginal and pure average tax changes, roughly analogous to substitution and income effects. Unfortunately, the tax rates they use in their empirical analysis are simply the average and marginal tax rates, each of which has income and substitution effects.

[4] For detailed discussions of U.S. personal bankruptcy laws and the incentives they create, see White (1998), Fay, Hurst, and White (2002), Gropp, Scholz, and White (1997), and Dye (1986).

[5] Berkowitz and White (2004) show how small, unincorporated businesses face lower credit access and higher interest rates in states with higher exemption levels.

[6] See also Wall (2004), Bruce, Deskins, and Mohsin (2004), and Black and Strahan (2002).

changes in policies over time while exploiting the large differences across states in levels of entrepreneurship, bankruptcy exemptions, and tax rates. The advantages of this approach over aggregate time-series studies—which have only one observation per time period—are that we can include a large number of control variables, use more-general specifications of policy variables, and control for trends more effectively. Another advantage, which we outline in greater detail below, is that it allows us to create a continuous variable for the homestead exemption, rather than having to group different exemption levels together into dummy variables, as is necessary when using individual-level panels.

Using the panel approach, we find a U-shaped relationship between marginal tax rates and entrepreneurship. At low tax rates the relationship is negative, and at high rates it is positive. Also, we find an S-shaped relationship between the homestead exemption and entrepreneurship. Specifically, an increase in the homestead exemption from very low or very high levels acts to reduce the number of entrepreneurs, while an increase in the middle range acts to increase the number of entrepreneurs.

SPATIAL AND TEMPORAL TRENDS IN U.S. ENTREPRENEURSHIP

We define the rate of entrepreneurship as the proportion of the working-age population that is classified as nonfarm proprietors. As with most of the literature, we exclude farm proprietors on the grounds that the decision to become a farm proprietor depends on different factors than the decision to become a nonfarm proprietor; also, farmers operate under their own set of bankruptcy laws.

Proprietors' employment is the number of people who are employed in their own business, regardless of whether that business is incorporated. Various other definitions of entrepreneurship have been used in the literature, such as the nonfarm self-employed, which excludes farmers and the incorporated.[7] The rate of entrepreneurship is usually calculated with the labor force or total employment in the denominator. We prefer to use the working-age population (ages 18-64) because, unlike the size of the labor force or the number employed, it is not likely to move with the number of entrepreneurs as people move between employment states. This distinction also recognizes the fact that entrepreneurs are drawn from the entire working-age population, not just those currently employed or in the labor force.

Figure 1 illustrates the cross-state differences in the levels and growth of entrepreneurship during our sample period, 1991-98. In general, states in the western half of the country had the highest levels of entrepreneurship. The eastern part of the country contained all of the regions with the lowest rates of entrepreneurship: the Great Lakes, the Upper South, and the Deep South. In the East, only New England states were in the top two quartiles of entrepreneurship. As Figure 1B shows, all states saw increases in their rates of entrepreneurship between 1991 and 1998, and there was some convergence. Southern states, New York, and some of the lagging western states had the highest growth in entrepreneurship.

EMPIRICAL MODEL

Following Georgellis and Wall (2000a), we estimate state rates of entrepreneurship with the following regression equation, using t to denote the time period and i to denote the state:

$$(1)\qquad E_{it} = \alpha_i + \tau_t + \boldsymbol{\beta}'\mathbf{X_{it}} + \boldsymbol{\theta}'\mathbf{Z_{it}} + \boldsymbol{\gamma}'\mathbf{G_{it}} + \varepsilon_{it}.$$

In the above expression, α_i is a state-specific component that is constant over time and τ_t is a year-specific component that is common to all states. The vectors $\mathbf{Z_{it}}$ and $\mathbf{X_{it}}$ measure, respectively, lagged business conditions and lagged average demographic characteristics in state i in year t. Government policy variables are included in the vector $\mathbf{G_{it}}$, and ε_{it} is the error term.

The demographic variables included in $\mathbf{X_{it}}$ capture the spatial and temporal differences in age, gender, and racial compositions of state employment. As outlined in Georgellis and Wall (2000b), rates of self-employment differ a great deal across these categories. We therefore include

[7] Bruce and Holtz-Eakin (2001) examine a variety of measures and conclude that it makes little difference which is used.

Figure 1A

Average Rates of Entrepreneurship, 1991-98

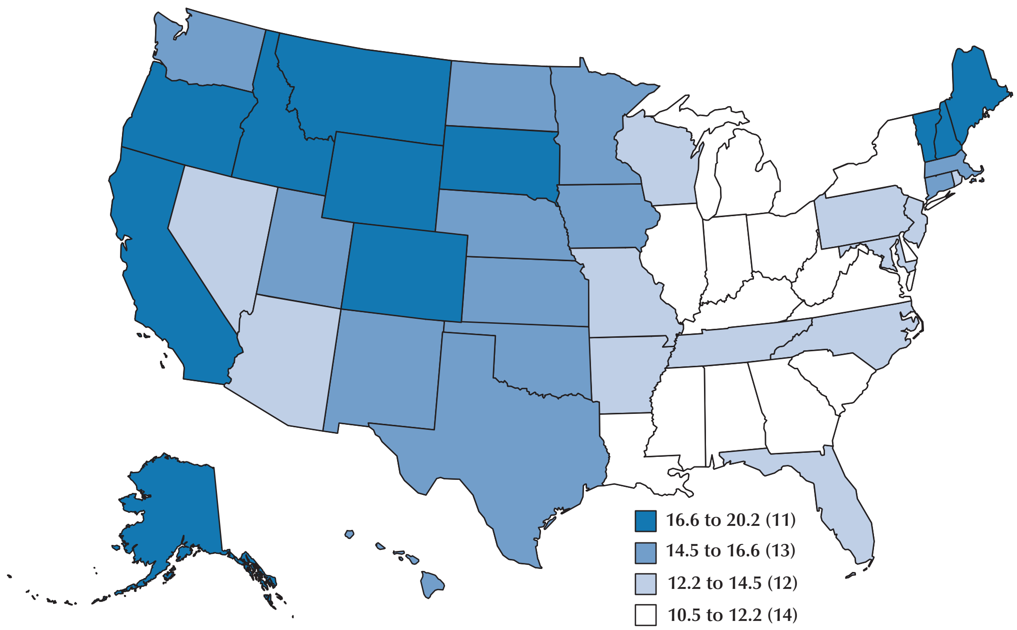
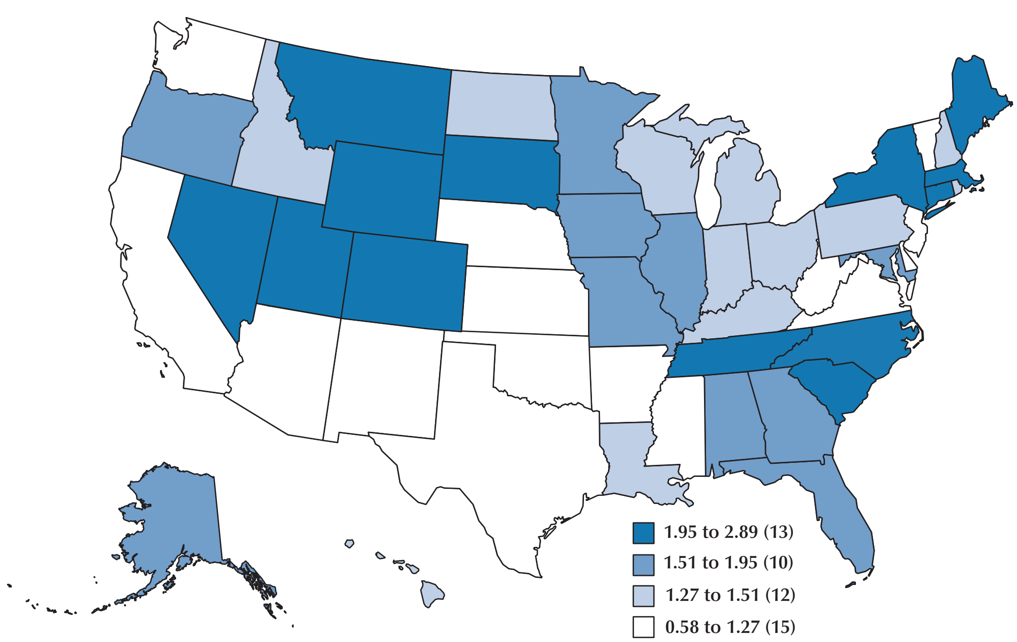


Figure 1B

Changes in Rates of Entrepreneurship, 1991-98



age variables that measure differences in employment shares of broad age categories. Also, because men are nearly twice as likely as women to be self-employed, we include the female share of a state's employment. Finally, $\mathbf{X}_{it}$ includes the black, Native American, Asian and Pacific Islander, and Hispanic employment shares. Large variations in self-employment across these groups might explain state-level differences in entrepreneurship. For example, the self-employment rate for blacks is only about one-third of that for whites and Asians.

Here and in the previous section we discuss these variables in terms of the supply of potential entrepreneurs. However, one should be careful about the interpretation of the estimated coefficients because these demographic groups might also differ in their demand for the products that are more likely to be produced by entrepreneurs. For example, as Georgellis and Wall (2000b) report, over 10 percent of self-employed women in 1997 were in the child-care business, while virtually no men were. This indicates that a state with a higher-than-average female employment share might have a higher-than-average supply of child-care providers. On the other hand, such a state also has a higher-than-average number of women demanding child-care services.

The vector of business conditions, $\mathbf{Z}_{it}$, includes measures of a state's economy that affect the profitability of entrepreneurship. These include the state's unemployment rate, per capita real income, per capita real wealth (as proxied by dividends, interest, and rent), relative proprietor's wage, and industry employment shares. As with our demographic variables, the interpretation of the roles of these variables is not entirely clear because each can simultaneously indicate the demand for entrepreneurs' services and the supply of entrepreneurs. For example, while we include the unemployment rate as a measure of the health of a state's economy, Parker (1996), among others, includes it as an indicator of the number of people with limited opportunities for wage-and-salary employment who might be pushed into self-employment.

As Georgellis and Wall (2000a) demonstrate, the specification of our control variables—the elements of $\mathbf{X}_{it}$ and $\mathbf{Z}_{it}$—is potentially important. The authors show, for example, that the relationship between the rates of self-employment and unemployment in Britain is hill-shaped. Indeed, the best fit in the present context would allow for nonlinear relationships. Nonetheless, our present purpose is to estimate the effects of taxes and the homestead exemption, and a simple linear specification for the control variables makes little difference in this regard. Therefore, for parsimony, we use a linear specification for these control variables.

Presently, the variables of most interest are those measuring marginal tax rates and the homestead exemption. For the former, we use the maximum marginal tax rates (state plus federal) as generated by the National Bureau of Economic Research's TAXSIM model (see Table 1 for the state maximum marginal tax rates in 1990 and 1997, the first and last years of data used in our study). Of the tax rate measures used in the literature, this one best fits our needs. For one, it is the measure used in the paper most comparable to ours—Fan and White (2003). But, more importantly, it is exogenous, unlike the average marginal tax rate also generated by TAXSIM. Although very few people will actually face the maximum marginal tax rate, there should be a very strong correlation between the marginal tax rates that the average person faces and the maximum rate.

We constructed our homestead exemption variable to take into account several state-level differences in bankruptcy law and to provide a measure of the percentage of the value of the average person's home that is exempt from bankruptcy proceedings. First, as noted above and as summarized by Table 1, there are large differences in the exemption level across states: In 1997, five states did not allow any homestead exemption, whereas seven had an unlimited exemption. Also, some states allow for the federal exemption to be substituted at the filer's discretion, and some states allow married filers to double the exemption level. We also take into account differences in the average house prices and the likelihood that a filer owns rather than rents.

Our homestead exemption variable starts by taking the state exemption level or, if the state

Table 1

State Homestead Exemptions and Maximum Marginal Tax Rates

	Maximum marginal tax rates (%)		Homestead exemptions ($)	
State	1990	1997	1990	1997
Alabama	3.65	3.12	5,000	5,000
Alaska	0	0	54,000	54,000
Arizona	6.51	4.8	100,000	100,000
Arkansas	7	7	No limit	No limit
California	9.3	9.78	7,500	15,000
Colorado	4.76	5.36	20,000	30,000
Connecticut	0	4.5	0	75,000
Delaware	7.7	6.9	0	0
Florida	0	0	No limit	No limit
Georgia	5.66	5.83	5,000	5,000
Hawaii	9	9	30,000	30,000
Idaho	8.2	8.2	30,000	50,000
Illinois	3	3	7,500	7,500
Indiana	3.4	3.4	7,500	7,500
Iowa	7.39	6.36	No limit	No limit
Kansas	5.15	6.45	No limit	No limit
Kentucky	4.39	6	5,000	5,000
Louisiana	4.14	3.75	15,000	15,000
Maine	8.5	8.5	7,500	12,500
Maryland	5	6	0	0
Massachusetts	5.95	5.95	100,000	100,000
Michigan	4.6	4.4	3,500	3,500
Minnesota	8	8.86	No limit	200,000
Mississippi	4.75	4.85	30,000	75,000
Missouri	4.39	6	8,000	8,000
Montana	8.59	6.83	40,000	40,000
Nebraska	6.4	7	10,000	10,000
Nevada	0	0	90,000	125,000
New Hampshire	0	0	5,000	30,000
New Jersey	3.5	6.37	0	0
New Mexico	7.83	8.4	20,000	30,000
New York	7.88	6.85	10,000	10,000
North Carolina	7	8.08	7,500	10,000
North Dakota	3.77	5.25	80,000	80,000
Ohio	6.9	7.2	5,000	5,000
Oklahoma	6.72	6.05	No limit	No limit
Oregon	8.12	9	15,000	25,000
Pennsylvania	2.1	2.8	0	0
Rhode Island	6.04	9.66	0	0
South Carolina	7	7.3	5,000	5,000
South Dakota	0	0	No limit	No limit
Tennessee	0	0	5,000	5,000
Texas	0	0	No limit	No limit
Utah	6.26	5.72	8,000	8,000
Vermont	6.54	8.85	30,000	30,000
Virginia	5.75	5.75	5,000	5,000
Washington	0	0	30,000	30,000
West Virginia	6.5	6.5	7,500	15,000
Wisconsin	6.93	6.93	40,000	40,000
Wyoming	0	0	10,000	10,000
Federal			7,500	15,000

allows the federal option, the maximum of the state and federal exemption levels. If this is greater than the average house price in the state, we use the average house price instead, which is a more accurate representation of the exemption that the average person would get. We then multiply this by the state's homeownership rate and, if the state allows married householders to double the exemption, we also multiply it by 1 plus the state's share of households in which both spouses reside together. The result of this divided by the average house price yields our homestead exemption rate.

Note that the sources for all of the data used to construct our variables are given in the data appendix, as are the summary statistics for all of the independent variables described above. We should also note that our two most important independent variables—the homestead exemption rate and the maximum marginal tax rate—are uncorrelated, with a correlation coefficient of –0.01.

As we mention above, one of the main benefits of our panel approach is that the relative abundance of observations means that we can easily allow for nonlinearities. This is important because, for each of our government policy variables, there are opposing effects, meaning that the relationships might be non-monotonic. This is easiest to see with tax rates, for which the standard negative labor-effort effect is countered by the positive tax-evasion effect. Assuming a non-trivial cost to being caught evading taxes, at low tax rates the incentive to evade taxes will not be terribly strong because the net expected benefits are not very high. Conversely, under very high tax rates, the benefit of evading taxes is much higher.

In preliminary analyses, we found that a cubic specification fits the homestead exemption rate well, whereas a quadratic specification fits the tax variable well. Thus, our baseline model, which we report and discuss in detail below, uses a quadratic tax variable and a cubic homestead exemption variable. In the section following our discussion of the baseline results, we discuss alternative specifications, the final of which justifies the cubic specification for the homestead exemption rate.

EMPIRICAL RESULTS

Our dependent variable is the rate of entrepreneurship, as defined above, for 1991-98, and our independent variables are all lagged by one year. To allow for the most general error structure given our data constraints, we estimate (1) using feasible generalized least squares (FGLS). This allows for state-specific heteroskedastic errors, although, because of a relatively short panel, we still need to assume that errors are uncorrelated across states (Beck and Katz, 1995). We also allow for each state's errors to follow their own AR(1) process.

Table 2 summarizes our results. As discussed above, we attach little importance to the coefficients on our demographic and business conditions variables, but simply note that omitting them would have a statistically significant effect on the results. More importantly, our estimation indicated that the marginal tax rate and the homestead exemption rate are both related non-monotonically to the rate of entrepreneurship.

Our estimates of the effects of marginal tax rates on entrepreneurship indicate that at tax rates at the low end of our observed rates—28 to 35 percent—an increase in the tax rate will reduce the number of entrepreneurs (see Figure 2). Beyond this range, higher marginal taxes will increase the number of entrepreneurs indirectly as, presumably, the tax-evasion incentives become large enough to begin outweighing the possible penalties.

The cubic relationship between the homestead exemption rate and entrepreneurship is illustrated by Figure 3. At very low and very high exemption rates—between 0 and 20 percent and above 60 percent—an increase in the homestead exemption leads to a decrease in the rate of entrepreneurship, suggesting that the credit-access effect dominates. At the mid-range of exemption rates—between 20 and 60 percent—an increase in the homestead exemption rate leads to an increase in the rate of entrepreneurship, suggesting that the wealth-insurance effect dominates. Note, though, that only rates between 50 and 72 percent lead to a higher rate of entrepreneurship than there would be with no homestead exemption at all.

The year dummies are also interesting and

Table 2

Baseline FGLS Results

Dependent variable:
state rate of entrepreneurship = (nonfarm proprietors' employment)/(working-age population)

	Coefficient	Standard error	*t*-Statistic
Policies			
Maximum marginal tax rate	–0.092	0.056	–1.66
Maximum marginal tax rate squared	1.3 e^{-3}	0.7 e^{-3}	1.75
Homestead exemption rate	–0.118	0.024	–4.93
Homestead exemption rate squared	0.004	0.001	4.77
Homestead exemption rate cubed	–3.3 e^{-5}	0.7 e^{-5}	–4.69
Demographics			
Adult share aged 45-65	0.173	0.054	3.22
Adult share aged 65+	0.034	0.078	0.44
Female share	0.080	0.020	4.06
Black share	–0.146	0.086	–1.70
Native American share	0.175	0.407	0.43
Asian and Pacific Islander share	–0.111	0.180	–0.62
Hispanic share	–0.067	0.066	–1.01
Business conditions			
Unemployment rate	0.106	0.025	4.26
Real per capita income	–1.1 e^{-4}	0.9 e^{-4}	–1.23
Real per capita wealth	0.310	0.229	1.35
Relative proprietor's wage	0.342	0.399	0.86
Industry shares	Yes	—	—
Year dummies			
1992	–0.221	0.055	–4.01
1993	–0.106	0.090	–1.18
1994	0.207	0.119	1.73
1995	0.606	0.152	3.98
1996	1.038	0.183	5.67
1997	1.153	0.219	5.25
1998	1.224	0.255	4.81
State fixed effects	Yes	—	—
Constant	–22.442	119.659	–0.19
Log-likelihood		–6.291	
Number of observations		400	
Estimated covariances		50	
Estimated autocorrelations		50	

NOTE: The estimation corrects for state-specific heteroskedasticity and autocorrelation. Omitted reference variables are as follows: adult share aged 18-44, white share of employment, government share of employment, and 1991.

Figure 2

Entrepreneurship and Marginal Taxes

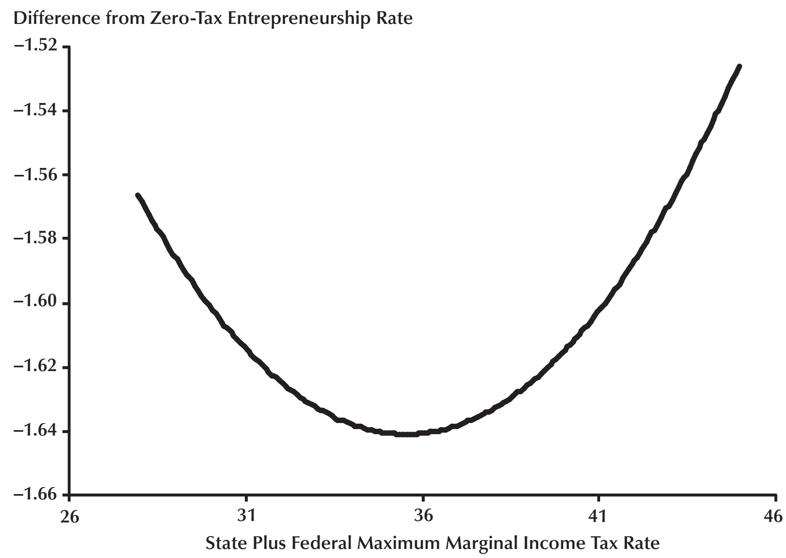


Figure 3

Entrepreneurship and the Homestead Exemption

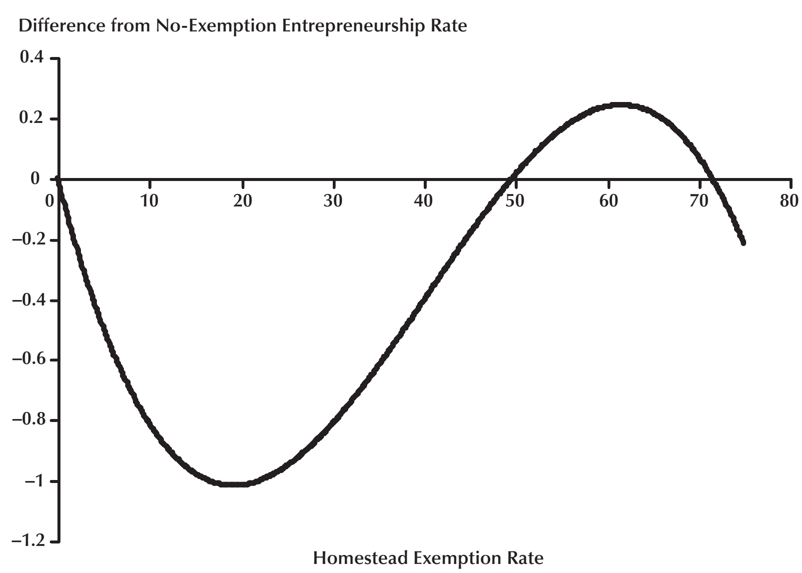
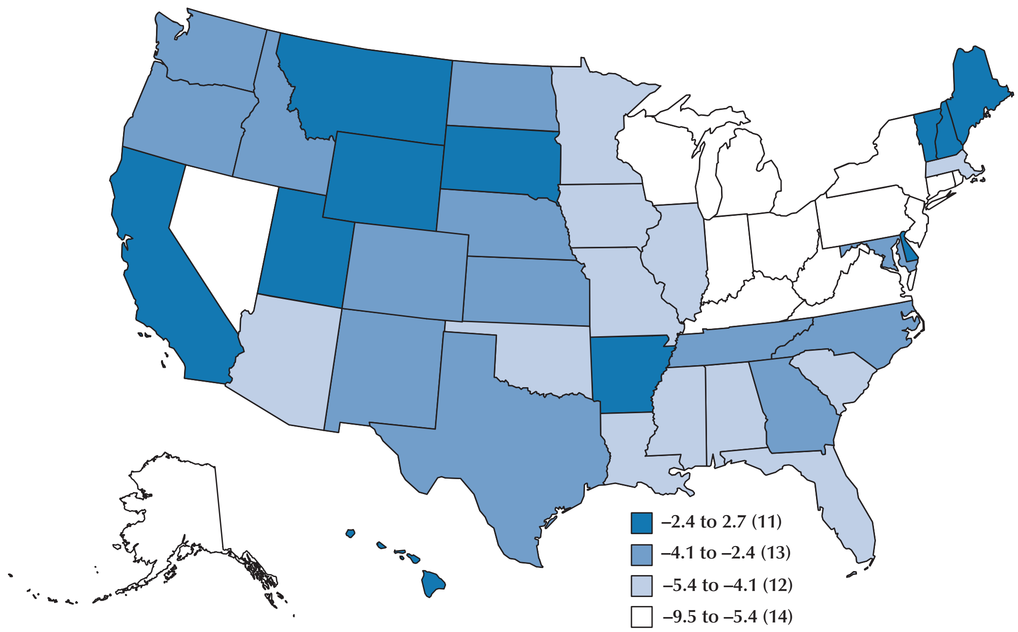


Figure 4

Estimated State Fixed Effects



suggest an underlying trend in entrepreneurship not captured by demographics, business conditions, or government policies. The estimated coefficient on the 1998 dummy indicates that state rates of entrepreneurship would have risen, on average, by 1.2 percentage points from 1991 to 1998 had all of the variables we include in our estimation remained at their initial levels.

Figure 4 plots the estimated fixed effects across the states, illustrating the extent to which differences in entrepreneurship are determined by differences in the variables included in our regression. Most noticeably, comparing Figures 1A and 4, we see that not all states with low levels of entrepreneurship also have low estimated fixed effects. In particular, states in the Great Lakes, Upper South, and Deep South regions have low levels of entrepreneurship, typically falling in the lowest quartile. However, the fixed effects for the Deep South states are not in the lowest quartile, while those for the Great Lakes and Upper South states are. This indicates that the relatively low levels of entrepreneurship in the Deep South are due to relatively inhospitable business conditions, demographic factors, or government policies. On the other hand, the low levels of entrepreneurship in the Great Lakes and Upper South are attributable to fixed factors, which Georgellis and Wall (2000a) suggest might include cultural, historical, or sociological factors that suppress entrepreneurship. At the other extreme are states in New England and the West, which have high levels of entrepreneurship and high estimated fixed effects. These conditions suggest that one of the reasons for the high levels of entrepreneurship is that these states contain the cultural, historical, and sociological makeup to pursue and succeed in entrepreneurship.

ALTERNATIVE ESTIMATES

Our baseline model uses specific functional forms for the policy variables and generalized least-squares estimation to allow for state-specific autocorrelation and cross-sectionally uncorrelated

Table 3

Alternative FGLS Results

Dependent variable:
state rate of entrepreneurship = (nonfarm proprietors' employment)/(working-age population)

	I	II	III	IV	V	VI
Maximum marginal tax rate	0.008*	–0.096*	0.066	–0.134*	–0.129*	–0.119*
	(0.004)	(0.055)	(0.084)	(0.065)	(0.065)	(0.053)
Maximum marginal tax rate squared	—	1.4 e^{-3}*	0.001	0.002*	0.002*	0.002*
		(0.7 e^{-3})	(0.001)	(0.001)	(0.001)	(0.001)
Homestead exemption rate	–0.003	–0.010	–0.158*	–0.116*	–0.115*	—
	(0.004)	(0.009)	(0.029)	(0.023)	(0.023)	
Homestead exemption rate squared	—	9.8 e^{-5}	0.006*	0.004*	0.004*	—
		(9.9 e^{-5})	(0.001)	(0.001)	(0.001)	
Homestead exemption rate cubed	—	—	–4.9 e^{-5}*	–3.2 e^{-5}*	–3.2 e^{-5}*	—
			(0.9 e^{-5})	(0.7 e^{-5})	(0.7 e^{-5})	
Second octile of homestead exemption rate	—	—	—	—	—	–0.285*
						(0.087)
Third octile of homestead exemption rate	—	—	—	—	—	–0.391*
						(0.095)
Fourth octile of homestead exemption rate	—	—	—	—	—	–0.477*
						(0.199)
Fifth octile of homestead exemption rate	—	—	—	—	—	–0.445*
						(0.126)
Sixth octile of homestead exemption rate	—	—	—	—	—	0.146
						(0.165)
Seventh octile of homestead exemption rate	—	—	—	—	—	0.382*
						(0.233)
Eighth octile of homestead exemption rate	—	—	—	—	—	0.259*
						(0.232)
Demographics, business conditions, year and state effects	Yes	Yes	Yes	Yes	Yes	Yes
Heteroskedasticity	Yes	Yes	No	Yes	Yes	Yes
Autocorrelation	State	State	State	None	Common	State
Log-likelihood	–7.56	–4.49	–114.92	–26.55	–26.46	0.514

NOTE: Standard errors are in parentheses; * indicates statistical significance at the 10 percent level.
Alternative I: baseline model with restriction that higher-order effects of policy variables are zero.
Alternative II: baseline model with restriction that third-order effect of homestead exemption is zero.
Alternative III: baseline model with assumption that errors are homoskedastic.
Alternative IV: baseline model with assumption that errors are not autocorrelated.
Alternative V: baseline model with assumption that autocorrelation is common across states.
Alternative VI: baseline model with home exemption rate octiles and state-specific heteroskedasticity and autocorrelation.

heteroskedasticity. To check the consequence of these choices on our estimation of the effects of our policy variables, we present the results of six alternatives.[8] These alternative results, which either use a different specification of the policy variables or place stronger restrictions on the error terms, are reported in Table 3 and illustrated by Figures 5 and 6.

Alternative I restricts the coefficients on the squared and cubed terms of the policy variables to zero. Estimation under these restrictions yields a positive and statistically significant effect for the marginal tax rate on entrepreneurship and a negative but statistically insignificant effect for the homestead exemption rate. Alternative II restricts the coefficient on the cubed term of the homestead exemption rate to zero while using the same quadratic functional form for the marginal tax rate as in the baseline model. The estimated relationship between the maximum marginal tax rate and entrepreneurship under this restriction differs very little from the baseline results. On the other hand, as previously stated, the estimated coefficients on the homestead exemption rate are both statistically no different from zero. The results from these two alternative specifications indicate that the choices we have made about the specification of the policy variables are important for our conclusions. Likelihood ratio tests reject the null hypotheses that the restrictions that these alternatives place on the higher-order terms do not have a statistically significant effect on the estimation. Therefore, the least-restrictive baseline model is preferred statistically to the two alternatives.

Three other alternatives place stronger restrictions on the error terms than does the baseline model: In alternative III they are assumed to be homoskedastic, in alternative IV they are not autocorrelated, and in alternative V their autocorrelation is common across states. As Figure 5 illustrates, none of these restrictions has an effect on the estimated U-shape for the relationship between marginal tax rates and the rate of entrepreneurship, although the coefficients in alternative III are not statistically significant. The important differences are that the estimated relationship is flatter with alternative III and steeper with alternatives IV and V.

For the relationship between the homestead exemption rate and the rate of entrepreneurship, only the estimates from alternative III differ in any non-trivial way. All three alternatives yield an S-shaped relationship, although the estimated relationship is everywhere steeper with alternative III than with the baseline model. Another important difference is that alternative III suggests that all homestead exemption rates above 42 percent will yield more entrepreneurship than would a zero exemption, whereas the baseline model suggests that this is true only for homestead exemption rates between 50 and 72 percent.

Alternative VI replaces the continuous homestead exemption variables with dummy variables for discrete ranges of the homestead exemption rate. Because this model removes any general assumption regarding functional form, it allows us to verify the general shape of the cubic relationship of our baseline model. We split the observed homestead exemption rates into octiles, each with 50 observations, and estimate the model with the first octile omitted to avoid perfect collinearity. As summarized by Table 3, for all but one of the octiles of the homestead exemption rate, the rate of entrepreneurship is statistically different from what it would be under the first octile. Further, as illustrated by Figure 6, these results confirm the general S-shape to the relationship between the homestead exemption rate and the rate of entrepreneurship. Note also that this specification has little effect on the estimated relationship between the rate of entrepreneurship and the maximum marginal tax rate.

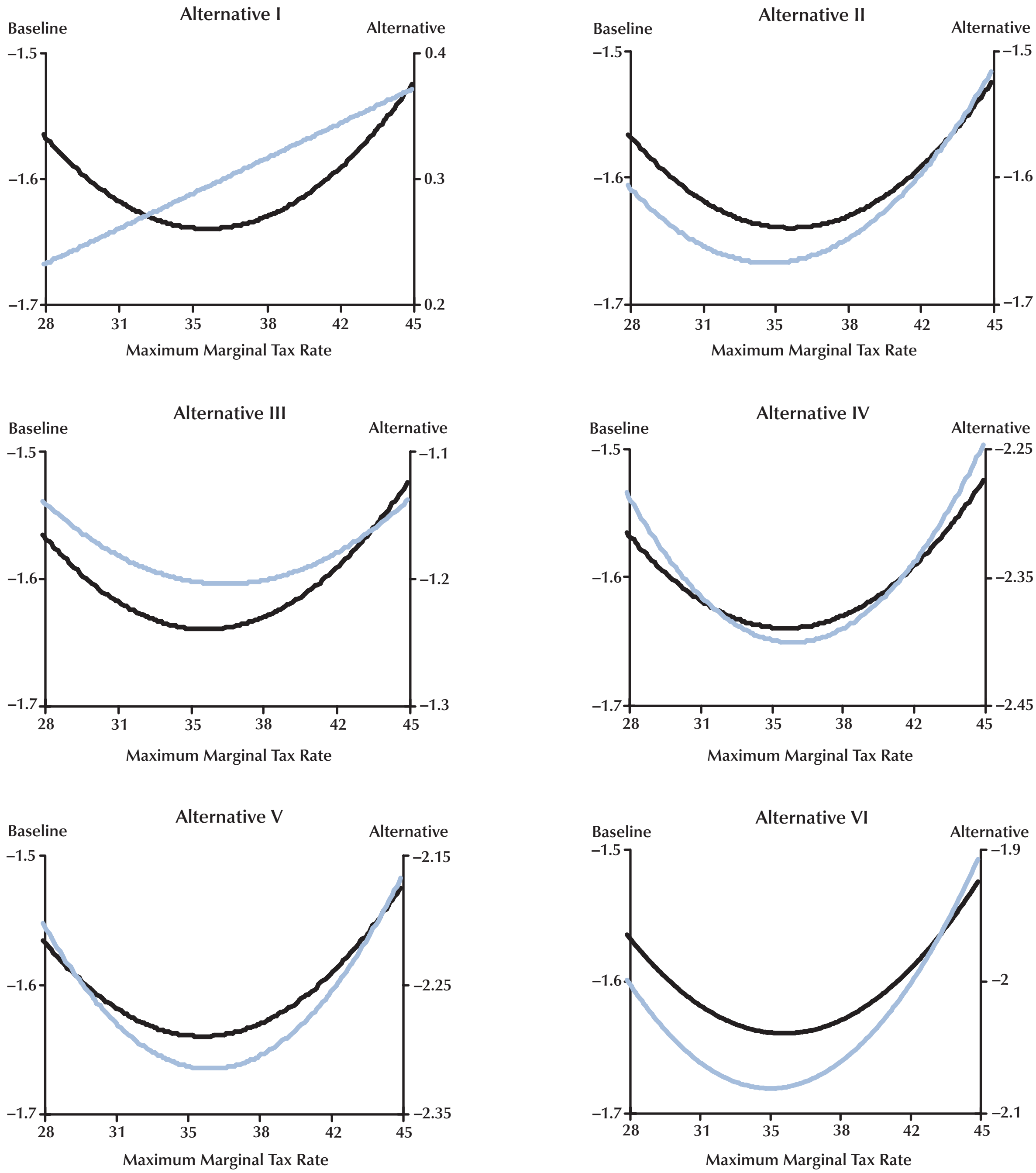
CONCLUDING REMARKS

This paper uses the panel approach of Georgellis and Wall (2000a) to estimate the effects of personal income tax rates and bankruptcy exemptions on entrepreneurship. Using data for all 50 states of the United States for 1991-98, we find non-monotonic relationships. Specifically, at low initial tax levels, an increase in marginal

8 Wall (2004) demonstrates how not allowing for autocorrelation and heteroskedasticity, in particular, has severe consequences for the state-level panel of entrepreneurship in Black and Strahan (2002).

Figure 5

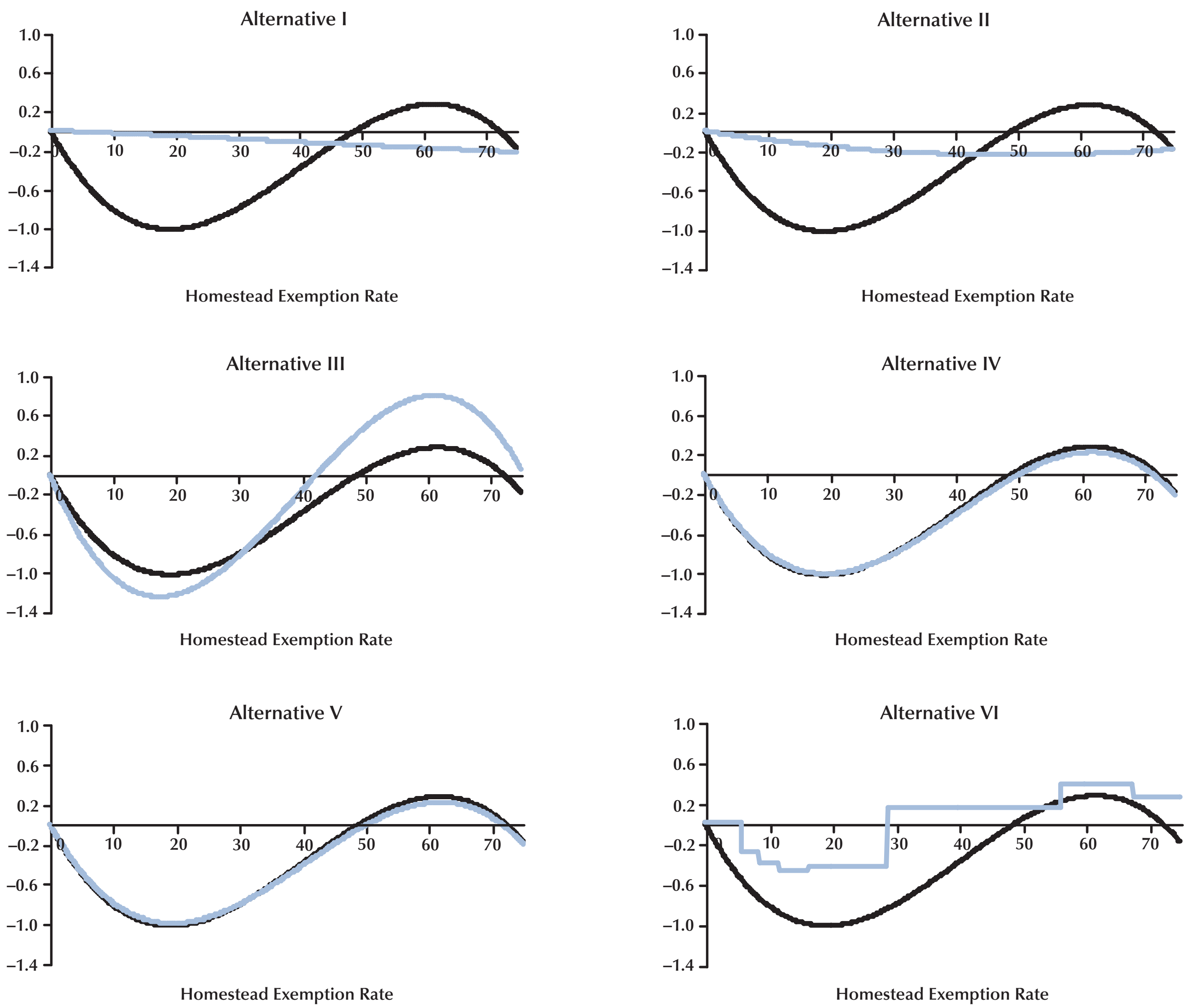
Alternative Estimates: Maximum Marginal Tax Rate



NOTE: Alternative I: baseline model with restriction that higher-order effects of policy variables are zero; Alternative II: baseline model with restriction that third-order effect of homestead exemption is zero; Alternative III: baseline model with assumption that errors are homoskedastic; Alternative IV: baseline model with assumption that errors are not autocorrelated; Alternative V: baseline model with assumption that autocorrelation is common across states; Alternative VI: baseline model with home exemption rate octiles and state-specific heteroskedasticity and autocorrelation.

Figure 6

Alternative Estimates: Homestead Exemption Rate



NOTE: Alternative I: baseline model with restriction that higher-order effects of policy variables are zero; Alternative II: baseline model with restriction that third-order effect of homestead exemption is zero; Alternative III: baseline model with assumption that errors are homoskedastic; Alternative IV: baseline model with assumption that errors are not autocorrelated; Alternative V: baseline model with assumption that autocorrelation is common across states; Alternative VI: baseline model with home exemption rate octiles and state-specific heteroskedasticity and autocorrelation.

tax rates reduces the number of entrepreneurs, although at higher initial tax levels it will do the opposite. We also find that at very low and very high initial levels, an increase in the homestead exemption will reduce the number of entrepreneurs. In the mid-range of homestead exemption rates, there is a positive relationship between the exemption level and entrepreneurship. Further, only for relatively high homestead exemption rates will the level of entrepreneurship be higher than if there were no homestead exemption at all.

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DATA APPENDIX

Data series	Source
Nonfarm proprietors' employment	Regional Economic Information System, Bureau of Economic Analysis, Table CA25
Unemployment rate	Bureau of Labor Statistics
Dividends, interest, and rent	Regional Economic Information System, Bureau of Economic Analysis, Table CA05
Per capita gross state product	Bureau of Economic Analysis
Average nonfarm proprietors' income; average wage and salary disbursements	Regional Economic Information System, Bureau of Economic Analysis, Table CA30
Industry employment shares	Establishment Survey, Bureau of Labor Statistics
Age, race, and sex employment shares	Bureau of Labor Statistics
Maximum marginal tax rates	TAXSIM, National Bureau of Economic Research
Homestead bankruptcy exemptions	Elias, Renaur, and Leonard, *How to File for Chapter 11 Bankruptcy*, various editions
Median house price	Derived using median house price from 1990 Census and the Home Price Index from the Office of Federal Housing Enterprise Oversight
Home ownership rate	Bureau of the Census
Share of households with householder and spouse	Bureau of the Census, derived from 1990 and 2000 Census assuming constant state-level rates of change

Table A1

Summary Statistics

	Mean	Standard deviation
Rate of entrepreneurship	14.51	2.90
Maximum marginal tax rate	38.37	4.14
Homestead exemption rate	28.71	24.75
Adult share aged 45-65	26.52	1.56
Adult share aged 65+	17.12	2.56
Female share of employment	46.07	1.32
Black share of employment	9.90	9.34
Native American share of employment	1.66	2.94
Asian and Pacific Islander share of employment	3.11	8.73
Hispanic share of employment	5.91	7.87
Unemployment rate	5.72	1.49
Real per capita income	$20,862	$3,746
Real per capita wealth	4.08	0.83
Relative proprietor's wage	0.74	0.11

Human Capital Growth in a Cross Section of U.S. Metropolitan Areas

Christopher H. Wheeler

Growth of human capital, defined as the change in the fraction of a metropolitan area's labor force with a bachelor's degree, is typically viewed as generating a number of desirable outcomes, including economic growth. Yet, in spite of its importance, few empirical studies have explored why some economies accumulate more human capital than others. This paper attempts to do so using a sample of more than 200 metropolitan areas in the United States over the years 1980, 1990, and 2000. The results reveal two consistently significant correlates of human capital growth: population and the existing stock of college-educated labor. Given that population growth and human capital growth are both positively associated with education, these results suggest that the geographic distributions of population and human capital should have become more concentrated in recent decades. That is, larger, more-educated metropolitan areas should have exhibited the fastest rates of increase in both population and education and thus "pulled away" from smaller, less-educated metropolitan areas. The evidence largely supports this conclusion.

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Human capital is now commonly held to be one of the fundamental drivers of economic growth. To be sure, the notion that the skills possessed by an economy's workforce promote technological advancement and productivity growth is an intuitively appealing one. Yet, there is also a fair amount of empirical evidence that supports this notion. In particular, a sizable literature in the past two decades has established a strong statistical association between human capital (usually captured by educational attainment) and the growth of employment, productivity, and income. Moreover, this relationship holds with striking regularity at different levels of geographic aggregation, including countries (Barro, 1991), U.S. states (Barro and Sala-i-Martin, 1992), and cities and metropolitan areas (Glaeser, Scheinkman, and Shleifer, 1995; Glaeser and Saiz, 2003; and Simon and Nardinelli, 2002).

Economic growth, however, is only one benefit that has been associated with human capital. A variety of studies also suggest that greater educational attainment within local economies (e.g., states or cities) may tend to be accompanied by lower rates of crime (Lochner and Moretti, 2004), greater civic involvement (Dee, 2004; Milligan, Moretti, and Oreopoulos, 2004), and less political corruption (Glaeser and Saks, 2004). Clearly, because these are desirable outcomes, identifying the determinants of human capital growth is a worthwhile undertaking. Unfortunately, while a host of theoretical models have done so,[1] surprisingly little empirical research has followed suit. Most existing studies have focused on what human capital produces rather than why some economies accumulate more of

[1] See Barro and Sala-i-Martin (1995) for a survey of human capital–based models of growth.

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it than others.[2] As such, our understanding of human capital accumulation remains limited.

This paper looks at the growth of human capital in a sample of more than 200 U.S. metropolitan areas identified in the decennial U.S. Census over the years 1980, 1990, and 2000. Defining human capital accumulation as the change in the fraction of a metropolitan area's employed labor force with a bachelor's degree or more, I find that metropolitan areas with larger populations and higher fractions of their workers with a bachelor's degree tend to accumulate human capital at faster rates than less-populous, less-educated metropolitan areas. The results suggest that a 1-standard-deviation increase in either total resident population or the fraction of workers with a four-year college degree (in the cross section of metropolitan areas) tends to be associated with a 0.4- to 0.7-percentage-point rise in the share of college graduates in the workforce over the next decade. These estimated magnitudes, it should be noted, are *not* meant to be interpreted as causal, but simply to quantify the strength of the observed associations between these two variables and the accumulation of highly educated workers. Although some evidence suggests that certain measures of industrial composition and observable city-level amenities (e.g., restaurants and universities) are also associated with changes in the college fraction, none are as robustly correlated as population and the existing level of human capital.

These findings are intriguing, as they seem to suggest that the geographic distribution of human capital across the cities of the United States should have grown more concentrated (or unequal) between 1980 and 2000. After all, because human capital accumulation tends to be positively associated with the current level of human capital, the gap between initially high-education cities and low-education cities ought to have widened in recent decades. The evidence strongly supports this conclusion. Various measures that characterize the degree of spread in the distribution of metropolitan area–level college attainment show rising dispersion between 1980 and 2000.

In addition, because previous research has established a positive link between population growth and education (e.g., Glaeser, Scheinkman, and Shleifer, 1995), one would expect to find a similar pattern of "divergence" in population levels across U.S. metropolitan areas in recent decades. That is, if more-populous cities accumulate highly educated workers more quickly than less-populous ones, then they should also gain population faster too. Rising educational attainment fuels population growth, which, in turn, spurs human capital accumulation and so on. This conclusion is also largely borne out in the data. The distribution of the logarithm of population became more concentrated within particularly large metropolitan areas between 1980 and 2000.

Although one might surmise that rising concentrations of population and education in the largest and most-educated cities have also led to a greater concentration of income, the evidence on this issue is somewhat mixed. In particular, while the data show that the distribution of metropolitan area–level average log hourly wages grew wider between 1980 and 1990, they also show that it narrowed slightly between 1990 and 2000. Growing concentrations of population and college-educated workers in the metropolitan areas of greatest size and abundance of human capital, then, have not been accompanied by substantial increases in the degree of inter-city (average) earnings inequality.

DATA

The data used in the analysis are taken primarily from the 5 percent public use samples of the 1980, 1990, and 2000 U.S. Census as reported by the Integrated Public Use Microdata Series (Ruggles et al., 2004). These data files include a variety of personal characteristics, including age, education, and earnings, for samples of more

[2] There are two notable exceptions: Moretti (2004) offers a short analysis of the determinants of changing college attainment rates among U.S. metro areas, similar to what I do here. Glaeser and Saiz (2003) examine whether educational attainment responds to economic growth. With both of these papers, however, the primary issue under consideration is *not* the determinants of human capital growth. Consequently, their analyses are much more cursory with respect to this issue than my analysis here.

than 11 million individuals in each year, as well as information about each individual's place of residence. These data are used to construct a time series of metro area–level characteristics, including human capital.

In principle, "human capital" could be defined in many different ways: e.g., time spent on a particular job, time spent working on all jobs, numbers of different jobs held, educational attainment, some measure of "innate" ability or productivity. This paper takes a standard approach by using educational attainment, which can be justified by noting that (i) schooling has been shown to have a significant causal influence on individual productivity, at least as quantified by earnings (Card, 1999), and (ii) it tends to be strongly correlated with a variety of outcomes commonly theorized to be tied to "human capital," including economic growth. For these reasons, education is treated as a suitable metric for human capital. More specifically, I use the fraction of a metro area's employed labor force with a bachelor's degree or more because previous work on economic growth and education externalities in cities has found this particular quantity to capture variation in educational attainment reasonably well.[3]

Formally, metro areas in the analysis represent either metropolitan statistical areas (MSAs), New England county metropolitan areas (NECMAs), or consolidated metropolitan statistical areas (CMSAs) in the event that an MSA or NECMA belongs to a CMSA.[4] A total of 210 of these local markets are identified in the 1980 data, 206 in 1990, and 245 in 2000. Only 188 appear in all three Census years.

Additional characteristics describing metro areas are derived from the USA Counties CD-ROM (U.S. Bureau of the Census, 1999) and from County Business Patterns (CBP) files for the years 1980, 1990, and 2000. The former dataset provides information about county-level population and land area, which is used to generate population and population density figures at the metro-area level.[5] The latter reports the numbers of various types of private sector establishments (e.g., restaurants and bars), which are used to characterize the amenity value of a metro area. Further details about the data appear in the appendix.

EMPIRICAL FINDINGS

Human Capital and Urban Agglomeration

Within the United States, human capital has typically been concentrated in metro areas. Among workers in the Census samples used here, 86.1 percent of all college graduates resided in a metro area in 1980. By 2000, this figure had risen to 89.9 percent. In contrast, approximately 78 percent of workers with only a high school diploma were metro dwellers in either year.

Why are highly educated workers drawn to cities? Numerous characteristics, of course, distinguish metro areas from non-metro areas and, thus, could offer some semblance of an answer. Besides larger and better-educated populations, urban agglomerations also tend to possess greater numbers of industries that highly educated workers may find particularly appropriate or appealing given their skills (e.g., professional and technical services). Metro areas also tend to offer a greater array of amenities (e.g., restaurants and museums), which may serve to attract and maintain a pool of highly educated labor (see Glaeser, Kolko, and Saiz, 2001).

Economically, the estimated returns to education do tend to be particularly high in metro areas. Consider, for instance, the results from a regression of log hourly earnings on five educational attainment indicators (no high school, some high school, a high school diploma only, some college or an associate's degree, a bachelor's degree or more), eight indicators representing years of potential work experience,[6] a metro residence dummy,

[3] See, for example, Black and Henderson (1999) and Moretti (2004).

[4] Throughout the paper, I use the terms "metropolitan area" and "city" interchangeably for expositional purposes. In all cases, "local markets" refers to MSAs, NECMAs, or CMSAs.

[5] County-level population data for the year 2000 are derived from the population estimates program of the U.S. Census Bureau at www.census.gov/popest/estimates.php. In all years, land area from 1990 is used to compute density.

[6] These indicators represent 6-10 years of experience, 11-15 years, 16-20 years, 21-25 years, 26-30 years, 31-35 years, 36-40 years, and 41 or more years.

and interactions between metro residence and each of the education and experience variables.[7] To keep the analysis simple, I have limited the sample of workers used for this regression to white males between the ages of 18 and 65. I have also performed the estimation separately for the 1980 and 2000 samples to account for any changes in the coefficient values over time.[8]

The resulting coefficient estimates, which for the sake of conciseness have been limited to the education variables, appear in Table 1. The raw coefficients on the five educational attainment dummies in the first five rows of results can be interpreted as the average log wages (conditional on all of the other covariates in the model) for workers in these education groups who reside *outside* of a metro area. The average log wages for workers inside metro areas is then given by the sum of these raw coefficients and the corresponding interaction listed in the remaining rows of the table.

With this interpretation in mind, it is evident that, although college graduates earn more than workers with less schooling, the premium associated with a college degree is particularly high within metro areas. In non-urban areas in 1980, for example, college-educated workers earned approximately 30 percent more than workers with only a high school diploma.[9] Within metro areas, that differential was 45 percent. By the year 2000, the college premium had risen to 49 percent outside of metro areas, 75 percent within them. In terms of raw (conditional) wage levels, college graduates earned an average of \$10.48 per hour outside of metro areas in 1980, \$12.26 within them.[10] By 2000, these figures stood at, respectively, \$10.80 and \$13.40, implying a 20-year growth rate of roughly 3 percent in rural areas, but 9.3 percent in urban areas.

These figures, of course, should not be interpreted causally. That is, a highly educated worker's metropolitan status does not necessarily *cause* him to earn more than if he were situated in a smaller labor market. On the contrary, the results may reflect, at least in part, a selection mechanism by which the most productive, highly educated workers have chosen to live in cities. Still, these results seem to suggest that there are strong economic incentives for highly educated workers to reside in urban areas.

To gain a better sense of which factors (e.g., metro area size, existing human capital, education premia, industrial composition) may underlie human capital accumulation, I now turn to the analysis of a cross section of metro areas. The underlying goal is to exploit the variation exhibited across cities with respect to their education, size, and other characteristics to draw inferences about which features are most strongly associated with the growth of human capital.

Correlates of Human Capital Accumulation: Baseline Results

As noted previously, the Census data used in this article identify more than 200 metro areas in each of the three years (1980, 1990, 2000) considered. Using this sample, I estimate the following simple regression in which the change in metro area *i*'s college fraction during decade *t*, $\Delta Coll_{i,t}$, is specified as

$$\Delta Coll_{i,t} = \mu + \delta_t + \beta X_{i,t} + \varepsilon_{i,t}, \quad (1)$$

where μ is a constant, δ_t is a decade-specific fixed effect, $X_{i,t}$ is a set of characteristics describing the metro area at the *beginning* of the decade, and $\varepsilon_{i,t}$ is a stochastic element, assumed to be uncorrelated across metro areas but potentially correlated within them (i.e., $\varepsilon_{i,t}$ and $\varepsilon_{i,s}$ may show some nonzero association). This equation is meant to be analogous to those used in empirical studies of economic growth in which a measure of growth is regressed on a set of initial characteristics (e.g., Barro, 1991, and Glaeser, Sheinkman, and Shleifer, 1995).

7 The regressions also include dummies for marital status, disability status, veteran status, and foreign-born status.

8 The 5 percent sample for 1990 does not report metropolitan status for all individuals in the sample. Hence, estimating the regression for this year is not possible.

9 Percentages are derived from the estimates in Table 1 by exponentiating the log wage differential and subtracting 1. A 26-log-point differential between college and high school graduates in non-metro areas in 1980, for example, corresponds to roughly 30 percent.

10 These estimates are based on exponentiating the coefficients in Table 1.

Table 1

Education Premia by Metropolitan Status

Variable	1980	2000
No high school	1.84 (0.004)	1.73 (0.006)
Some high school	1.96 (0.003)	1.81 (0.005)
High school	2.09 (0.003)	1.98 (0.004)
Some college	2.15 (0.003)	2.11 (0.004)
College or more	2.35 (0.003)	2.38 (0.004)
No high school–metro	0.013 (0.004)	–0.014 (0.007)
Some high school–metro	0.031 (0.004)	0.035 (0.005)
High school–metro	0.046 (0.003)	0.053 (0.004)
Some college–metro	0.081 (0.004)	0.107 (0.004)
College–metro	0.156 (0.004)	0.215 (0.004)

NOTE: Coefficients are from regressions of log hourly wages on education indicators and their interactions with a metropolitan status dummy; 1,850,727 observations for the year 1980; 2,135,811 observations for the year 2000; standard errors appear in parentheses.

Among the characteristics considered in the vector $X_{i,t}$ are the following: (i) an estimate of a metro area's return to a college degree,[11] (ii) its level of human capital (given by the fraction of college-educated workers in the labor force), (iii) its raw size (given by the logarithms of population and population density), and (iv) its broad industrial composition (measured by shares of total employment accounted for by each of 20 industries). Summary statistics for each of these regressors appear in Table 2.[12]

Results are given in Table 3. The first column, labeled *I*, reports the resulting coefficients when each covariate is entered into the regression separately. In all instances, estimation of equation (1) also includes a set of three region dummies to account for any exogenous differences in the rate of human capital accumulation in different parts of the country.[13]

Based on the estimates, many of these regressors do turn out to be significantly associated with the growth of the college fraction, at least in a simple, univariate sense. Metro areas with initially larger populations, higher levels of population density, and larger fractions of workers with a bachelor's degree or more all see their college attainment rates rise by more over the following decade than smaller, less-dense, less-educated metro areas. In addition, greater fractions of employment accounted for by industries such as agriculture, mining, and manufacturing (either durable or nondurable) tend to correlate negatively with human capital accumulation, whereas a strong presence of industries such as finance, insurance, real estate, and business and repair services are positively associated with the change in the college attainment rate. Given that the former set of industries tends to employ fewer highly educated workers than the latter set of industries (see Table 4), these associations are rather intuitive. The estimated city-specific return to a college degree, while positive, is not statistically important. Greater discussion of this last regressor is provided below.

[11] Metro-area college degree returns are derived from city-year–specific regressions of log hourly wages on five education indicators, eight experience indicators, and dummies for marital status, disability status, veteran status, and foreign-born status. The coefficient on the college completion dummy is used to estimate the return to a college degree.

[12] Because they are easier to interpret, Table 2 lists summary statistics for population and population density levels rather than logarithms. In the regression analysis, I use these variables in log form, which is reasonably standard in the empirical literature on cities.

[13] A list of the state-level composition of the four U.S. Census regions appears in the appendix.

Table 2

Metropolitan Area Summary Statistics

Variable	Mean	Standard deviation	Minimum	Maximum
Estimated return	2.47	0.115	2.05	2.84
Population	888,590.6	1,912,186	100,376	19,397,717
Density	578.3	1,178.4	6.01	16,258.1
College fraction	0.218	0.065	0.09	0.455
Fraction agriculture, forestry, fisheries	0.007	0.006	0.001	0.063
Fraction mining	0.006	0.016	0	0.148
Fraction construction	0.068	0.017	0.033	0.19
Fraction nondurable manufacturing	0.077	0.047	0.014	0.365
Fraction durable manufacturing	0.123	0.072	0.009	0.453
Fraction transportation	0.045	0.014	0.018	0.152
Fraction communications	0.015	0.006	0.004	0.052
Fraction utilities	0.015	0.007	0.003	0.075
Fraction wholesale trade	0.044	0.013	0.015	0.126
Fraction retail trade	0.163	0.022	0.096	0.24
Fraction finance, insurance, real estate	0.061	0.021	0.027	0.24
Fraction business and repair services	0.051	0.018	0.016	0.149
Fraction private household services	0.005	0.003	0	0.02
Fraction personal services	0.024	0.015	0.012	0.23
Fraction entertainment and recreation services	0.012	0.012	0.003	0.139
Fraction medical services	0.091	0.024	0.04	0.292
Fraction educational services	0.105	0.031	0.052	0.27
Fraction social services	0.013	0.004	0.005	0.034
Fraction other professional services	0.02	0.009	0.004	0.088
Fraction public administration	0.055	0.031	0.015	0.255

NOTE: Summary statistics are taken over 661 city-year observations.

The next two columns of results, *II* and *III*, report the coefficients from two different specifications of (1) in which various combinations of these covariates appear. The longer of these (*III*) suggests that, unlike what is reported above, very few of the initial industry shares are significantly associated with human capital accumulation. Indeed, comparing the results from columns *I* and *III*, only one industry share enters significantly in both cases: finance, insurance, real estate. Industrial composition, therefore, seems largely unimportant for explaining the growth of human capital, at least once we have conditioned on initial education, size, and returns.

Among the remaining covariates, only two show consistently positive and significant associations with human capital accumulation: log population and the initial college fraction. Both of these regressors produce significant coefficients in all three reported specifications. Log density, by contrast, becomes insignificant when industry shares are included, and the initial return to a college degree enters negatively (and significantly) in specifications *II* and *III*. This latter result may simply reflect the inverse association between various measures of urban growth (e.g., population and average earnings) and initial wages, which is a common finding in the urban economics lit-

Table 3

Human Capital Accumulation Regression Results

Variable (initial value)	*I*	*II*	*III*
Estimated return	0.018 (0.011)	–0.02* (0.006)	–0.03* (0.014)
Log population	0.006* (0.001)	0.003* (0.001)	0.004* (0.002)
Log density	0.007* (0.001)	0.003* (0.001)	0.002 (0.002)
College fraction	0.16* (0.015)	0.12* (0.02)	0.11* (0.04)
Fraction agriculture, forestry, fisheries	–0.38* (0.13)	—	0.003 (0.17)
Fraction mining	–0.13* (0.03)	—	–0.01 (0.06)
Fraction construction	–0.002 (0.07)	—	0.18* (0.09)
Fraction nondurable manufacturing	–0.06* (0.02)	—	0.02 (0.04)
Fraction durable manufacturing	–0.03* (0.017)	—	0.03 (0.04)
Fraction transportation	0.07 (0.08)	—	0.01 (0.08)
Fraction communications	0.94* (0.19)	—	0.02 (0.23)
Fraction utilities	–0.29* (0.15)	—	–0.08 (0.15)
Fraction wholesale trade	0.01 (0.09)	—	–0.06 (0.1)
Fraction retail trade	–0.13* (0.06)	—	–0.01 (0.05)
Fraction finance, insurance, real estate	0.36* (0.06)	—	0.19* (0.07)
Fraction business and repair services	0.36* (0.11)	—	–0.15 (0.1)
Fraction private household services	–0.56 (0.42)	—	–0.3 (0.4)
Fraction personal services	0.02 (0.05)	—	0.06 (0.08)
Fraction entertainment and recreation services	0.04 (0.08)	—	0.004 (0.1)
Fraction medical services	0.03 (0.05)	—	0.03 (0.06)
Fraction educational services	–0.0005 (0.05)	—	–0.04 (0.06)
Fraction social services	0.76* (0.43)	—	0.25 (0.4)
Fraction other professional services	0.83* (0.24)	—	0.1 (0.2)
Fraction public administration	0.09* (0.03)	—	0.04 (0.05)

NOTE: The dependent variable is the change in college fraction for 1980-90 and 1990-2000. Region indicators and a dummy for the 1980-90 decade appear in all regressions. Column *I* reports coefficients from separate regressions for each regressor. Columns *II* and *III* report coefficients from regressions that include all regressors for which estimates are reported. Heteroskedasticity-consistent standard errors, adjusted for correlation within metro areas, appear in parentheses; * denotes significance at the 10 percent level or better.

erature (e.g., Glaeser, Scheinkman, and Shleifer, 1995). Higher returns to a college degree, not surprisingly, tend to be associated with higher average wages overall in these data. As growth slows, human capital accumulation tends to slow as well.[14]

How significant are the estimated associations between, on the one hand, initial log population and the college completion rate and, on the other, the subsequent change in the college completion fraction? Based on the point estimates from the longest specification in Table 3, a 1-standard-deviation increase in log population (in the cross section) corresponds to a 0.43-percentage-point rise in the college attainment rate over the next decade. A 1-standard-deviation increase in the

[14] The positive coefficient on the initial estimated college return in specification *I* may therefore emanate from omitted-variable bias. As shown previously, returns to a college degree tend to be higher in metro areas, suggesting a positive association with population and the college attainment rate. Not including these two variables in specification *I* may therefore bias a truly negative coefficient on initial returns upward.

Table 4

College Attainment by Major Industry

Industry	1980	1990	2000
Agriculture, forestry, fisheries	0.154	0.16	0.154
Mining	0.138	0.179	0.141
Construction	0.072	0.094	0.089
Nondurable manufacturing	0.112	0.153	0.196
Durable manufacturing	0.111	0.158	0.183
Transportation	0.09	0.123	0.144
Communications	0.146	0.231	0.328
Utilities	0.123	0.181	0.194
Wholesale trade	0.152	0.199	0.212
Retail trade	0.092	0.116	0.136
Finance, insurance, real estate	0.227	0.306	0.364
Business and repair services	0.2	0.255	0.33
Private household services	0.033	0.052	0.068
Personal services	0.067	0.105	0.12
Entertainment and recreation services	0.194	0.226	0.259
Medical services	0.219	0.289	0.33
Educational services	0.546	0.55	0.562
Social services	0.359	0.41	0.467
Other professional services	0.467	0.53	0.537
Public administration	0.252	0.298	0.352

NOTE: Fractions of each industry's total employment with a bachelor's degree or higher.

initial fraction of workers with a bachelor's degree or more has a somewhat larger implied association: a 0.72-percentage-point rise in the college attainment rate over the next 10 years.[15] Although they may seem small compared with average college completion rates near 22 percent for the metro areas in the sample, these magnitudes are far from negligible. In particular, they represent between 20 and 34 percent of the cross-sectional standard deviation of the 10-year change in the college fraction in these data, which is approximately 2.1 percentage points.

[15] The cross-sectional standard deviations for log population and the college completion rate are roughly 1.08 and 0.065. In terms of population levels, 1 standard deviation corresponds to roughly 680,000 residents.

Robustness

In this section, I consider a few simple alterations to the statistical analysis to assess the robustness of the results. The first seeks to account for the influence of certain amenities (e.g., restaurants, theaters, museums) on human capital accumulation. As noted previously, Glaeser, Kolko, and Saiz (2001) have demonstrated that cities have significant consumption aspects that seem to influence the willingness of individuals to live in dense urban environments. If the highly educated have an especially strong preference for these characteristics, amenities may play an important part in human capital accumulation that the analysis above misses. Indeed, it may not be a city's population or initial level of educational attainment that are important for explaining the growth of a city's college share, but its array of urban amenities. Population or education may simply be proxies for these types of characteristics. To explore this possibility, I consider the influence of the following eight amenities: eating and drinking establishments; movie theaters; elementary and secondary schools; live entertainment venues; museums, botanical gardens, and zoos; colleges and universities; hospitals; and commercial sports clubs (which includes professional athletics teams).[16] Initial values of these quantities, the first four of which are expressed in per capita terms, are added to equation (1).

Because the number of colleges and universities may not adequately capture the full extent of the college community in a metro area, I also include the total number of workers employed in these institutions. This variable should help to discern whether a metro area has, say, a particularly large university rather than a small college. In addition, although the number of elementary and secondary schools per capita is intended to serve as a rough proxy for the quality of a city's education system, it is a highly imperfect measure. As an additional proxy for school quality, I include in the regression the fraction of children between the ages of 3 and 17 who are enrolled in public school. In theory, cities with good school systems should have relatively large fractions of their school-aged children enrolled in public education. Cities with ineffective and undesirable public school systems, after all, should be characterized by higher proportions of their children attending private schools.

The second alteration takes a different approach to controlling for the influence of industrial composition. While initial shares of a metro area's employment across a broad array of sectors may offer some explanatory power with respect to human capital accumulation, how they change over time may be more relevant. That is, it may not be the initial share of employment in a city's durable manufacturing sector that affects its college fraction, but the *change* in the fraction accounted for by that sector. Again, as demonstrated in Table 4, there are substantial differences in college attainment across the 20 industries considered. Therefore, one might expect that rising shares of employment in, say, retail trade, which employs relatively few college-educated workers, would have a negative influence on a city's overall level of education; whereas, a rise in the fraction of workers employed in educational services, which employs primarily college-educated labor, would accomplish just the opposite. To address this potential misspecification of the regression, I include contemporaneous changes in each sector's employment share in (1) and drop the initial levels.

Although this approach likely introduces a simultaneity issue into the estimation (i.e., changes in employment shares may be influenced by contemporaneous changes in the fraction of college-educated workers in the local population), it should be stressed that the objects of primary interest in this second alteration are the coefficients on log population and the initial college fraction, not those on the changes in each industry share (which, accordingly, may be biased). The idea behind this regression, quite simply, is to see whether initial size and education are still significantly correlated with subsequent changes in human capital even after removing all of the variation in human capital accumulation associated with changes in a metro area's industrial base.

Results appear in Table 5. As before, I report coefficient estimates from three different specifications to gauge the sensitivity of the findings to variations in the model. The first column, labeled *I*, reports coefficients from the regression of the change in the college attainment rate on the initial estimated return earned by college graduates, log population, log density, the initial college fraction, and initial quantities of the 10 amenities listed above.[17] Interestingly, five of these amenities enter significantly. Eating and drinking places per capita, live entertainment venues per capita, and numbers of colleges and universities all enter

[16] Many of these variables were identified by Glaeser, Kolko, and Saiz (2001) as being significantly related to population growth.

[17] Results were similar when the 20 initial industry shares were included. Because reporting all of these additional coefficients would have been excessive, I have omitted them from the regression.

Table 5

Robustness Checks

Variable	I	II	III
Initial estimated return	–0.015 (0.011)	–0.028* (0.01)	–0.027* (0.01)
Initial log population	0.005* (0.002)	0.004* (0.002)	0.004* (0.002)
Initial log density	0.002 (0.001)	0.0006 (0.002)	–0.0001 (0.001)
Initial college fraction	0.1* (0.02)	0.13* (0.02)	0.11* (0.02)
Initial eating and drinking places per capita	11.6* (3.8)	—	6.6* (4)
Initial movie theaters per capita	56.6 (72.1)	—	47.7 (72.4)
Initial live entertainment venues per capita	47.7* (23.4)	—	25.7 (24.9)
Initial elementary and secondary schools per capita	59.5 (53.3)		64.9 (54.5)
Initial museums, botanical gardens, zoos	–0.0003* (0.0001)	—	–0.0002 (0.0001)
Initial colleges and universities	0.0005* (0.0001)	—	0.0003* (0.0001)
Initial employment in colleges and universities	0.0008 (0.002)		–0.00004 (0.002)
Initial hospitals	–0.0003* (0.00007)	—	–0.0002* (0.0001)
Initial commercial sports clubs	0.0001 (0.0003)	—	0.0002 (0.0002)
Initial fraction students in public school	–0.009 (0.03)	—	–0.02 (0.03)
Δ Fraction mining	—	0.37* (0.22)	0.32 (0.23)
Δ Fraction construction	—	–0.01 (0.22)	–0.05 (0.24)
Δ Fraction nondurable manufacturing	—	0.03 (0.22)	–0.0003 (0.24)
Δ Fraction durable manufacturing	—	0.06 (0.22)	0.014 (0.23)
Δ Fraction transportation	—	0.05 (0.24)	0.08 (0.26)
Δ Fraction communications	—	–0.29 (0.29)	–0.27 (0.3)
Δ Fraction utilities	—	0.09 (0.28)	0.07 (0.29)
Δ Fraction wholesale trade	—	0.24 (0.25)	0.21 (0.26)
Δ Fraction retail trade	—	0.05 (0.22)	0.04 (0.24)
Δ Fraction finance, insurance, real estate	—	0.56* (0.25)	0.51* (0.26)
Δ Fraction business and repair services	—	0.56* (0.26)	0.53* (0.27)
Δ Fraction private household services	—	–0.04 (0.5)	0.04 (0.53)
Δ Fraction personal services	—	–0.11 (0.21)	–0.11 (0.22)
Δ Fraction entertainment and recreation services	—	0.007 (0.24)	–0.06 (0.26)
Δ Fraction medical services	—	0.28 (0.24)	0.25 (0.25)
Δ Fraction educational services	—	0.48* (0.23)	0.41 (0.25)
Δ Fraction social services	—	0.93* (0.38)	0.8* (0.4)
Δ Fraction other professional services	—	0.63* (0.36)	0.57 (0.37)
Δ Fraction public administration	—	–0.02 (023)	–0.06 (0.25)

NOTE: The dependent variable is the change in college fraction for 1980-90 and 1990-2000. Region indicators and a dummy for the 1980-90 decade appear in all regressions. Employment in colleges and universities is expressed in 10,000s. Heteroskedasticity-consistent standard errors, adjusted for correlation within metro areas, appear in parentheses; * denotes significance at the 10 percent level or better.

positively; the number of museums, botanical gardens, and zoos and the number of hospitals both enter negatively.[18] In spite of this result, however, the coefficients on log population and the college fraction do not change appreciably from what was reported above.

The second column of results drops these 10 amenities and adds changes in 19 of the 20 industry employment shares to determine whether specifying industrial composition in 10-year differences rather than initial levels makes any difference in the remaining coefficient estimates.[19] Compared with the specification of industry mix in initial levels, a greater number of industries now produce significant associations, and many of these are quite reasonable, at least intuitively. An increase in the importance of finance, insurance, and real estate, as well as social and business and repair services, for example, should be associated with increases in the fraction of workers with a bachelor's degree or more. These sectors, after all, tend to employ relatively large proportions of college-educated labor. This conclusion is indeed borne out regardless of whether the 10 amenities listed above are included in the regression (column *III*) or not (column *II*).

At the same time, inclusion of changes in industrial composition has very little impact on the estimated initial population and college fraction coefficients. Both remain statistically significant, and the magnitudes are very similar to those reported in all previous specifications. Such a finding seems to reinforce the conclusion that, even after accounting for a city's industrial composition, a city's initial scale and education are strongly associated with the rate at which it accumulates highly educated workers.

Of course, characterizing the industrial composition of a metro area by using a set of 20 broad sectors is less than ideal. There is a fair amount of heterogeneity inherent in each industry; hence, this classification scheme may miss important differences in the types of employers present in each metro area. For example, the types of employers belonging to the nondurable manufacturing sector in one city (e.g., drugs or chemicals) may be quite different from those in another (e.g., textiles or food processing). These differences may be important in explaining the growth of human capital, but would be missed by the present analysis. More seriously, these unmeasured differences may very well be directly correlated with either population or the college fraction. In such an instance, the coefficients reported thus far for these two regressors would be upwardly biased.[20]

I attempt to address this matter by looking, instead, at a collection of more than 200 industries, representing sectors at a mostly three-digit (standard industrial classification) level, although some two- and four-digit industries, as well as combinations of two-, three- and four-digit industries, also appear.[21] These are the most detailed industrial categories available in the decennial Census files.

Unfortunately, because adding more than 200 industry shares to the estimation of (1) is not practically feasible, I use the following approach: First, I create a "predicted" college attainment fraction, $PColl_{i,t}$, for each metro area, i, in each year t, as follows:

$$PColl_{i,t} = \sum_{s=1}^{N_{i,t}} Share_{s,i,t} Coll_{s,t}, \tag{2}$$

where $Share_{s,i,t}$ is the share of sector s in metro

[18] The number of hospitals may be associated with the growth in the numbers of relatively old workers who tend to possess less education than younger workers.

[19] Because changes in all 20 industry shares (by definition) sum to 0, I drop the change in the employment share of agriculture, forestry, and fisheries.

[20] For example, one city may attract human capital because it has a strong presence of nondurable manufacturing, which hires mostly highly educated workers (e.g., drugs and chemicals), whereas another may attract less human capital because it has a strong presence of nondurable manufacturing, which hires primarily less-educated workers (e.g., textiles and food processing). The presence of high- and low-human capital nondurable manufacturers will therefore be directly related to each city's initial stock of human capital, but the association between industrial composition and human capital accumulation (which is significant in this example) will be picked up by the initial stock of human capital.

[21] Specifically, there are 223 industries in the 1980 data, 221 in the 1990 data, and 214 in the 2000 data. These are identified using consistent codes established using the correspondence provided by the U.S. Bureau of the Census. Tobacco and crude petroleum and natural gas are examples of two-digit industries; drugs, electric light and power, and grocery stores are examples of three-digit industries; jewelry stores and retail florists are examples of four-digit industries.

Table 6

Residual College Fraction Regressions

Variable (initial value)	I	II
Estimated return	–0.026* (0.01)	–0.024* (0.01)
Log population	0.003* (0.001)	0.004* (0.002)
Log density	0.001 (0.001)	0.0006 (0.001)
College fraction	0.08* (0.02)	0.065* (0.02)
Eating and drinking places per capita	—	5.8* (3.3)
Movie theaters per capita	—	44.3 (65.2)
Live entertainment venues per capita	—	55.7* (21.1)
Elementary and secondary schools per capita		38.6 (45.4)
Museums, botanical gardens, zoos	—	–0.0003* (0.0001)
Colleges and universities	—	0.0004* (0.0001)
Employment in colleges and universities	—	0.0002 (0.002)
Hospitals	—	–0.0002* (0.00006)
Commercial sports clubs	—	0.0001 (0.0002)
Fraction students in public school	—	–0.02 (0.03)

NOTE: The dependent variable is the change in the difference between a city's college fraction and its predicted college fraction based on its detailed industrial composition. Region indicators and a dummy for the 1980-90 decade appear in all regressions. Employment in colleges and universities is expressed in 10,000s. Heteroskedasticity-consistent standard errors, adjusted for correlation within metro areas, appear in parentheses; * denotes significance at the 10 percent level or better.

area i's total employment in year t, $Coll_{s,t}$ is the college completion fraction for sector s in year t (calculated using aggregate data for the United States), and $N_{i,t}$ is the number of sectors in metro area i in year t. Second, I compute a "residual" college fraction given by $(Coll_{i,t} - PColl_{i,t})$, which measures the difference between a city's actual college-completion fraction and the fraction that would result if its industries resembled the national average. I interpret this difference as the part of a city's college-attainment fraction that is not explained by its detailed industry composition. I then consider regressions of the form

$$(3) \qquad \Delta\left(Coll_{i,t} - PColl_{i,t}\right) = \mu + \delta_t + \beta X_{i,t} + \varepsilon_{i,t},$$

where two specifications of the regressors $X_{i,t}$ are considered: One controls for the estimated college return, log population, log density, and the college fraction, all in initial levels; the other further adds initial values of the 10 amenities discussed above. The resulting estimates appear in Table 6.

In general, they demonstrate very little change from what has already been reported. Among the amenities, the same five variables (eating and drinking places per capita; live entertainment venues per capita; numbers of museums, botanical gardens, and zoos; numbers of colleges and universities; and numbers of hospitals) all enter significantly and with the same signs as before. Additionally, the initial college-return produces a significantly negative coefficient, while the logarithm of population and the initial fraction of college-educated workers in total employment generate significantly positive coefficients.

With these latter two regressors, it is worth noting that the coefficients are now somewhat smaller than what is reported in Tables 3 and 5. For example, in Table 6, log population produces coefficients between 0.003 and 0.004 rather than between 0.003 and 0.006 previously, whereas the initial college completion rate generates a coefficient ranging from 0.065 to 0.08 rather than from 0.1 to 0.16. These decreases are consistent with the idea mentioned previously that using

Table 7

Growth Regressions

Dependent variable	Specification	Initial college fraction	Initial log population	Initial average log hourly wage
Population growth	*I*	0.28* (0.13)	—	—
	II	—	0.011* (0.005)	—
	III	0.41* (0.14)	0.02* (0.006)	–0.33* (0.08)
Average hourly wage growth	*I*	0.26* (0.04)	—	—
	II	—	—	–0.11* (0.035)
	III	0.42* (0.05)	0.02* (0.003)	–0.39* (0.05)

NOTE: Regressions of metro area–level population growth and average hourly wage growth on initial values of the college fraction, log population, and average log hourly wages. Region indicators and a dummy for the 1980-90 decade appear in all regressions. Heteroskedasticity-consistent standard errors, adjusted for correlation within metro areas, appear in parentheses; * denotes significance at the 10 percent level or better.

20 broad industry shares leads to upwardly biased coefficients on the initial college fraction and log population. Still, the evidence is remarkably consistent with respect to the influence of these two variables. Regardless of how the statistical model is specified, initial population and education are significant predictors of human capital accumulation.

Human Capital, Growth, and Divergence

The finding that more-populous and -educated cities tend to experience the largest increases in human capital has an intriguing implication with respect to the geographic distributions of population and college-educated labor. Specifically, it suggests that the distributions of these two quantities should have been characterized by increasing concentration over the 1980-2000 period. Human capital accumulation, after all, tends to be faster in cities with larger initial fractions of highly educated workers. This mechanism should then lead to a growing gap between the education levels across cities over time as the top end of the distribution pulls away (or "diverges") from the bottom. Because previous work has shown that more-educated cities also tend to see faster population growth (e.g., Glaeser, Scheinkman, Shleifer, 1995), I arrive at a similar implication with respect to the distribution of population. This section examines whether there has been this type of "divergence" in the distribution of these two quantities.

Before doing so, I attempt to establish some basic results relating the growth of two quantities—population and average hourly wages—to education. While the former is of greater interest in this particular exercise, the latter more closely resembles the object of interest in most studies of economic growth (i.e., per capita income). Results from the regression of each quantity's 10-year growth rate on the initial level of human capital appear in the specifications labeled *I* in Table 7.[22] Not surprisingly, each shows a significantly positive association with initial education. Here, the magnitudes indicate that a 1-standard-deviation (i.e., a 6.5-percentage-point) increase in a city's college attainment rate tends to be accompanied by a 1.8-percentage-point rise in its rate of population growth and a 1.7-percentage-point rise in its rate of average wage growth over the next 10 years. These figures represent, respectively, 16 and 20 percent of the cross-sectional standard deviations in these two growth series. These associations, therefore, seem to be both statistically and economically important.

To explore whether there has been divergence across city-level human capital, population, and

[22] As with all of the other regressions, these include three region dummies and an indicator for the 1980-90 decade.

average wages, I consider two approaches. The first looks for so-called β-convergence, the test for which involves a simple regression of the growth of a quantity on its initial level.[23] A negative coefficient on the initial level of a variable would indicate a tendency for that quantity to converge to a common level across metro areas. After all, a negative coefficient would indicate that cities with low levels of human capital, for example, would experience faster human capital growth than cities with high levels. This process should generate a less-concentrated distribution of human capital over time as the bottom of the distribution catches up with the top. The second approach looks for σ-convergence, which is based on how the cross-sectional dispersion of a particular quantity changes over time. Decreasing dispersion (i.e., falling concentration) would be indicative of σ-convergence.[24]

One common criticism of these statistical approaches, particularly tests for β-convergence, pertains to the appropriateness of pooling a set of extremely heterogeneous economies in the same regression (see Durlauf and Quah, 1999). While this point is certainly valid when considering studies of countries, which tend to vary substantially in terms of various fundamental characteristics including how their economies function (e.g., Japan and Nigeria), it is less likely to be a significant issue when comparing the experiences of metro areas within the same country (e.g., Seattle and Atlanta).

The β-convergence results for metro-area college attainment are already well-established in the findings shown thus far. The strong positive association between the initial level of a city's college fraction and its subsequent change over the next decade indicates divergence in this variable. Results for the logarithm of population and the average log hourly wage appear in the specifications labeled *II* in Table 7.

The population series also shows divergence which, intuitively, is precisely what one would expect in light of the results shown to this point. Larger populations tend to be associated with more rapid human capital accumulation, which raises education levels. This, in turn, leads to faster population growth. Hence, one would expect to see a positive association between initial population and its subsequent rate of growth. Interestingly, however, the positive association between initial population and its subsequent growth also holds after conditioning on the initial college fraction and the initial average log hourly wage. This result is reported in specification *III*. The direct association between population and population growth, therefore, does not seem to be driven entirely by education. There is some aspect of metro area size that, independent of education, draws additional population.

Average hourly wages, by contrast, show evidence of convergence rather than divergence. That is, higher average wages tend to be followed by slower rates of wage growth over the next decade. This finding, too, is sensible given the evidence already presented. Recall that higher wages tend to be accompanied by slower subsequent human capital accumulation. The significantly negative coefficients on the initial college return in the regression results presented above demonstrate this point clearly. Slower human capital accumulation, then, implies slower growth of average hourly wages. Thus, one would expect to see a negative association between initial average wages and future wage growth. This relationship turns out to hold whether initial education and log population are accounted for or not (compare specifications *II* and *III*).

To look at σ-convergence, I need a measure that characterizes the degree of spread in the distributions of human capital, log population, and the average log hourly wage.[25] In an effort to keep the analysis broad, I consider several possible

[23] Again, all regressions also include three region dummies and a time effect to pick up differences in growth across decades. The β in β-convergence refers to the coefficient on the initial level of a variable in a growth regression.

[24] The σ in σ-convergence refers to the standard deviation. Barro and Sala-i-Martin (1995) provide an overview of the statistical techniques commonly used in studies of convergence/divergence.

[25] For this exercise, I use population and average wages in logarithmic form because the distributions of their levels will tend to show increasing dispersion even if growth is unrelated to the initial level. For example, the gap between the populations of two cities, one with population of 100, the other with a population of 1000, will grow wider if both cities grow by the same percentage (and possibly if the smaller city grows by a larger percentage).

Table 8

Features of the Education, Population, and Average Hourly Wage Distributions

Variable	Statistic	1980	1990	2000
College fraction	Mean	0.178	0.219	0.253
	10th percentile	0.122	0.148	0.17
	25th percentile	0.15	0.18	0.206
	50th percentile	0.175	0.216	0.248
	75th percentile	0.203	0.25	0.284
	90th percentile	0.238	0.296	0.339
	Standard deviation	0.043	0.054	0.064
	90-10 difference	0.116	0.148	0.168
	90-50 difference	0.063	0.08	0.091
	50-10 difference	0.053	0.068	0.077
	75-25 difference	0.053	0.069	0.078
Log population	Mean	12.96	13.06	13.19
	10th percentile	11.76	11.79	11.91
	25th percentile	12.07	12.17	12.36
	50th percentile	12.77	12.88	13.01
	75th percentile	13.61	13.71	13.9
	90th percentile	14.37	14.62	14.76
	Standard deviation	1.057	1.075	1.092
	90-10 difference	2.61	2.83	2.86
	90-50 difference	1.6	1.74	1.75
	50-10 difference	1	1.09	1.1
	75-25 difference	1.54	1.54	1.54
Average log hourly wage	Mean	2.46	2.45	2.5
	10th percentile	2.33	2.32	2.43
	25th percentile	2.39	2.38	2.5
	50th percentile	2.46	2.45	2.55
	75th percentile	2.51	2.51	2.61
	90th percentile	2.58	2.58	2.68
	Standard deviation	0.098	0.109	0.105
	90-10 difference	0.24	0.26	0.25
	90-50 difference	0.12	0.14	0.13
	50-10 difference	0.13	0.13	0.12
	75-25 difference	0.12	0.13	0.11

NOTE: Statistics are based on 188 metro areas for the college fraction and average log hourly wage and 187 metro areas for log population.

measures: the standard deviation and a host of inter-quantile differences (e.g., the difference between the 90th percentile and the 10th). One important consideration in looking at these distributional features is maintaining a consistent sample of metro areas. The sample of metro areas identified by the Census does change from one year to the next. As a consequence, there may be changes in the degree of spread in the distribution of these variables that stem from changes in the composition of the sample rather than an actual convergence or divergence mechanism. In computing these distributional features, then, I confine the sample to those 188 metro areas that appear in all three years.

The resulting estimates appear in Table 8. Looking at the distribution of college attainment rates, it is evident that, although there has been an increase in the fraction of workers with a bachelor's degree or more at all points of the distribution, that increase has been larger at the top than at the bottom. The 90th percentile, for example, rose by more than 10 percentage points between 1980 and 2000, increasing from 0.238 to 0.339. The corresponding increases for the median and 10th percentiles over this period were 7.3 and 4.8 percentage points. Accordingly, each of the four listed percentile gaps (90-10, 90-50, 50-10, 75-25) grew wider over time. Rising dispersion can also be inferred from the evolution of the standard deviation, which started at 0.043 in 1980, rose to 0.054 in 1990, and stood at 0.064 by 2000. Evidently, human capital became more unevenly distributed during this time frame.[26]

The logarithm of population, the distributional features of which appear just below the human capital results in Table 8, reveals a similar trend. On average, metro areas in the United States experienced population gains between 1980 and 2000, and these gains were registered at all five quantiles of the distribution. Again, however, the gains tended to be somewhat larger at the top of the distribution than at the bottom. With the exception of the inter-quartile difference (75-25), which did not change between 1980 and 2000, all other quantile differentials increased in both decades. Increasing dispersion in the logarithm of population also shows up in the standard deviations, which increased from 1.057 in 1980 to 1.075 in 1990 to 1.092 in 2000. Log population, therefore, also shows evidence of both types of divergence.

These results are particularly striking because they stand somewhat at odds with what conventional economic analysis might suggest. Indeed, as cities grow in population, they tend to become more congested, which, in turn, raises costs (financial and otherwise) to both workers and employers. These "agglomeration diseconomies" should, therefore, work to slow subsequent rates of population growth as firms and workers seek less-congested labor markets. Similarly, as the fraction of workers with a bachelor's degree rises, the relative return received by college-educated workers (all else equal) should decline because the supply of such workers has risen relative to demand. This is a standard diminishing marginal productivity argument whereby the return received by a factor of production (e.g., college-educated labor) declines as it is used more intensively relative to all other inputs. A lower return paid to college-educated labor, of course, should reduce the rate at which workers with a bachelor's degree move into an area. Empirically, however, there seems to be little support for these theoretical ideas in the data.

Recall that average hourly wages show a very different pattern. Regressions of wage growth on initial wage levels reveal a significantly negative relationship between the two. One might expect, therefore, to see a decrease in the degree of dispersion in the distribution of city-level average log wages. The estimated dispersion measures in Table 8, however, show only a decrease between 1990 and 2000, when the standard deviation and all four quantile differences narrowed. During the 1980s, all but the 50-10 difference increased. These results demonstrate an important difference between β- and σ-convergence. Although a negative association between the initial level of a variable and its subsequent growth rate may certainly reduce the degree of variance in a distribution, it may also increase it. Durlauf and Quah

[26] Moretti (2004) documents a similar rise in the degree of human capital "inequality" across U.S. metro areas.

(1999), for example, show how β-convergence may generate a wider distribution if economies with low levels of a variable overshoot economies with high levels. Distributional dynamics of this sort may help to explain these results.

Another possible explanation may relate to the influence of population and the college fraction, which, as shown in Table 7, tend to be positively related to wage growth over the next decade. As these two variables have diverged, they may have led to a divergence in wage levels during the 1980s if their influence outweighed the natural tendency for wage levels to converge. Assuming that this natural tendency was stronger during the 1990s, of course, the cross-sectional dispersion in average log wages would have declined.[27]

CONCLUDING DISCUSSION

This paper has explored the issue of human capital accumulation across a set of U.S. metro areas. Among the more prominent findings is that cities with larger populations and larger fractions of workers with college degrees tend to see faster growth in their stocks of human capital. Because human capital also tends to be positively associated with population growth, this process has led to a divergence of both human capital and population in the United States between 1980 and 2000. Hence, the largest and most-educated cities in the country have tended to accumulate population and human capital faster than smaller and less-educated cities.

The divergence of human capital and population has not, interestingly, generated much divergence with respect to average wage levels across cities. Although the amount of dispersion in the distribution of metro area–level average wages did grow larger between 1980 and 2000, this growth occurred during the decade of the 1980s. Dispersion in wage levels actually declined somewhat between 1990 and 2000. This result could be related to the mechanism described above in which the increasing concentration of workers with college degrees may depress the returns they receive.

If true, however, why would college-educated workers continue to flock to labor markets with large populations and stocks of highly educated workers? Glaeser (1999) suggests that workers with a college degree may seek to surround themselves with other college graduates because they are able to learn from one another. Highly educated workers, according to this line of reasoning, are especially committed to the acquisition of productive skills. Since previous work suggests that there may be productive externalities associated with the presence of college-educated individuals (e.g., Moretti, 2004), the positive association between initial college attainment rates and subsequent changes in these rates may reflect the desire of highly educated workers to reside in environments that facilitate learning.

Peri (2002) echoes this view, suggesting that, if skill acquisition is an important reason for the concentration of human capital in cities, we should expect to see large numbers of "young" college-educated workers in cities. Young workers, after all, are more likely than their older counterparts to seek learning opportunities because they are in the early stages of their careers and, therefore, know relatively little. The evidence he reports is certainly consistent with this idea. Between 1970 and 1990, the ratio of college-educated workers with fewer than 20 years of work experience to those with more than 20 years rose from 1.5 to 2.12 within the metro areas of the United States.

Populous cities may also help facilitate the job search process for highly educated married couples. Costa and Kahn (2000) suggest that "power couples" (i.e., those in which both partners have a bachelor's degree or more) have increasingly moved into large metro areas over the past several decades because cities are more likely to offer job opportunities for both spouses. Large cities, therefore, may provide a solution to the occupational co-location problem.

An additional possibility that deserves to be mentioned involves the amenity value of college-educated workers themselves. That is, while the

[27] A similar argument relating to the relative strengths of the college attainment rate, population, and average wage levels could be made in explaining the divergence patterns of human capital and log population. In those cases, evidently, the mechanisms leading to divergence were stronger than any effects that wage levels might have had.

college-educated may be enticed to locate in cities with a large presence of certain types of establishments (say, eating and drinking places), they might also want to be around other college-educated workers because they desire homogeneity in their social interactions. So, even though concentrations of highly educated workers may be associated with diminishing returns and lower earnings (at least, all else equal), college graduates may still want to surround themselves with other highly educated workers because they find them to be desirable neighbors. Of course, a strong presence of college-educated workers may also be associated with characteristics that have not been accounted for directly here (e.g., low crime, greater civic engagement, good schools), but which are especially desirable to these types of workers.

Whatever the reason, divergence in the distribution of human capital may, over longer time horizons, begin to lead to divergence in earnings and productivity across cities and regions. Although there is only limited evidence of that having occurred between 1980 and 2000, it may occur to a greater extent in future decades. In particular, if technologies respond to the supply of skills (e.g., as suggested by Acemoglu, 1998), cities with large stocks of college-educated labor may increasingly use technologies that complement these types of workers. This trend may further reinforce the divergence of human capital by encouraging highly educated workers to congregate in the most educated cities as well as lead to greater productivity differentials between cities with small stocks of human capital and those with large stocks.

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APPENDIX

Census Data

The data are taken from the 1980, 1990, and 2000 5 percent samples of the Integrated Public Use Microdata Series (IPUMS) described by Ruggles et al. (2004). Specifically, I use the 1980 5 percent state sample, the 1990 5 percent state sample, and the 2000 5 percent sample. These files have roughly 11.3 million, 12.5 million, and 14 million observations, respectively.

To calculate educational attainment distributions among each metropolitan area's labor force, I focus on the working age population (i.e., those who are between the ages of 18 and 65) who report positive wage and salary earnings and are not in school at the time the Census was taken. In estimating the returns to various levels of formal schooling, I further limit the analysis to white males who earn between $1 and $250 per hour. This trimming procedure is designed to eliminate the influence of outlier observations, which occasionally appear due to the computation of hourly earnings as the ratio of annual wage and salary earnings to the product of weeks worked and usual hours per week. All dollar figures are converted to real terms (year 2000 dollars) using the personal consumption expenditure chain type price index.

Potential experience is calculated as the maximum of (age minus years of education minus 6) and 0. Because years of education is not reported for all individuals in the 1990 and 2000 Census, where educational attainment is sometimes reported as a range, I have imputed years of schooling completed using Table 5 of Park (1994).

As noted in the text, metro areas are defined as metropolitan statistical areas (MSAs), New England county metropolitan areas (NECMAs), or consolidated metropolitan statistical areas (CMSAs) if an MSA or NECMA belongs to a CMSA. Although somewhat large when considering local labor markets (e.g., New York– northern New Jersey–Long Island), CMSAs greatly facilitate the construction of geographic areas that have reasonably consistent geographic boundaries. Due to changes in geographic definitions, residents of one MSA within a CMSA in a particular year are sometimes categorized as residing in a different MSA (within the same CMSA) in another year. Aggregating to the CMSA level eliminates any problems arising from this type of definitional change.

Across the 210 metro areas identified in the 1980 Census, the average number of individual level observations used to construct the college attainment and industry share statistics is 14,484.1 (minimum = 1,557, maximum = 315,128). Among the 206 metro areas identified in the 1990 data, the average is 15,863.4 (minimum = 1,426, maximum = 329,632). In the 245 metro areas from the 2000 Census, the mean is 16,526.4 (minimum = 1,426, maximum = 371,278). When confining the sample to white males only for the college return regressions, the mean numbers of observations (minimum, maximum) per metro area are 6,928.1 (666, 144,886) for 1980, 7,275.7 (670, 144,698) for 1990, and 6,600.7 (516, 134,898) for 2000.

A complete list of the detailed industries appears in the IPUMS documentation at www.ipums.org. These can be found in the link to the industry codes for 1980. The 1990 and 2000 codes are translated into equivalent 1980 codes using the programs that accompany this paper.

Additional Data Details

Metro-area population density is calculated as a weighted average of county-level densities, where the weights are given by population shares. This measure is intended to give a better sense of the average density per square mile faced by a typical city dweller than average density (metro area population to metro area land area), which may be misleading, particularly among cities in the western United States, which encompass extremely large, but sparsely populated counties.

U.S. Census Regions

Midwest: Illinois, Indiana, Iowa, Kansas, Michigan, Minnesota, Missouri, Nebraska, North Dakota, Ohio, South Dakota, Wisconsin

Northeast: Connecticut, Maine, Massachusetts, New Hampshire, New Jersey, New York, Pennsylvania, Rhode Island, Vermont

South: Alabama, Arkansas, Delaware, District of Columbia, Florida, Georgia, Kentucky, Louisiana, Maryland, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia, West Virginia

West: Alaska, Arizona, California, Colorado, Hawaii, Idaho, Montana, Nevada, New Mexico, Oregon, Utah, Washington, Wyoming

Macroeconomic News and Real Interest Rates

Kevin L. Kliesen and Frank A. Schmid

Economic news affects the perceptions of investors, forecasters, and policymakers about the strength or weakness of the economy. These expectations are updated on the basis of regularly occurring surprises in macroeconomic announcement data. The response of asset prices to positive or negative announcement surprises has been a regular feature of the literature for more than 20 years. In this vein, the authors evaluate the responses of the yield of 10-year Treasury inflation-indexed securities to nearly three dozen macroeconomic announcements. They find that the real long-term rate of interest responds positively to surprises in a handful of key macroeconomic indicators, including labor productivity growth. Also, the authors find no support for the proposition that the Federal Reserve has information about its actions or the state of the real economy that is not in the pubic domain and, hence, not already priced in the real long-term interest rate.

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Federal Reserve officials often make remarks and offer their thoughts in public forums. Recent comments by Bill Poole and Janet Yellen, of the Federal Reserve Banks of St. Louis and San Francisco, respectively, have added to the discussion of the role of economic data in monetary policy:

> "I think there has been an effort to emphasize that increasingly, the policy decisions will be data-driven, driven by incoming information…"[1]

> "Uncertainties and risks that could complicate things considerably were evident even before the havoc unleashed by Hurricane Katrina, so our approach during this phase must be particularly dependent on information from incoming data."[2]

Their comments suggest that key information is contained in the evolving flow of these data that informs policymakers' assessments of the strength of the economy and perhaps also affects the future stance of policy.

Moreover, studies with macroeconomic announcement data suggest that surprises in the data can influence such things as the market price of Treasury securities or inflation expectations. We focus our analysis on the relationship between surprise data announcements and the yield on Treasury inflation-indexed securities (TIIS, a measure of the real interest rate) from January 1997 through June 2003. Consider this example: Suppose the Federal Reserve and financial market participants view the monthly jobs number within the employment report released by the Bureau of Labor Statistics as a reliable indicator of the near-term strength of the economy. In this case, a positive (negative) surprise would signal to the Fed and the markets that the economy was growing

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[1] *USA Today* (2005).

[2] Yellen (2005).

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at a quicker (slower)-than-expected pace. Under these circumstances, the demand for investment goods would be expected to increase (decrease) and the real interest rate would have to rise (fall) to clear the market.

There are many other examples of news that might affect real long-term interest rates (such as a surprise increase in labor productivity growth or in the government budget deficit). But it is not our intention here to test the theory that budget deficits cause higher interest rates or to model the real rate of interest in a macroeconomic setting. Rather, we simply test whether there is a core set of economic variables that traders in the TIIS market respond to more than others. Along these lines, other potential influences on the TIIS market are actions and commentary by Federal Reserve officials. Accordingly, we also test whether TIIS investors re-price the real long-term interest rate in response to surprises in monetary policy actions.

We look at the data from January 31, 1997, through June 30, 2003, and ask whether the real long-term rate of interest responds to a sample of 35 surprise economic announcements from that period, as well as to the surprises in the federal funds interest rate target. We measure the real long-term rate of interest of on-the-run (that is, most recently issued) 10-year TIIS. We gauge surprises in macroeconomic announcements by the difference between the expected value and the actual released value of the data series. The former is the median forecast among a sample of forecasters and market participants. Except for the growth of real GDP, the GDP price index, and nonfarm labor productivity (there are forecasts for the preliminary and revised growth rates), the latter is the first-reported value for the series.

Our analysis suggests that participants in the TIIS market respond to the announcements for seven economic data series in a statistically significant manner: business inventories, the employment cost index, the preliminary GDP estimate, initial jobless claims, new home sales, nonfarm payroll employment, and the preliminary estimate of nonfarm labor productivity. Finally, we fail to reject the hypothesis that uncertainty surrounding the real long-term interest rate is unaffected by Federal Reserve communication and surprises in monetary policy actions. Taking into consideration both those results in our analysis, we find no support for the proposition that the Federal Reserve has information about its own actions or the state of the real economy that is not in the pubic domain and, hence, priced in the real long-term interest rate.

RELATED LITERATURE

The studies most closely related to our work are Calomiris et al. (2003), Gürkaynak, Sack, and Swanson (2003), and Kohn and Sack (2003). Calomiris et al. study the response of the real interest rate, as measured by the market yield of the 10-year TIIS, to surprises in 19 macroeconomic data releases, among them the monthly federal budget deficit/surplus reported by the U.S. Treasury Department. Surprises in labor productivity or monetary policy announcements are not included in the regression. Calomiris et al. find that surprises in the federal budget surplus cause no statistically significant change in the real interest rate. Gürkaynak, Sack, and Swanson (2003) analyze the response of the *forward* real interest rate to surprises in macroeconomic data releases and in Federal Reserve monetary policy actions—that is, changes to the targeted federal funds rate set by the Federal Open Market Committee (FOMC). Their forward rates are derived from the yields of 10-year TIIS.[3] The authors fail to reject the hypothesis that the "long-term equilibrium real rate of interest" is unaffected by surprises in these productivity and federal budget numbers. In a separate regression, Gürkaynak, Sack, and Swanson (2003) study the impact on the same dependent variable of surprises in announced changes of the targeted

[3] The studied pair of 1-year forward rates applies to the 12-month time window between the maturity dates of the on-the-run 10-year TIIS and the 10-year TIIS issued 12 months earlier. Prior to July 2002, and starting in 1997, 10-year TIIS were issued only once per year, in January. This implies that the authors analyze changes to the 1-year real interest rate that is expected to prevail at the beginning of a 12-month time window that begins, on average, 8.5 years from the time of the data release. The analyzed time period runs from January 1997 through July 2002 and covers 39 macroeconomic data series.

federal funds rate; again, the authors do not reject the null hypothesis of no influence.[4] Faust et al. (2006) estimate a Kalman filter approach in gauging the surprise effect of macroeconomic announcements to the U.S. dollar exchange rate and to nominal (short- and long-term) interest rates. These authors find that the surprises in such announcements for a wide variety of macroeconomic variables have a statistically significant effect on the dollar and interest rates.

Kohn and Sack (2003) study the effect of Federal Reserve communication on financial variables using daily observations for the period January 3, 1989, through April 7, 2003. In their analysis, Fed communication comprises statements released by the FOMC and, since June 1996, Congressional testimonies and speeches delivered by the Chairman of the Federal Reserve. Kohn and Sack make no attempt to gauge the influence on the *level* of Treasury yields; rather, the authors measure the effect of Fed communication on Treasury yield *volatility*. Kohn and Sack investigate the effect that Federal Reserve communication has on various financial variables, such as the yields (to maturity) of the nominal 2-year and 10-year Treasury notes. Kohn and Sack find that statements of the FOMC and testimonies of the Chairman of the Federal Reserve have a statistically significant impact on the variance of 2-year and 10-year Treasury note yields; no such influence was found for the Chairman's speeches. We build on Kohn and Sack when studying the effect of Federal Reserve communication on the (conditional) variance of the yield of the 10-year TIIS or, put differently, on the uncertainty that surrounds the real long-term rate of interest.

THE DATA

Our analysis covers the period from January 31, 1997, through June 30, 2003. The starting date of this sample period is determined by the availability of the 10-year TIIS yield; the ending date is determined by the series of macroeconomic data releases provided by Money Market Services (MMS). The dataset comprises median polled forecast values for 38 macroeconomic data series, along with the sample standard deviations of these forecast values. The MMS survey is conducted every Friday morning among senior economists and bond traders with major commercial banks, brokerage houses, and some consulting firms, mostly in the greater New York, Chicago, and San Francisco areas. Among these 38 variables in the survey, there are three items—CPI, PPI, and retail sales—for which there also exists a "core" measure. Although the comprehensive versions of the CPI and the PPI include food and energy items, the respective core measures do not. For retail sales, the narrowly defined concept excludes motor vehicles and parts. In the regression analysis, we use the core concepts only; this leaves us with 35 macroeconomic variables.[5]

We relate daily changes in the real long-term rate of interest to the surprise component in macroeconomic data releases. Like Gürkaynak, Sack, and Swanson (2003), we define the surprise component as the difference between the actual and the median forecast values; but unlike these authors (and unlike Calomiris et al.), we normalize these surprises by the sample standard deviation of the individual forecasts, which we take to be a measure of forecaster uncertainty surrounding these expectations. In the literature, normalizing announcement surprises, though common, is not universal.[6] We also control for the surprise component in changes (or the absence thereof) of the targeted federal funds rate, which we measure as suggested by Kuttner (2001) and discussed by Watson (2002). For each scheduled and unsched-

4 These findings of surprises in macroeconomic data releases and monetary policy actions on real interest rates are included only in the Gürkaynak, Sack, and Swanson working paper (2003) but not in the published version (Gürkaynak, Sack, and Swanson, 2005).

5 We find no statistically significant difference, for any of our statistical analyses, between the core and the comprehensive measures.

6 Fleming and Remolona (1997, 1999) calculate the surprise component by normalizing the difference between the actual and the forecast values by the mean absolute difference observed for the respective variable during the sample period. Balduzzi, Elton, and Green (2001) normalize the difference between the actual and the forecast values by the standard deviation of this difference during the sample period. Gürkaynak, Sack, and Swanson (2005) normalize the announcement surprise by its standard error. Faust et al. (2006) simply use a non-normalized forecast error (surprise) in their analysis of intraday exchange rate and nominal interest rate data.

uled FOMC meeting, we scaled up by $30/(k+1)$ the change of the price of the federal funds futures contract for the current month on the day of the FOMC meeting, t, where $t + k$ denotes the last calendar day of the month.[7] (Note that this variable is not on the same scale as the surprise component in the macroeconomic data releases.) In a sensitivity analysis, we use an alternative measure of the surprise component in monetary policy actions; this alternative measure, devised by Poole and Rasche (2000), rests on price changes of federal funds futures contracts also.[8] Finally, we control for Federal Reserve communication and actions. Our concept of Federal Reserve communication comprises (i) the Fed Chairman's semiannual testimony to Congress (formerly known as Humphrey-Hawkins testimony) and (ii) speeches and other testimonies of the Fed Chairman.

In announcement studies, the timing of the data releases can sometimes be an issue. This is particularly true if intraday observations are used. In this paper, there are two potential timing issues, neither of which is likely to significantly influence the results because we do not use intraday prices. First, most data releases occur in the morning at 8:30 and 10:00 eastern standard time. However, there are some releases that occur in the afternoon, such as consumer credit and the budget surplus/deficit, and some that have an irregular release time (auto and truck sales). This is also the case with Fed speeches and testimonies, which can occur when markets are open or closed. A second potential issue are data releases and speeches that occur on holidays or when the markets are closed. Data releases on days when the markets were closed were moved to the next trading day (the day on which this information was priced in the marketplace). We also moved Federal Reserve communication to the next trading day if this communication occurred after hours (that is, after the real interest rate had been recorded) or on days on which there was no trading. Thus, in some cases, U.S. markets will have a shorter time period in which to react to the announcement surprise, while in other cases they will have a longer time period.

Table 1 shows the frequency of the macroeconomic announcements during the period of analysis (January 1997 through June 2003). The number in parentheses—the number of data releases during the analyzed time period—differs because of missing values in the recorded real interest rate. For example, there are 77 observations (surprises) for most monthly variables, such as business inventories. Of the 77 observations for business inventories, 68 were used. Note that Table 1 also includes two monetary policy variables and one Fed communication variable. We also report matches for scheduled and unscheduled FOMC meetings—the federal funds target variable, the surprise component of which was calculated as outlined above—and the two Federal Reserve communication variables defined above. The only weekly series in the dataset, initial jobless claims, has the highest frequency. The next-to-highest frequency is observed for testimonies other than semiannual testimony to Congress, followed by monthly data releases, FOMC actions (federal funds target), quarterly data releases, and the Chairman's semiannual testimonies to Congress. An exception is nonfarm productivity, which entered the MMS dataset during the analyzed time period. The first surveyed number refers to the first quarter of 1999.

Table 2, center column, offers a frequency distribution for the coincidence of surprises in macroeconomic data releases (MMS survey) and monetary policy actions. For instance, in the sample period of 1,527 trading days, there are 445 trading days on which there were no surprises in data releases or monetary actions, possibly because no data were released or no action taken. There are 600 trading days (39 percent) with more than one surprise and 268 trading days (18 percent) with more than two surprises. Table 2 (right column) offers a frequency distribution with Federal Reserve communication included.

[7] Following Gürkaynak, Sack, and Swanson (2003), we use the (unscaled) change in the price of the federal funds futures contract due to expire in the following month if the FOMC meeting took place within the last seven calendar days of the month.

[8] See Gürkaynak, Sack, and Swanson (2002) for a discussion of how measures of market expectations are measured in relation to monetary policy actions.

Table 1

Number of Total Macroeconomic Announcements and Monetary Policy Variables that Correspond with Daily Inflation Compensation Observations

Data series	Total (actual used)
Auto sales	77 (68)
Business inventories	77 (67)
Capacity utilization	77 (67)
Civilian unemployment rate	77 (67)
Construction spending	77 (72)
Consumer confidence	77 (69)
Consumer credit	77 (72)
Consumer price index (CPI-U)	77 (74)
CPI excluding food and energy (CPI-U, "core")	77 (74)
Durable goods orders	77 (69)
Employment cost index (Q)	25 (25)
Existing home sales	61 (56)
Factory orders	77 (72)
Federal funds target: unscheduled FOMC meeting	4 (4)
Federal funds target: scheduled FOMC meeting	52 (50)
GDP price index (advance) (Q)	26 (26)
GDP price index (preliminary) (Q)	26 (22)
GDP price index (final) (Q)	26 (23)
Goods and services trade balance (surplus)	77 (74)
Chairman's speeches and testimonies	145 (137)
Hourly earnings	74 (63)
Housing starts	77 (73)
Industrial production	77 (67)
Initial jobless claims (W)	334 (306)
Leading indicators	78 (73)
Purchasing managers index (PMI)	77 (65)
New home sales	78 (74)
Nonfarm payrolls	77 (66)
Nonfarm productivity (preliminary)	17 (16)
Nonfarm productivity (revised)	17 (17)
Personal consumption expenditures	78 (62)
Personal income	78 (62)
Producer price index (PPI)	77 (67)
PPI excluding food and energy ("core")	77 (67)
Real GDP (advance) (Q)	26 (26)
Real GDP (final) (Q)	26 (22)
Real GDP (preliminary) (Q)	26 (23)
Retail sales	77 (72)
Retail sales excluding autos ("core")	77 (72)
Treasury budget (surplus)	77 (71)
Truck sales	77 (68)

NOTE: Variables not included in the dataset of macroeconomic data releases are italicized. Monthly series if not indicated otherwise (Q: quarterly; W: weekly). Numbers in parentheses indicate actual number of observations used in the analysis; this number differs from total because of missing observations for the measures of inflation compensation due to holidays or unreported values.

Table 2

Frequency Distribution of Concurrence in Surprises

Number of surprises per trading day	MMS survey and federal funds target	MMS survey, federal funds target, and Federal Reserve communication
0	445	410
1	482	478
2	332	343
3	147	159
4	82	94
5	21	24
6	12	12
7	3	3
8	1	2
9	2	2
Total	1,527	1,527

EMPIRICAL APPROACH AND FINDINGS

The empirical approach rests on the following regression equation:

$$(1) \qquad r_t - r_{t-1} = \alpha + \beta \cdot D + \sum_{k=1}^{35} {}_{k} \cdot x_t^k + \gamma \cdot ff_t + \varepsilon_t ,$$

where $r_t - r_{t-1}$ is the change in the real interest rate from trading day $t-1$ to trading day t; D is an indicator variable that is equal to 1 if all explanatory variables are equal to 0 (and is equal to 0 otherwise); x_t^k is the surprise component in the macroeconomic data release; ff_t is the surprise component in the Federal Reserve action (the federal funds target variable); and ε_t is an error term.[9]

The change in the real long-term interest rate is measured by the daily change in the on-the-run 10-year TIIS yield. Although the Treasury has in the past issued 5- and 20-year and a small number of 30-year TIIS, we focus solely on the 10-year yield because that is the maturity that has been continuously issued since 1997. Figure 1 shows a kernel estimate of the distribution of this dependent variable (thick line), along with a frequency distribution (candlesticks) and a normal distribution (blue line) based on the sample moments. The change in the real interest rate exhibits statistically significant excess kurtosis (5.164) and mild but statistically significant skewness (0.401).[10]

Table 3 shows the results of regression equation (1). The table shows traditional t values and—because of the excess kurtosis of the dependent variable—significance levels obtained from distribution free bootstrap t intervals (see Efron and Tibshirani, 1993). The empirical results reported in the table suggest that there are seven economic announcements that matter: business inventories, the employment cost index, the annualized rate of growth of the GDP price index (preliminary estimate), initial jobless claims, new home sales, nonfarm payroll employment, and the preliminary estimate of nonfarm labor productivity. With the exception of new home sales, each of the coefficients has the predicted sign. That is, stronger-

9 The intercept indicator variable, D, eliminates the influence of certain observations on the observed mean of the dependent variable—specifically, those observations for which none of the explanatory variables contains information pertinent to the measured inflation compensation.

10 Excess kurtosis means that, compared with the normal distribution, there is excess probability mass in the center of the distribution. We use a Gaussian kernel along with an (under the null of normal distribution) optimal bandwidth of $(4/3)^{0.2} \cdot \hat{\sigma} \cdot T^{-0.2}$, where T is the number of sample observations and $\hat{\sigma}$ is the sample standard deviation (Silverman, 1986).

Table 3

On-the-Run 10-year TIIS Yield and Data Surprises

Explanatory variable	Coefficient	*t*-Statistic	Bootstrap
Auto sales	$-2.715 \cdot 10^{-3}$	–0.948	
Business inventories	$-4.176 \cdot 10^{-3}$	–2.114**	**
Capacity utilization	$1.476 \cdot 10^{-4}$	0.056	
Civilian unemployment rate	$-2.471 \cdot 10^{-3}$	–1.587	
Construction spending	$-7.883 \cdot 10^{-4}$	–0.486	
Consumer confidence	$1.184 \cdot 10^{-3}$	0.730	
Consumer credit	$1.425 \cdot 10^{-3}$	1.210	
Consumer price index (CPI-U, "core")	$-3.002 \cdot 10^{-3}$	–1.245	
Durable goods orders	$4.171 \cdot 10^{-4}$	0.379	
Employment cost index	$4.972 \cdot 10^{-3}$	1.978**	*
Existing home sales	$1.921 \cdot 10^{-4}$	0.237	
Factory orders	$-1.467 \cdot 10^{-4}$	–0.051	
Federal funds target	$7.257 \cdot 10^{-2}$	1.075	
GDP price index (advance)	$6.833 \cdot 10^{-4}$	0.388	
GDP price index (preliminary)	$1.748 \cdot 10^{-3}$	2.456**	**
GDP price index (final)	$-1.595 \cdot 10^{-3}$	–0.880	
Goods and services trade balance (surplus)	$-1.039 \cdot 10^{-3}$	–0.530	
Hourly earnings	$-8.496 \cdot 10^{-4}$	–0.520	
Housing starts	$3.213 \cdot 10^{-4}$	0.165	
Industrial production	$4.051 \cdot 10^{-3}$	1.028	
Initial jobless claims	$-2.009 \cdot 10^{-3}$	–3.103***	***
Leading indicators	$9.660 \cdot 10^{-3}$	1.395	
Purchasing managers index (PMI)	$2.855 \cdot 10^{-3}$	1.226	
New home sales	$-2.990 \cdot 10^{-3}$	–1.739*	*
Nonfarm payrolls	$3.840 \cdot 10^{-3}$	3.057***	***
Nonfarm productivity (preliminary)	$5.764 \cdot 10^{-3}$	2.263**	*
Nonfarm productivity (revised)	$-3.469 \cdot 10^{-3}$	–0.818	
Personal consumption expenditures	$-3.529 \cdot 10^{-3}$	–0.848	
Personal income	$-2.310 \cdot 10^{-3}$	–0.773	
Producer price index (PPI, "core")	$-9.685 \cdot 10^{-5}$	–0.050	
Real GDP (advance)	$1.695 \cdot 10^{-3}$	0.690	
Real GDP (preliminary)	$-3.731 \cdot 10^{-3}$	–1.282	
Real GDP (final)	$-5.376 \cdot 10^{-3}$	–1.498	
Retail sales, excluding motor vehicles and parts ("core")	$-2.279 \cdot 10^{-4}$	–0.094	
Treasury budget (surplus)	$-2.545 \cdot 10^{-3}$	–0.958	
Truck sales	$2.776 \cdot 10^{-3}$	0.847	
Intercept indicator variable (D)	$1.724 \cdot 10^{-3}$	1.062	
Intercept	$-1.342 \cdot 10^{-3}$	–1.212	
F-statistic (1)	2.147***		
F-statistic (2)	2.216***		
R^2	0.051		
R^2 adj.	0.027		
Ljung-Box statistic	3.323		
Rao's score test	13.63***		
Number of nonzero observations	1,082		
Number of observations	1,527		

NOTE: */**/* Indicates significance at the 1/5/10 percent levels, respectively (*t*-tests are two-tailed). *F*-statistics and *t*-statistics are Newey and West (1987) corrected. Federal funds target is not included in the MMS survey. *F* statistic (1): all MMS survey variables and federal funds target; *F*-statistic (2): all MMS survey variables. The number of nonzero observations indicates the number of trading days where there was a surprise in a macroeconomic data release or a monetary policy action priced in the market.**

Table 4

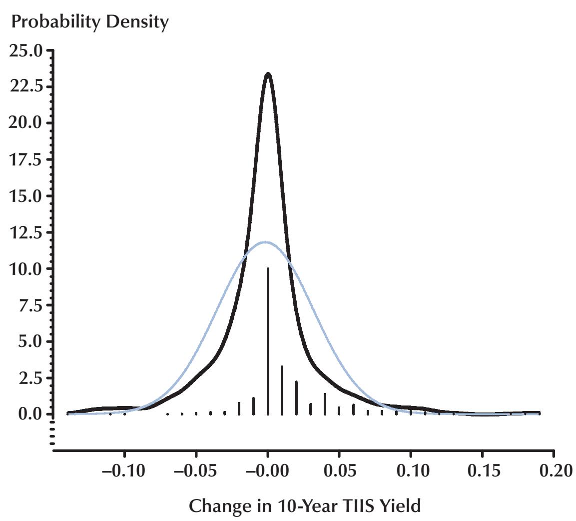
Instrumental-Variables Approach

Explanatory variable	Coefficient	*t*-Statistic	Bootstrap
Federal funds target (GSS)	$1.281 \cdot 10^{-1}$	1.517	Not significant
Federal funds target (PR)	$1.185 \cdot 10^{-1}$	1.538	Not significant

NOTE: Neither regression coefficient is statistically significant (*t*-tests are two-tailed; *t*-statistics are Newey and West (1987) corrected). GSS and PR indicate the federal funds market measure for monetary policy surprises as suggested by Gürkaynak, Sack, and Swanson (2002) and Poole and Rasche (2000), respectively.

Figure 1

Distribution of Daily Changes in the Real Long-Term Interest Rate



than-expected economic growth raises the real rate of interest. Recall that the announcement surprises have been normalized by the sample standard deviation of the individual forecasts (measured over the entire sample). If stronger-than-expected economic growth arises from a productivity shock, then that growth raises the desired capital stock; the real rate of interest must rise to restore the goods-market equilibrium. Indeed, our results show that we can reject the null hypothesis that surprises in productivity growth have no impact on the real long-term rate of interest.

Two other potentially interesting results from Table 3 are worth mentioning. First, our results suggest that surprise changes in the federal budget deficit (surplus) have no discernable impact on market participants who buy and sell 10-year TIIS. Second, a surprise increase in inflation (growth of the GDP price index) is expected to raise the real long-term interest rate. The latter result is perhaps puzzling given that the long-term real interest rate is thought to be determined by real factors (capital formation, productivity growth, population, etc.). The R^2 in Table 3 is about 5 percent, which implies that surprises in macroeconomic announcements explain only 5 percent of the variation of the dependent variable around its mean, the remainder being noise. Overall, the results from Table 3 suggest that the real long-term interest rate can change in response to surprise increases in some macroeconomic data releases, but that other factors appear to be more economically significant.

The results in Table 3 also allow us to speculate about the hypothesized linkage between the effects of surprise changes in the federal funds target rate. We find that surprises in changes of the targeted federal funds rate have no discernable impact on the real rate of interest. This finding squares with Weiss (2006), who finds no measurable effect of the federal funds rate (as derived from futures contracts) on the 10-year TIIS yield. Poole, Rasche, and Thornton (2002) argue that monetary policy surprises as gauged by changes in federal funds futures prices are measured with error. This is because federal funds futures prices not only change in response to monetary policy actions, but also respond to other information

Table 5

Uncertainty about Real Interest Rates

Explanatory variable	Coefficient	*t*-Statistic	Bootstrap
Panel A: GSS measure of federal funds target surprises			
Federal Reserve communication	$-2.217 \cdot 10^{-4}$	–0.493	
Federal funds target	$4.209 \cdot 10^{-3}$	0.689	
Intercept indicator variable (D)	$-4.828 \cdot 10^{-4}$	–1.056	
Intercept	$1.243 \cdot 10^{-3}$	2.774***	**
Number of nonzero observations	180		
Number of observations	1,527		
Panel B: PR measure of federal funds target surprises			
Federal Reserve communication	$-1.139 \cdot 10^{-4}$	–0.265	
Federal funds target	$5.118 \cdot 10^{-3}$	0.653	
Intercept indicator variable (D)	$-3.759 \cdot 10^{-4}$	–0.859	
Intercept	$1.137 \cdot 10^{-3}$	2.657***	*
Number of nonzero observations	182		
Number of observations	1,527		

NOTE: ***/**/* Indicates significance at the 1/5/10 percent levels, respectively (*t*-tests are two-tailed). GSS and PR indicate the federal funds market measure for monetary policy surprises as suggested by Gürkaynak, Sack, and Swanson (2002) and Poole and Rasche (2000), respectively. The variable Federal Reserve Communication equals 1 on trading days on which the Chairman of the Federal Reserve's semiannual testimony to Congress (formerly known as Humphrey-Hawkins Testimony) or speeches and other testimonies of the Fed Chairman were priced in the market. The number of nonzero observations indicates the number of trading days where there was a surprise in a macroeconomic data release or a monetary policy action priced in the market.

pertinent to the future path of the federal funds rate. Because of the measurement error introduced by such ambient price changes of federal funds futures contracts, the regression coefficient of the federal funds target variable is biased toward 0. We account for this error-in-variable problem with an instrumental variables approach. We use as an instrument for the federal funds target an indicator equal to 1 if the federal funds target exceeds its median positive value, equal to –1 if it falls short of its median negative value, and equal to 0 otherwise.[11]

Table 4 shows the regression results of the instrumental variables approach applied to equation (1). We use two alternative definitions of the surprise component of monetary policy actions (the federal funds target variable). First, we provide results for the concept that we used above—the measure suggested by Gürkaynak, Sack, and Swanson (2003), which is denoted federal funds target (GSS) in the table. Second, we present results for the surprise measure devised by Poole and Rasche (2000); this measure is denoted federal funds target (PR) in the table. Unlike the GSS measure, which rests on the scaled price change of the current month's federal funds futures contract (unless the monetary policy surprise happens within the last seven days of the month), the PR measure always uses the price change of the next month's federal funds futures contract. For the GSS measure, the regression coefficient for the federal funds target variable is indeed larger (in absolute value) than it is without the error-in-variable correction (shown in Table 3) but remains statistically insignificant. But, for the PR measure, the regression coefficient for the federal funds target variable is smaller (in absolute value) than it is without the error-in-variable correction (not shown); it remains statistically insignificant as well.

[11]For details on this error-in-variable approach, see Greene (2003).

We have been unable to establish evidence that monetary policy actions of the Federal Reserve affect the real long-term rate of interest. But the Federal Reserve has another channel of influence—communication. As discussed above, the surprise component in Federal Reserve communication is next to impossible to ascertain. Yet, following Kohn and Sack (2003), we can analyze the effect of Federal Reserve communication on the (conditional) variance of the dependent variable; this variance may be viewed as a measure of uncertainty that surrounds the future path of real short-term interest rates. Note that, if Federal Reserve communication and surprises in monetary policy actions affect the uncertainty surrounding the real rate of interest, then the error term of the regression equation (1) is heteroskedastic; Rao's score test on heteroskedasticity indeed rejects the null hypothesis that there is no such heteroskedasticity.[12]

We study the impact of Federal Reserve communication and surprises in monetary policy action on real interest rate uncertainty by analyzing the squared residuals from regression equation (1)—as shown in Table 3—in an estimation approach suggested by Amemiya (1977, 1978). We regress these squared residuals on (i) the (absolute value of the) federal funds target variable, an indicator variable that is equal to 1 on days when Federal Reserve communication was priced in the market and 0 otherwise and (ii) the previously introduced intercept indicator variable (D). The regression results, presented in Table 5, indicate that neither Federal Reserve communication nor monetary policy surprises influence the conditional variance of the real rate of interest. Hence, we do not reject the hypothesis that neither surprises in Federal Reserve monetary policy action nor Federal Reserve communication affect the uncertainty surrounding the real long-term interest rate.

CONCLUSION

The results in this paper suggest that real interest rates—as measured by market yields on Treasury inflation-indexed securities—respond to surprise announcements of macroeconomic data. Our findings are consistent with economic theory, which suggests that stronger-than-expected growth, perhaps caused by surprises in productivity growth, affects the real long-term interest rate. In the case of nonfarm productivity growth, the greater the surprise in the released nonfarm productivity growth number, the greater the accompanying increase in the real long-term rate of interest. We found no evidence that surprise increases in the monthly federal budget deficit increase the real rate of interest. Further, we find no evidence supporting the proposition that Federal Reserve communication or surprises in monetary policy actions—as gauged by changes in the targeted federal funds rate—influence the expected value or variance of the real long-term interest rate.

[12] For Rao's score test, see Amemiya (1985).

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Using Cyclical Regimes of Output Growth to Predict Jobless Recoveries

Michael J. Dueker

Gaps between output and employment growth are often attributed to transitional phases by which the economy adjusts to shifts in the rate of trend productivity growth. Nevertheless, cyclical factors can also drive a wedge between output and employment growth. This article shows that one measure of cyclical dynamics—the expected output loss associated with a recession—helps predict the gap between output and employment growth in the coming four quarters. This measure of the output loss associated with a recession can take unexpected twists and turns as the recovery unfolds. The empirical results in this paper support the proposition that a weaker-than-expected rebound in the economy can partially mute employment growth for a time relative to output growth.

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"In like a lamb, out like a lamb" is a common refrain one hears about business recessions. The assertion is that the robustness of the recovery is proportional to the severity of the contraction phase of a cyclical downturn (Wynne and Balke, 1992). If this is true, a so-called jobless recovery might be considered part and parcel of a mild recession. But, beware the Ides of March because this argument misses the point that a jobless recovery is a big event—the cost of a business cycle downturn rises substantially if the economy does not enjoy a snap-back phase of above-trend growth following a contraction in output.

The degree to which people expect that a contraction in output will not be undone in the future can be measured as the expected output loss associated with an economic downturn. In this article, I develop a measure of expected output loss from recession. In some recessions, the timing and relative magnitude of expected output loss closely mirrors the widely used Hodrick-Prescott measure of the output gap. In the recoveries from the past two recessions—both of which were labeled jobless recoveries—the expected permanent output loss looks much worse than the output gap, in terms of both magnitude and duration. This article demonstrates that one can use this output loss measure as a predictor of the extent to which output growth will outpace employment growth at least six months ahead.

This article provides empirical evidence that cyclical forces significantly influence the gap between output and employment growth, in addition to the effects that shifts in trend productivity generate. Wen (2005) uses a rational expectations model to show that firms optimally hoard labor in anticipation of stronger demand for their goods. If the economy's rebound from a recession is weaker than anticipated, firms might find that they had been holding too much labor, resulting in a period of muted employment growth.

MEASURING EXPECTED OUTPUT LOSS FROM A RECESSION

A key part of this article's perspective on the consequences of recessions is that the snap-back,

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high-growth phase the economy experiences following a recession has a random duration. In many recessions, the snap-back phase lasts long enough such that the output loss associated with the recession is 1 percent or less. From this vantage point, a so-called jobless recovery occurs when the snap-back phase lasts an unexpectedly short time or is skipped altogether. In general, labor productivity continues its upward trend during the recession, so by the end of the recession the effective, productivity-adjusted labor input is high relative to output. This ratio can return to its equilibrium value either through above-normal growth in output or below-normal growth in employment. In a jobless recovery, the latter predominates, although the reasons for this outcome are not always clear.

This article assumes that each recovery from a recession is the result of stochastic transitions between output growth states. Simply not enough data exist at this point to parameterize these transitions as functions of novel labor market patterns. Gordon (1993) and Schreft and Singh (2003, p. 65), in contrast, offer a structural change perspective on jobless recoveries. The latter authors posit that changes in the labor market may contribute to a greater tendency toward jobless recoveries going forward. They suggest that "just-in-time employment lets firms wait to see that a recovery is robust before hiring, yet still expand production on short notice by hiring temps and using overtime." Aaronson, Rissman, and Sullivan (2004) concur that just-in-time hiring practices played an important role in the recovery from the 2001 recession.

To build an empirical model of jobless recoveries, I use a model of output growth in which the expected output loss associated with a recession could undergo sizable changes between the start and the end of the recovery.[1] To do this, I estimate a four-state Markov switching model, with four distinct growth states for real gross domestic product (GDP). Real GDP growth is denoted y and the growth states are μ_i:

$$
\begin{aligned}
&y_t = \mu_{S_t} + e_t \\
&e_t \sim N\left(0,\sigma^2\right) \\
&S_t = i, i = 1,\ldots,4 \\
&\mu_1 < \mu_2 < \mu_3 < \mu_4 .
\end{aligned} \tag{1}
$$

In this set-up, the fourth state has the highest growth rate and, therefore, will represent the snap-back growth. A jobless recovery will be one where state 4 is either skipped or is shorter than in other recoveries.

Related Empirical Models of Asymmetric Cycles

The four-state Markov switching model of GDP growth falls within a large class of models of asymmetric business cycles. For a model of output growth, asymmetry implies that the fluctuations above and below the unconditional mean growth rate are not mirror images. Sichel (1993) described particular attributes that the asymmetry might have, including asymmetries in steepness and deepness. McQueen and Thorley (1993) added asymmetry in sharpness. Clements and Krolzig (2003) noted that a two-state Markov switching model cannot imply an asymmetry in steepness. The four-state Markov switching model will generally display all three types of asymmetry.

Sichel (1994) suggested that the rebuilding of inventories implied three states in U.S. economic activity: normal growth, recession, and a snap-back phase of high growth following a recession, as inventories were restocked. Kim, Morley, and Piger (2005) effectively add to a two-state Markov switching model a third state whose timing and length are deterministic functions of the preceding recession state. Van Dijk and Franses (1999) similarly extend two-state threshold autoregressive models so that they have multiple regimes but in a framework where predetermined transition variables determine the regime, leaving no room for contemporaneous surprises regarding the regime. This framework, however, does not reflect the public perception that jobless recoveries are unpleasant surprises. For this reason, I use a four-state Markov switching model where all transitions between regimes are stochastic.

[1] Engemann and Owyang (2006) also present an empirical model of jobless recoveries.

Estimates of the Four-State Markov Switching Model

The transition probabilities for the Markov states are

$$(2)\quad Prob(S_t = i \mid S_{t-1} = j) = p_{ij,} i = 1,\ldots 4; j = 1,\ldots,4.$$

This leads to a matrix of transition probabilities that enter the likelihood function, l_t, which is expressed as a prediction-error decomposition:

$$(3)\quad \sum_t l_t = \sum_t ln\left(\sum_i Prob(S_t = i \mid y_{t-1}) f(y_t \mid S_t = i)\right).$$

The results of estimating the model for quarterly U.S. chain-weighted real GDP growth from 1958:Q1 to 2005:Q3 are shown below. The estimated growth states (expressed as annual rates) and their unconditional probabilities are as follows:

$$\mu_i, i = 1,\ldots,4 = \begin{pmatrix} -4.0\% & 0.072 & State\,1: \ recession \\ 1.7\% & 0.366 & State\,2: slow\ growth \\ 3.6\% & 0.326 & State\,3: ordinary\ growth \\ 7.6\% & 0.236 & State\,4: snap\text{-}back\ growth \end{pmatrix}.$$

Figure 1 plots GDP growth against the probability-weighted fitted value, using the smoothed-state probabilities. It is remarkable that, using either the filtered or smoothed probabilities, the weighted average of the four growth states explains enough of the dynamics in GDP growth that the residuals show no significant serial correlation. In fact, after 1994 the degree of serial correlation in the residuals is even lower than for the full sample, despite the model finding fewer transitions in the growth states. In contrast, in a two-state model—e.g., Hamilton (1989)—it is necessary to model GDP growth as an autoregressive process to remove the serial correlation. Figure 2 shows the smoothed probabilities of the snap-back, high-growth state. The recoveries from the 1990-91 and 2001 recessions were the times when this snap-back phase was largely absent.

The transition probability matrix has entries such that $Prob(S_t = i \mid S_{t-1} = j) = p_{ij}$ appear in row i and column j:

$$\begin{pmatrix} 0.267300 & 0.133134 & 9.18590e-08 & 0.0163723 \\ 0.607963 & 0.612537 & 0.0533640 & 0.342089 \\ 0.0156676 & 0.0249191 & 0.946636 & 0.0302375 \\ 0.109070 & 0.229411 & 2.22045e-16 & 0.611301 \end{pmatrix}.$$

Note that standard errors are not reported for these maximum-likelihood parameter estimates because the estimated information matrix is not positive definite, given that several transition probabilities lie near the boundary of the parameter space, i.e., zero.

The transition probability matrix shows that the probability that the economy will shift from either the recession state or the low-growth state straight to the ordinary growth state without passing through the fast, snap-back growth state 4 is low ($p_{41} = 0.109$). Also, there is a good chance that the economy could bounce back and forth more than once between the low-growth state 2 and the snap-back growth state 4 before entering the relatively persistent ordinary growth state 3 ($p_{21} = 0.608$ and $p_{12} = 0.133$). It is quite likely, according to this transition matrix, that the economy will spend a nontrivial period of time in the snap-back growth state following a recession. In fact, the unconditional probability of the economy being in the snap-back growth state is almost 24 percent. Based on this model, one would expect that much of the output loss from a recession would be undone by the snap-back state. The fact that there is no transition from the persistent ordinary growth state 3 to the snap-back growth state 4 (p_{43} is essentially zero) means that a recovery will remain "jobless" if the ordinary growth state takes hold before much snap-back growth has taken place.

Given the accrued output loss to date from a recession and the filtered probabilities of the current state, one can use this Markov switching model to calculate, at each quarter following the onset of the recession, an expected value of the output loss associated with that recession. Figure 3 illustrates a hypothetical example, based on the parameter estimates from the four-state Markov switching model. In this example, we calculate the expected output loss from a recession that started four quarters ago. In the four quarters that have already ensued, the first quarter was in the

Figure 1

GDP Growth and Fitted Value from Four-State Markov Model Using Smoothed State Probabilities

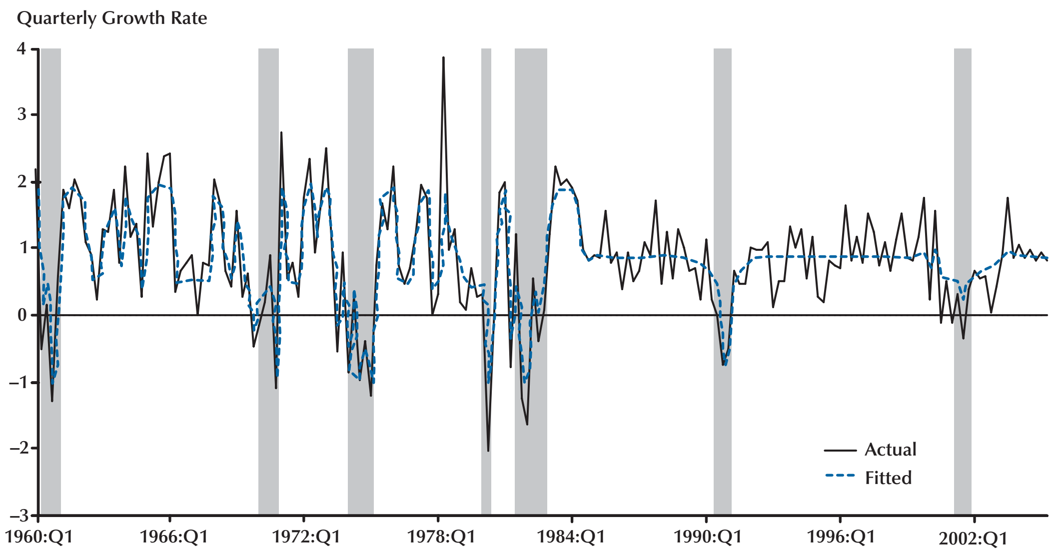


Figure 2

Smoothed Probability of Fast-Growth State 4

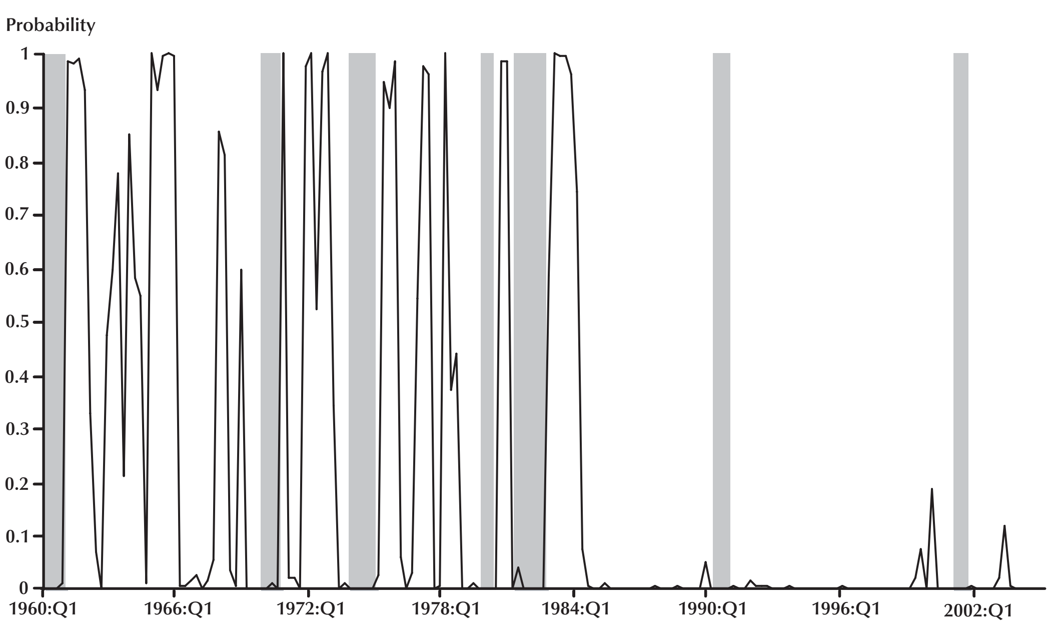
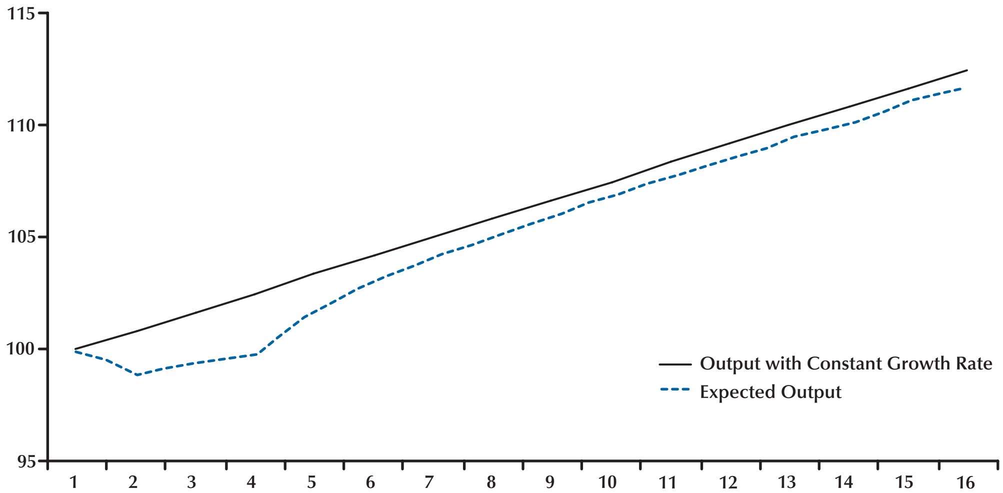


Figure 3

Path of Expected Output Four Quarters After the Onset of a Hypothetical Recession



recessionary state 1, the next two were in the slow-growth state 2, and the fourth was in the snap-back state 4. From that point, the model is simulated many times (4,000) to arrive at an expected path for the level of output. I compare this path with a reference path in which output grew at a constant rate equal to the model-implied unconditional growth rate (0.83 percent per quarter) for the entire time. Provided that the length of the simulated path is long enough so that the probabilities of the four growth states converge to their unconditional probabilities and the implied growth rate becomes fixed at its unconditional value (0.83), the ending point of the simulation will provide a measure of the long-run or "permanent" output loss associated with the recession. With the transition probabilities estimated here, a simulation length of 40 quarters is more than sufficient to converge to the unconditional probabilities. Thus, in this example plotted in Figure 3, the expected long-run output loss from the hypothetical recession is about 0.7 percent, with the expectation taken four quarters after the onset of the hypothetical recession.

Using the unconditional, constant-growth path described above as a reference path, we can calculate this measure of expected long-run output loss at each quarter following the onset of each recession in our sample. Throughout the recession itself, the expected output loss will become larger because, for every quarter that the economy remains in recession, the probability that the economy will remain in recession for an above-average length of time increases. As with any duration, the right tail of the distribution is inevitably longer than the left tail, since there is no way that today's situation can last for an arbitrarily shorter-than-expected time, but it can last for an arbitrarily longer-than-expected time. As the economy begins to recover from the recession, however, the expected output loss associated with the recession recedes in accordance with the number of quarters spent in the snap-back growth state.

Figure 4 plots the average across all U.S. recessions since 1960 of the expected long-run output loss as a function of quarters since the onset of recession. Across all recessions, the snap-back growth state occurs for enough quarters to undo most of the output loss associated with the preceding recession. Across all recessions, after 18 quarters, the expected long-run output loss is about 1 percent. In this light, we can see why the past two jobless recoveries—following the 1990-91 and the 2001 recessions—disappointed the public.

Figure 4

Expected Effect on Output from Recession, Calculated at Each Quarter from Recession Onset

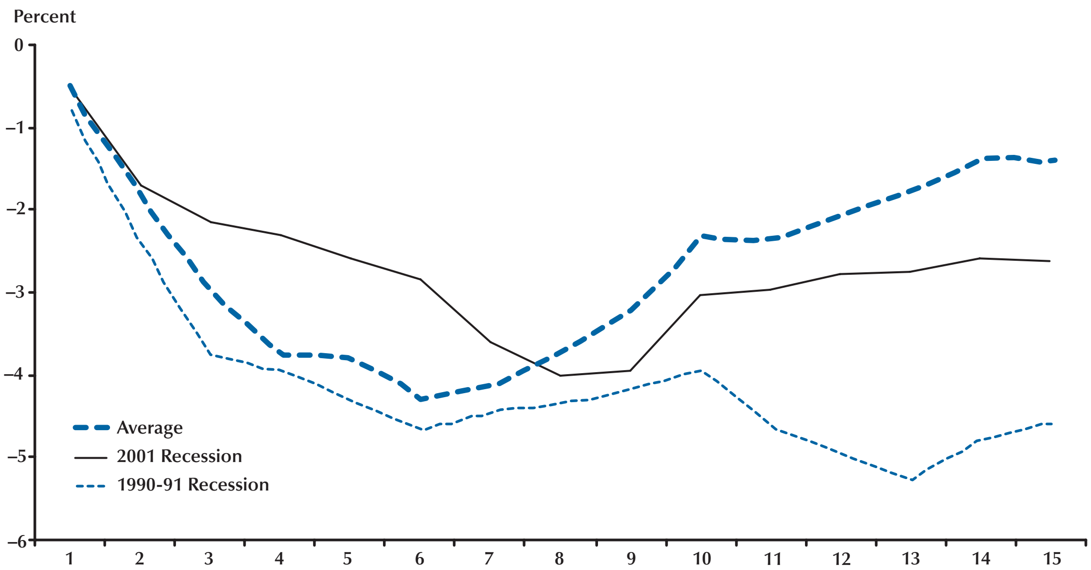
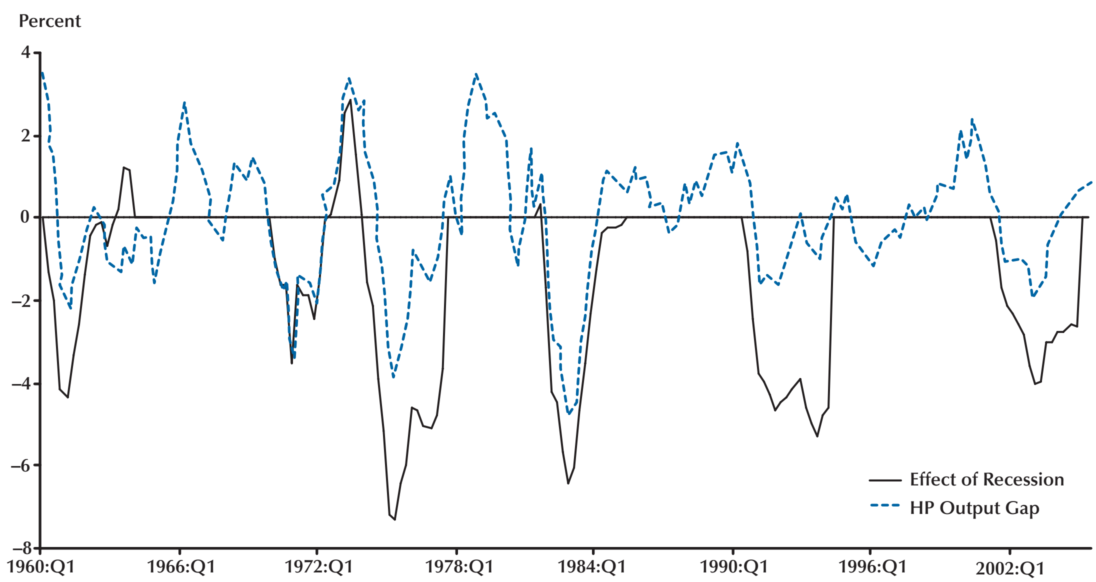


Figure 5

Expected Long-Run Effect on Output from Recession and HP Output Gap



Since neither recession lasted an unusually long time, the path of expected output loss was fairly typical, or even slightly milder than normal, during the actual recessions. Yet, the failure to spend a considerable length of time in the snap-back phase following the recession—especially in the aftermath of the 1990-91 recession—meant that the usual diminution of expected output loss failed to materialize. Figure 4 shows that the expected output losses associated with the 1990-91 and 2001 recessions were well above the typical 1 percent—4.5 and 2.6 percent, respectively—15 quarters after recession onset.

Figure 5 plots the expected output loss following each recession since 1960 alongside the widely used Hodrick-Prescott measure of the output gap. Note that the short 1980 recession, in which the recovery melded with the start of another recession in 1981, is excluded from the output loss calculations. Also, the expected output loss converges to a constant after about 15 quarters. At that point, the output loss from the recession essentially has been realized and is no longer an expected value. Nor are expectations of the future related to the value of the output loss measure at this point, so those observations beyond 15 quarters are dummied out of the expected output loss measure in Figure 5.

Figure 5 shows that the correspondence between the expected output loss and the Hodrick-Prescott output gap is fairly close for the 1960-61, 1969-70, and 1981-82 recessions. In contrast, the expected output loss measure makes the 1974-75, 1990-91, and 2001 recessions look worse than the output gap does. In the case of the 1974-75 recession, the economy did spend time in the snap-back growth state, as seen in Figure 1. It did not spend enough time in that state, however, to overcome the large output loss accrued during the recession. Figure 1 shows a contrasting picture for the jobless recoveries after the 1990-91 and 2001 recessions, when the economy spent minimal time in the snap-back growth state. For this reason, the output loss from the past two recessions was quite large in relation to the maximum size that the output gap attained.

PREDICTING CYCLICAL GAPS BETWEEN OUTPUT AND EMPLOYMENT GROWTH

One well-known disadvantage of Hodrick-Prescott filtering is that the two-sided nature of the filter makes filtered data inappropriate for use in prediction models. The filtered value at time t is a function of future values of the data. The expected output loss measure, in contrast, was constructed from unsmoothed regime probabilities, using information only through time t. Thus, the only sense in which the expected output loss measure is constructed from future information is through the full-sample parameter estimates. This parameter channel, however, is a very weak source of future information in comparison with a two-sided filter. Consequently, I examine how well the expected output loss measure can be used to predict the effect that business cycle dynamics will have on the gap between output and employment growth.

To test the importance of such a cyclical channel in the determination of the gap between output and employment growth, I regressed the gap between quarterly GDP growth and employment growth (the log change in aggregate payroll employment):

$$\Delta y_t - \Delta n_t = \mu + \gamma_i ELoss_{t-1} + \varepsilon_t, \quad (4)$$

where y is the log of GDP, n is the log of employment, and *ELoss* is the expected output loss from recession. The coefficient on γ_i is significant for lag lengths from $i = 1$ through 4 quarters. Table 1 presents the estimates of γ_i, $i = 1,\ldots,4$ and shows that the expected output loss is a significant predictor of the gap between output and employment growth at each horizon up to four quarters. I then estimated the same equation for the four-quarter moving average of the gap, allowing for three moving-average terms to account for the serial correlation induced by the overlapping data. This specification answers the question of whether the expected output loss is a significant predictor of the gap between output and employment growth in the coming year:

Figure 6

Expected Long-Run Effect on Output from Recession (Lagged One Year) and Moving Average of Gap Between Output and Employment Growth

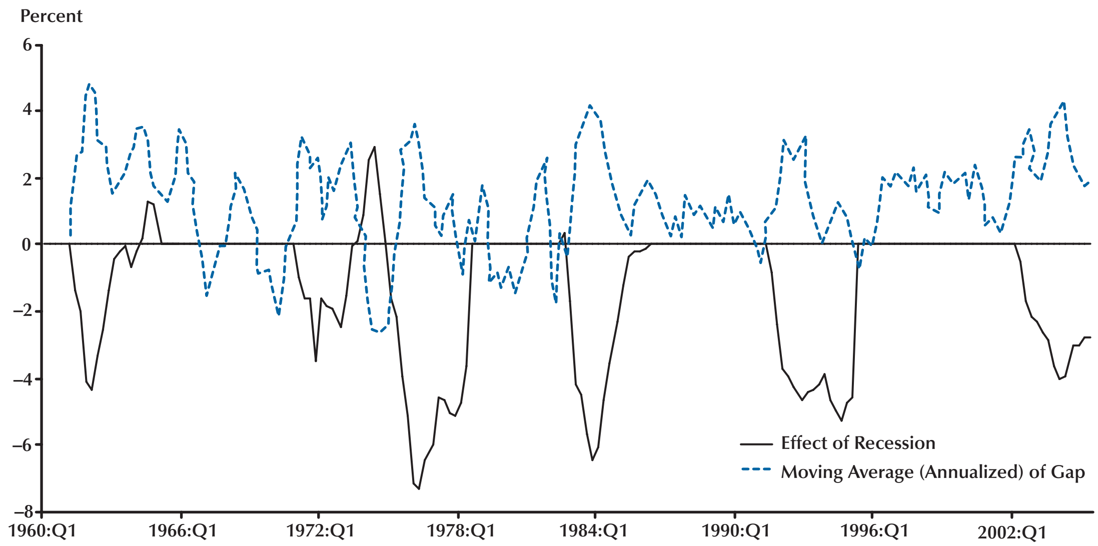


Table 1

Using Expected Output Loss from Recession to Explain the Gap Between Output and Employment Growth

Parameter	Value	Standard error
Period-by-period specifications—equation (4)		
γ_1	–0.100	(0.023)
γ_2	–0.085	(0.023)
γ_3	–0.071	(0.023)
γ_4	–0.054	(0.023)
Moving-average specification—equation (5)		
Γ	–0.038	(0.012)
θ_1	0.998	(0.043)
θ_2	0.933	(0.049)
θ_3	0.851	(0.041)

$$1/4 \sum_{j=0}^{3} \left[\Delta y_{t-j} - \Delta n_{t-j} \right] = \mu + \Gamma ELoss_{t-4} + \sum_{k=1}^{3} \theta_k \varepsilon_{t-k} + \nu_t. \tag{5}$$

Not surprisingly, given the period-by-period results, the estimated value of Γ is also a significant predictor of the gap between output and employment growth in the following year. Table 1 includes the estimates of Γ and the moving-average coefficients $\theta_k, k = 1,\ldots,3$.

The reason for the significant Γ coefficient in the moving-average specification from equation (6) becomes clear in a plot of the expected output loss from recession with the moving average of the gap between output and employment growth in the subsequent four quarters. Figure 6 plots these two variables together. Figure 6 shows the tendency for one to be the negative image of the other, and this relationship has held throughout the sample, not only for the two most recent jobless recoveries.

SUMMARY AND CONCLUSION

This article uses a four-state Markov switching model of U.S. GDP growth to derive a novel measure of the time path of expected output loss associated with each recession since 1960. A key feature that distinguishes this model of snap-back growth is that the occurrence and length of the snap-back state are allowed to be random. Thus, the expected output loss from a recession is still evolving after the recession has ended, in accordance with the strength of the recovery. One key feature of the estimated Markov model is that once the economy enters the ordinary growth state, it cannot return directly to the snap-back state. Thus, once a strong recovery has been skipped or has ended early, the expected output loss from the preceding recession is essentially known, and not just an expected value, at that point.

For many recessions, especially 1960, 1969, and 1981, the expected long-run output loss measure corresponds closely with the Hodrick-Prescott measure of the output gap. For the recessions where the long-run output loss was larger than average, such as 1974, 1990, and 2001, the expected output loss measure makes those downturns look more severe in comparison with the output gap measure. The constructed measure of the expected output loss associated with a recession is a significant predictor of the gap between output and employment growth in the coming four quarters, which could help policymakers identify jobless recoveries as they unfold.

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