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THE RYBCZYNSKI THEOREM, FACTOR-PRICE EQUALIZATION, AND IMMIGRATION: EVIDENCE FROM U.S. STATES

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ABSTRACT

Recent literature on the labor-market effects of U.S. immigration tends to find little correlation between regional immigrant inflows and changes in relative regional wages. In this paper we examine whether immigration, or endowment shocks more generally, altered U.S. regional output mixes as predicted by the Rybczynski Theorem of Heckscher-Ohlin (HO) trade theory. This theorem describes how regions can absorb endowment shocks via changes in output mix without any changes in relative regional factor prices. Treating U.S. states as HO regions, we search for evidence of regional output-mix effects using a new data set that combines state endowments, outputs, and employment in 1980 and 1990. We have two main findings. First, state output-mix changes broadly match state endowment changes. Second, variation in state unit factor requirements is consistent with relative factor-price equalization (FPE) across states, which is a sufficient condition for our output-mix hypothesis to hold. Overall, these findings suggest that states absorb regional endowment shocks through mechanisms other than changes in relative regional factor prices.

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1 Introduction

In recent decades rising immigration into the United States has steadily increased the share of immigrants in the total population. Borjas, Freeman, and Katz (1997) report that this share rose from 4.8% in 1970 to 6.2% in 1980 and to 7.9% in 1990. Recent immigrants tend to have much lower education levels that the typical U.S. worker (Borjas, 1994) and tend concentrate in states with relatively large populations of previous immigrants, such as California, Florida, New York, and Texas. A vast literature examines whether the U.S. regions that have had relatively large influxes of low-skilled immigrants have also had relatively low wage growth for low-skilled U.S. native workers. The near uniform finding is that immigration has, at most, a very small negative effect on native wages: there is a near zero correlation between regional immigrant inflows and changes in relative regional wages (see surveys in Borjas, 1994 and Friedberg and Hunt, 1995).

In this paper we examine whether U.S. regions have absorbed immigrant inflows (or shocks to endowments more generally) by altering the mix of goods they produce, thus relieving pressure for wages to change. The focus on output mix is motivated by the Rybczynski Theorem (1955), a core result of Heckscher-Ohlin (HO) trade theory. This theorem states that when a region is open to trade with other regions, changes in regional relative factor supplies can be fully accommodated by changes in regional outputs without requiring changes in regional factor prices. An increase in the relative endowment of a factor increases the output of products which employ that factor relatively intensively and decreases the output of at least some other products. This shift in output mix increases the regional relative demand for the factor whose endowment has increased, thereby matching the increase in its regional relative supply and eliminating pressure on factor prices to change. Trade is essential for this mechanism to work, as regional output changes are accommodated by corresponding changes in regional exports and imports. So long as the region is sufficiently small, these output and trade-flow changes do not affect world prices and thus do not trigger Stolper-Samuelson (1941) factor-price effects.

Our approach is to treat U.S. states as Heckscher-Ohlin regions and to examine changes over time in state factor endowments, output mix, and factor usage. The focus on output mix and factor usage distinguishes our work from the previous literature which concentrates on cross-region variation in wages. To think of a concrete example, over the last two decades many low-skilled immigrants settled in California. During this period, California expanded production and exports of nonskill-intensive goods, such as apparel, canned food products, and toys. California's shift towards these sectors may have helped accommodate its immigrant influx, partially or entirely obviating the need for California's factor prices to change relative to the rest of the country. We examine the plausibility of this story for California and other big states.

Changes in output mix are by no means the only mechanism through which U.S. states could accommodate immigrant inflows without changes in relative regional factor prices. An obvious alternative adjustment mechanism is regional migration of labor or capital. Native U.S. workers may have left (or slowed down their migration to) states where immigrants have concentrated. To the extent that these regional migrations offset each other, net changes in state relative endowments may have been very small, requiring minimal changes in state output mixes or factor prices. The literature is divided about whether immigrant inflows contribute to native outmigration. Filer (1992) and Borjas, Freeman, and Katz (1997) find evidence that they do, while Card (1997) finds evidence that they do not.

We address this issue by focusing on *total* state labor endowments, rather than on the separate stocks of native and foreign workers. We assume that within each education category native and foreign workers are perfect substitutes, and then examine whether state output-mix changes are sufficient to accommodate the total change in state labor endowments. The focus on net endowment changes, rather than on net immigrant inflows, is one contribution of the paper.

For our empirical analysis we construct a new data set combining real state value added by industry and state labor employment by industry for four education categories: high-school dropouts, high-school graduates, those with some college, and college graduates and beyond. The data cover a subsample of 15 large U.S. states and 40 sectors, spanning all civilian industries, in 1980 and 1990. In much of our analysis we focus on the "gateway" immigrant states of California, Florida, Illinois, New Jersey, New York, and Texas. In 1960 60% of all U.S.

immigrants lived in one of these six states; by 1990 that share had risen to 75% (Borjas, et al 1997). In 1992 60% of all U.S. legal immigrants came into California or New York alone, while another 20% entered the other four gateway states (Borjas, et al 1996).

We present two kinds of evidence on the output-mix hypothesis. Our first approach is to analyze changes from 1980 to 1990 in state endowment mixes and state output mixes to see whether state output growth was relatively high (low) in sectors that were intensive in the use of factors whose relative supplies were expanding (declining). This attempt to find "direct" evidence for the output-mix hypothesis is complicated by the fact that during our sample period there likely were many shocks to preferences and technology, independent of immigration-related endowment shocks. For example, in the 1980s there was a sharp increase in the relative demand for skilled workers, which many authors attribute to skill-biased technological change (SBTC) (Bound and Johnson, 1992; Katz and Murphy, 1992; Berman, Bound, and Griliches, 1994). The *ex ante* likelihood that factor prices and output mixes have changed for reasons other than immigration makes it impossible to test the simple textbook version of the Rybczynski Theorem, where the only exogenous shock is to endowments. Accordingly, when we decompose how states absorb endowment shocks we attempt to control for national shocks, such as SBTC.

Our second approach to testing the output-mix hypothesis is to test for factor-price equalization (FPE) across U.S. states.[1] A sufficient condition for our output-mix hypothesis, in which relative regional wages are insensitive to regional relative factor-supply changes, is that *relative* FPE holds across U.S. states – i.e., that factor prices for productivity-equivalent units are equalized across states. Relative FPE would be consistent, for instance, with Hicks neutral technology differences among states (Trefler, 1993). A sufficient condition for relative FPE between two states is that for each factor in each industry the two states have the same unit factor requirements, up to some scalar which is constant across industries. We test for relative FPE by

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[1] The FPE theorem, another core result of HO trade theory, is due to Samuelson (1948). It is usually expressed in terms of absolute FPE in which wages are exactly the same for each factor in each region. See Blackorby, Schworm, and Venables (1993) on necessary and sufficient conditions for FPE.

comparing industry unit factor requirements across states. Relative FPE would imply that the related states all occupy the same cone of diversification, and thus experience common relative-wage responses, if any, to an endowment shock in any one state.[2] In this way, evidence of relative FPE is "indirect" support for the output-mix hypothesis.

To preview our results, we find support for the hypothesis that states have absorbed endowment changes without changes in relative factor prices. First, we find evidence that state output-mix changes broadly match state endowment-mix changes. States whose endowment mix changed in line with the national endowment mix had output-mix changes in line with national output-mix changes. In contrast, states where immigration helped alter the endowment mix had output-mix changes reflecting the endowment shock. Second, we find that variation in unit factor requirements across states is consistent with relative FPE. Using regression analysis, we retain the null hypothesis of relative FPE between individual states and a control group of states for the large majority of cases in our sample. This finding suggests that U.S. states accommodate state-specific endowment changes without state-specific factor-price changes.

Our research is related to two bodies of literature. The first, mentioned above, is that on immigration and wages in the United States. Why immigration has had minimal impact on the wages of U.S. workers remains a puzzle. Borjas, Freeman, and Katz (1997) comment that local output-mix changes are one potential explanation for the insensitivity of wages to immigration, but we are aware of no study before ours which analyzes this mechanism in detail. Our research is also related to empirical tests of HO trade theory. Harrigan (1995, 1997) and Bernstein and Weinstein (1998) examine whether national outputs vary systematically with national factor endowments, as predicted by the HO model. Davis, et al (1997) and Maskus and Webster (1999)

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[2] With relative FPE there are no state-specific wage responses to moderate state-specific endowment shocks. An endowment shock to any one state triggers an output-mix response in that state. If that state is small, this response does not affect world product prices and thus does not induce any Stolper-Samuelson (1941) wage effects. If that state is big, in contrast, world product prices do change with the output-mix change. This triggers Stolper-Samuelson wage changes in the state with the original shock. But it also triggers the same Stolper-Samuelson wage changes in all states with which it has relative FPE and thus shares the same cone of diversification. In either case, with relative FPE there are no *state-specific* wage responses to moderate state-specific endowment shocks. The qualifier "moderate" highlights the fact that sufficiently large endowment shocks alter the set of goods produced, and thus factor prices, in the affected state.

develop tests of FPE to indirectly test the HO model. The former find evidence consistent with FPE across Japanese regions, but not across OECD countries. This methodology is also applied in Davis and Weinstein (1998), with more favorable results for the HO model. Our work highlights a limitation of this methodology, and we extend it to develop a sharper test of FPE.

There are four additional sections to this paper. Section 2 examines state endowment-mix changes and their link to state output-mix changes. Section 3 formalizes these results by using an accounting decomposition derived from the production side of HO trade theory. Section 4 presents regression evidence on relative FPE among U.S. states. Finally, section 5 concludes.

2 State Endowment Mixes and State Output Mixes: Summary Calculations

This section examines changes in state labor endowments and output mixes. First, we show that during the 1980s endowment changes varied across U.S. states. Second, we document that states also had different output-mix changes: states tended to expand in sectors that were intensive in the use of growing factors.

To construct state labor endowments (for both native and foreign workers), we use data from the 5% Public Use Microsample (PUMS) of the *U.S. Census of Population and Housing*. An individual is included as part of the state labor endowment if he or she is a member of the state labor force. Later in the analysis, we will require measures of industry employment and output by state. To construct the former, we combine PUMS data with industry employment data from the U.S. Bureau of Economic Analysis (BEA). Data on real industry value added at the state level also come from the BEA. To match industries from these two data sources we aggregate all civilian industries into 40 sectors, which are a mix of one-digit and two-digit industry classifications. The Data Appendix describes data sources and variable construction.

We examine four education categories of labor. While it would be desirable to also examine non-labor factors, such as capital and land, there are no industry data on state employment of these factors. Within education categories, we aggregate over foreign and native workers, which is appropriate given that changes in output mix depend on changes in *total* factor endowments.

We thus implicitly assume that within each educational category native- and foreign-born workers are perfect substitutes.[3] If changes in output mix are sufficient to absorb changes in total factor endowments, then by implication changes in output mix can also account for the specific component of changes in factor endowments due to immigration.

2a Labor Endowments across U.S. States, 1980-1990

Tables 1a and 1b present data on labor endowments for 12 states plus the overall United States in 1980 and 1990. In addition to the six immigration gateway states (California, Florida, Illinois, New Jersey, New York, Texas), we include data on six other large states in the northeast (Massachusetts), midwest (Ohio, Michigan), south (Georgia, North Carolina), and west (Washington).[4] Each row of Table 1a reports the share of the total state (or national) labor force accounted for by each of the four labor categories; Table 1b reports changes in these shares.[5]

Table 1a shows that states differ widely in the distribution of the labor force across education categories. Relative to the United States as a whole, the labor force in northeastern states (MA, NJ, NY) is skewed towards college graduates, the labor force in midwestern states (OH, IL, MI) is relatively concentrated among high-school graduates, and the labor force in southern states (FL, GA, NC, TX) is relatively concentrated among high-school dropouts. California is distinct in that by 1990 its labor force is concentrated in the extremes of the skill distribution, with relatively high endowment shares for both high-school dropouts and college graduates.

Table 1b shows, consistent with previous findings, that during the 1980s there was a national increase in the relative supply of more-educated workers (Bound and Johnson, 1992; Juhn, Murphy, and Pierce, 1993; Katz and Murphy, 1992). For the United States as a whole, the endowment shares for those with a high-school education or less declined while the endowment

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[3] Illegal immigrants are included in our data, to the extent they are enumerated in the *Census of Population and Housing* and work for establishments that are surveyed by the BEA. Given obvious data constraints, we make no attempt to distinguish between legal and illegal immigrants.

[4] We select large states to guarantee sufficiently large sample sizes of workers by education category at the state and industry level in the PUMS data (see note 11).

[5] Results using the working age population, instead of the total labor force, are similar to those reported in Tables 1-3.

shares for those with more than a high-school education rose. Interestingly, this shift varies markedly across states. The increase in the endowment share of college graduates was highest in northeastern states. In the midwest, changes in endowment shares generally mirrored those in the rest of the country, though the region did show a relatively large increase in the share of those with some college. In the south, there was a relatively large shift away from high-school dropouts in Georgia and North Carolina, but not in Florida. In the west, and particularly in California, there was a relatively small shift away from high-school dropouts and a relatively large shift away from high-school graduates.

Table 2, which shows the share of individuals in each labor category who are foreign born in 1980 and 1990, provides further insight into state endowment shifts. The gateway states for immigration are immediately apparent. California, Florida, New Jersey, and New York have relatively high immigrant shares in all education categories, with California being the clear outlier among these. Illinois and Texas (and also Massachusetts) have high concentrations of immigrants among high-school dropouts, but not among other labor categories. Immigrant concentrations are much lower in the other states in the midwest, south, and west. In most states, immigrant shares rose markedly in each education category during the 1980s.

Comparing Tables 1 and 2, an interesting pattern becomes apparent. While over the 1980s the gateway states have high and rising immigrant shares, particularly in the lowest education categories, all of these states except California still had a moderate to large decline in the relative supply of very low-skilled workers. In Florida, the relative supply of high-school dropouts declined, but less so than in the rest of the country. This implies that for many states a declining supply of low-skilled native workers offset immigrant inflows, due to some combination of native outmigration or labor-force exits.

Table 3, which shows the change in the shares of native-born and foreign-born individuals by education category in the total labor force, illustrates this pattern clearly. Despite rising immigrant shares among the low-skilled, the share of foreign-born high-school dropouts in the total labor force either is constant or declines in 9 of the 12 states. Only California, Florida, and

Texas show a substantial increase in the share of foreign-born high-school dropouts in the total labor force. For Florida and Texas, however, the decline in the native-born high-school dropouts far exceeds the increase foreign-born high-school dropouts. With the clear exception of California, shifts in the native-born labor force have mitigated the impact of immigration on state relative labor endowments in high-immigration states. This suggests one reason why immigrants may not have pressured native wages: native endowment patterns may have partially offset immigration flows, dampening the net change in regional relative labor endowments.

2b Changes in Output Mix for U.S. States, 1980-1990

According to our output-mix hypothesis, variation across states in endowment-mix changes should be systematically related with variation across states in output-mix changes. For changes in output mix to matter for how states absorb endowment shocks, industries must differ in the intensity with which they use different factors. Table 4 shows this to be the case. For each of the 40 industries, we list three measures of industry factor intensity: the ratios of employment of high-school dropouts, high-school graduates, or those with some college to employment of college graduates. All measures in Table 4 use data for national industry employment in 1980 and 1990 (see appendix).

There are substantial differences in factor intensity across industries. In 1990 for the least skill-intensive industries, the ratio of high-school dropouts to college graduates is 9.3 in household services, 7.0 in automotive repair services, and 6.4 in textiles; among the most skill-intensive industries these ratios are 0.05 in legal services, 0.07 in investment banking, and 0.11 in education services. Thus, while household service firms employ about 9 high-school dropouts per college graduate, law firms employ 20 college graduates per high-school dropout. Industries that are intensive in college graduates relative to high-school dropouts also tend to be intensive in college graduates relative to high-school graduates or those with some college. The ranking of factor intensities by industry is relatively similar across the three labor types. The rank correlations of industries by the different factor intensity measures in Table 4 lie between 0.67 and 0.93. Relative factor intensities are also quite stable over time and across states.

The within-industry decline in the relative employment of low-skilled workers, which has been documented extensively (Bound and Johnson, 1992; Katz and Murphy, 1992; Murphy, Juhn, and Pierce, 1993; Berman, Bound, and Griliches, 1994), is apparent in Table 4. There is a large decrease in the employment of high-school dropouts and high-school graduates relative to college graduates (and those with some college) over the 1980s. Interestingly, this decline is sharpest in some of the least skill-intensive sectors, such as apparel, leather, and household, personal, and lodging services. Combined with the well-documented rise in the wage premium to skilled workers, these relative-employment shifts suggest skill-biased technical change.

Table 5a presents initial evidence on the output-mix hypothesis. To see how industry output growth varies by industry factor intensities, for each state we calculate industry growth as

$$(1) \qquad z_m = \sum_{n=1}^{N} \Delta \ln(x_n)(\lambda_{mn} - \overline{\lambda}_n)$$

where Δ represents the time-difference operator, x_n is real value added in industry n, l_{mn} is the share of industry n in total state employment of labor type m, and $\overline{l}_n$ is the mean of the l_{mn} terms across the four labor types for industry n. l_{mn} measures the intensity of industry n in labor type m and $\overline{l}_n$ controls for the overall size (or average labor intensity) of industry n.[6] There are two ways of viewing z_m. One is as the growth in demand for labor type m, relative to the growth in demand for other labor types, implied by growth in industry value added. The other is as the

[6] This interpretation follows naturally from standard trade theory. To preview our discussion in Section 3, for a given region let **X** be the industry value-added vector, **V** be the factor-endowment vector, and **C** be the matrix of unit factor requirements. From equation (2), factor-market equilibrium implies **CX=V**. Suppose there is a small change in factor supplies, which, by Rybczynski logic, leaves factor prices unchanged. Using "hats" to indicate percentage changes, we can rewrite the factor-market clearing condition as, $\lambda\hat{\mathbf{X}} = \hat{\mathbf{V}}$, where $\boldsymbol{\lambda}=\mathbf{CXdiag(V)}^{-1}$ is the matrix of factor shares, which shows the share of each factor's total endowment that each industry uses in production. The **λ** matrix describes how factor-supply changes are translated into output-supply changes and is an obvious measure of industry factor intensity.

[7] Even if **C** were constant in our data, a complication with testing the Rybczynski Theorem is that there may be more goods than factors (N>M), in which case the supply of each individual good is indeterminate and there is no unique mapping from factor supplies to outputs. Ethier (1984) develops a method for testing the Rybczynski Theorem that is robust to output indeterminacy and Bernstein and Weinstein (1998) examine these issues using data for Japanese regions and OECD countries. To apply the Ethier methodology to our data, we would still need to treat the **C** matrix as constant over time, which is clearly unwarranted.

factor-share-weighted-average change in log value added, normalized by the overall employment-share-weighted-average change in log value added. By construction the z_m terms sum to zero across labor types for a given region. Thus, a positive (negative) entry indicates that a state's output growth was relatively concentrated (unconcentrated) in sectors that are intensive in the use of a given labor type. We calculate z_m for each labor type in each state using data on all 40 sectors. The change in log value added is over the period 1980 to 1990 and each l_{mn} term is averaged over 1980 and 1990. In Table 5a, each row corresponds to a different state and each column corresponds to a different labor type.

The key message of Table 5 is that changes in state output mixes are broadly consistent with Rybczynski-type effects from changes in state endowment mixes. In northeastern states, where relative endowments shifted towards college graduates, growth in real value added is highest in industries that are intensive in the use of college graduates and lowest in industries that are intensive in the use of high-school dropouts. The exception to this pattern is New York, which had the smallest relative decline in high-school dropouts in the region. In midwestern states value added growth generally mirrors that in the nation as a whole, as did endowment changes in the region. In southern states, value added growth is lowest in high-school dropout intensive sectors, which is consistent with the fact that the region had a large decline in the relative supply of high-school dropouts over the period. The exception is Florida, which shows no shift away from high-school-dropout-intensive sectors and which had a much smaller shift away from high-school dropouts than did the rest of the region. In the west, there is growth in very low-skill- and very high-skill-intensive sectors and relative declines in sectors intensive in intermediate skill levels. This is consistent with endowment shifts in the region, in particular in California which had relative growth in both high-school dropouts and college graduates.

The output-mix changes summarized in Table 5a are generally supported by looking at specific industries in individual states. To give one example, Table 5b shows annualized growth in state valued added minus growth in national value added by sector for California during the 1980s. Columns (3)-(5) in Table 5b rank sectors by their California factor intensity, using the three measures of factor intensity from Table 4. The six industries with the highest growth in real value added included two very skill-intensive sectors -- FIRE and legal services -- and three very unskill-intensive sectors -- textiles, apparel, and household services. The sixth industry, machinery, is not skill-intensive overall but it does contain the skill-intensive computer industry which, through the expansion of Silicon Valley, accounted for a large fraction of industry growth in California during the 1980s. California's growth in very high-skill and very low-skill intensive industries mirrors the state's endowment shifts, which, relative to the rest of the country, favored very high-skilled and very low-skilled labor. Table 5b also shows that some of the lowest-growth industries (leather, furniture) were also intensive in low-skilled labor. This exemplifies how, with many goods and few factors, output changes are not pinned down for each individual industry (i.e., there is output indeterminacy). To address this issue, we now turn to a more formal application of the production side of the HO model.

3 State Endowment Mixes and State Output Mixes: Accounting Decompositions

The previous section gave concrete evidence on state endowment changes and suggestive evidence of state output-mix changes. To examine output-mix changes more systematically, we decompose the absorption of state factor supply changes into portions accounted for by changes in output mix and changes in industry production techniques. While these accounting decompositions do not permit causal inference on whether endowment changes have contributed to output-mix changes, they are useful for identifying the mechanisms through which states absorb endowment shocks. Our approach is similar to that in Gandal, Hanson, and Slaughter (1999), who examine immigration shocks and output-mix changes in Israel.

We begin with the factor-market equilibrium conditions of HO production theory. Let there be N total industries and M primary factors of production. The standard assumptions are constant returns to scale in production, perfect competition, and no distortions in the economy. These assumptions are not essential for the analysis in this section, but will be required in the following section. It is conventional in production theory to focus on net industry outputs, but we work with value-added industry outputs because we only have value added data (in Section 4 we revisit the implications of using value-added data). In each state, factor-market equilibrium at each point in time is given by the following equation:

$$(2)\ \mathbf{V} = \mathbf{CX}$$

where **V** is an Mx1 vector of state primary factor endowments; **X** is an Nx1 vector of real state value-added output; and **C** is an MxN matrix of direct unit factor requirements in the state, such that element c_{mn} shows the units of factor *m* required to produce one dollar of real value added in industry *n*. Equation (2) says that the total supply of each factor equals total demand for each factor. We construct the data such that equation (2) holds as an identity for all states in all years (see the appendix). This requires defining the endowment vector **V** to equal total employment of factors in a state. Since we lack industry employment data on capital and land, we limit our attention to the rows of **V**, **C**, and **X** that apply to labor inputs.

Were it the case that immigration caused state labor endowments to change very quickly, we could examine changes in **V** and **X** holding **C** constant. This would allow us to test the Rybczynski Theorem directly by seeing whether states absorbed the observed changes in factor supplies through changes in output supplies, with constant factor prices and thus constant unit factor requirements. In our case we observe factor-supply changes over a ten-year period, so it is absurd to treat unit factor requirements as constant. During this period there were many shocks to product demand and technology, which surely caused changes in product and factor prices and thus in unit factor requirements. We must confront the fact that the **C** matrix is changing for reasons unrelated to changes in factor supplies. Our approach is simply to calculate the relative contribution of changes in outputs and changes in production techniques to absorption of factor supply changes. As we shall see, this exercise is informative both about the type of shocks states experience and how states adjust to these shocks.[7]

To convert equation (2) into the accounting decomposition we desire, we take first differences over time, which yields,

$$(3)\ \mathbf{\Delta V = .5(C_0+C_1)\Delta X + .5\Delta C(X_0+X_1)}$$

The subscripts indicate time periods 0 and 1, and $\Delta\mathbf{V}$, $\Delta\mathbf{X}$, and $\Delta\mathbf{C}$ are level changes across time. This equation decomposes the observed change in a state's factor supplies ($\Delta\mathbf{V}$) into two portions: that accounted for by output-mix changes (the first term on the right in (3)) and that accounted for by changes in production techniques (the second term on the right in (3)).

Since equation (3) holds as an identity, it yields no insights about causal relationships between $\Delta\mathbf{V}$, $\Delta\mathbf{X}$, and $\Delta\mathbf{C}$. For instance, **X** depends on endowments, product prices, and technology, and **C** depends on technology and factor prices, which in turn depend on endowments, product prices, and technology. From (3), we can make no direct inferences about the source of changes in **X** and **C**. Still, equation (3) is useful in an important respect. Since we can construct (3) on a state-by-state basis, we can control for changes in production techniques at the national industry level, which is an indirect way of controlling for national shocks to technology, product prices, and factor prices. This will reveal idiosyncratic changes in production techniques across states and thus possible violations of relative FPE.

Tables 6a-6d show the three components of equation (3) for high-school dropouts, high-school graduates, those with some college, and college graduates, respectively, for the twelve states. There are 40 industries in each state, and the change in variables is over the period 1980 to 1990. Column (1) shows the change in state factor supplies, column (2) shows mean unit factor requirements times the change in industry value added (summed over industries in a state), and column (3) shows the change in unit factor requirements times mean industry value added (summed over industries in a state). To control for regional business cycles, we divide both sides of equation (1) by total state employment and then perform the first difference in equation (3). This makes the factor supply changes in column (1) equal to the change in the share of a given labor type in total state employment.

Consider first the results for high-school dropouts and high-school graduates in Tables 6a and 6b. The negative values in column (1) show that there was a decline in the share of employment for less-educated workers in all states. All states had positive real value added growth on average, which increased demand for all factors as indicated by the positive values in column (2).

What allowed states to accommodate the fall in the relative labor supply of less-educated workers was a decline in unit labor requirements for these workers, as indicated by the negative values in column (3). Given that the relative wage of these workers also fell over the 1980s, this is consistent with skill-biased technological change. California had a relatively small shift away from high-school dropouts, but a relatively large shift away from high-school graduates.

Next, consider the results for those with some college and college graduates, shown in Tables 6c and 6d. Rising employment shares for more-educated workers in the 1980s, as indicated by the positive values in column (1), was accommodated by an increase in labor demand due to growth in real value added (positive values in column (2)) and increases in unit labor requirements (mostly positive values in column (3)). Interestingly, the changes in output mix in column (2) account for a relatively large fraction of the change in labor supplies in column (1). This is surprising in light of results by Davis and Haltiwanger (1991), Berman, Bound, and Griliches (1994) and others, which suggest that within-industry changes in factor usage, captured in our analysis by changes in the **C** matrix, account for most of the observed change in relative labor demand. Our findings suggest that between industry changes in output supplies are also an important part of the story, at least for more-educated workers.

Table 6 indicates that changes in the supply of different labor types have been accommodated by a combination of output changes and factor usage changes. It says nothing, however, about the shocks that caused these changes. Changes in factor usage at the state level could be due to changes in factor prices – resulting from technological change, product price changes, or other shocks – that differed across states. Such a scenario would be inconsistent with our output-mix hypothesis, since it would violate relative FPE across states.

To examine whether changes in unit labor requirements vary across states, we extend the decomposition in equation (3) to control for national changes in factor usage. To the extent that state changes in unit factor requirement mirror national changes, there is little scope for large deviations in relative factor prices across states. For each state, we decompose the change in the input requirement matrix, $\Delta\mathbf{C}$, into two components: (i) the generalized change in factor usage, equal to the state **C** matrix in 1980 times the percentage change in input requirements (on an industry-by-industry and factor-by-factor basis) for all other U.S. states over the period 1980-1990, and (ii) the idiosyncratic change in factor usage, equal to the residual, **ΔC** minus the generalized change in factor usage. The larger is the idiosyncratic component of changes in factor usage, the larger are the implied changes in relative state factor prices – due to state-specific changes in endowments, technology, or other factors – and the more likely it is that relative FPE is violated. Equation (4) describes this decomposition as

$$(4)\ \mathbf{\Delta V = .5(C_0+C_1)\Delta X + .5\Delta C_G(X_0+X_1) + .5\Delta C_I(X_0+X_1)}$$

where the subscripts *G* and *I* index generalized and idiosyncratic changes, respectively.

Columns (4) and (5) of Tables 6a-6d report the results for equation (4). Overall, idiosyncratic changes in unit factor requirements account for a small portion of state absorption of factor supply changes. Similar to column (3), generalized factor-usage changes in column (4) are large and negative for those with a high-school education or less (Tables 6a and 6b) and moderate and positive for those with at least some college (Tables 6c and 6d). For those with high school or less, idiosyncratic changes in factor usage in column (5) are much smaller in absolute value than the generalized changes, which suggests that changes in factor usage for less-educated workers were relatively similar across states. For those with some college and college graduates, idiosyncratic changes in column (5) are also smaller in absolute value relative to generalized changes, except for northeastern states and California which had a smaller shift towards more educated workers than did the rest of the country. These states may have adopted production techniques that favored more-skilled workers ahead of other states, in which case the results in column (5) would indicate technological convergence across states.

The results of this section suggest that during the 1980s changes in output mix helped accommodate changes in state factor supplies and that changes in unit factor requirements were relatively similar across states. Both findings are consistent with the output-mix hypothesis. One important unanswered question is whether variation in unit labor requirement across states is consistent with relative FPE. If relative FPE is violated, then it seems unlikely that changes in state output mixes could have accommodated state endowment shocks without changes in relative state factor prices. We now address this issue by testing for relative FPE directly.

4 Testing for Relative FPE across U.S. States

In the previous section we saw that in all states during the 1980s there was a large shift away from the use of less-educated workers and that in most states this shift matched the national shift away from these workers. Our main concern is whether changes in unit labor requirements were sufficiently different across states to be inconsistent with relative FPE. If we find this to be the case, then we cannot rule out the possibility that variation across states in changes in unit labor requirements reflect variation across states in changes in factor prices, indicating that one way in which states adjust to endowment shocks is through changes in factor prices relative to the rest of the country. In this section, we examine whether variation in unit labor requirement across states is consistent with relative FPE.

4a Methodology

Our test for relative FPE across U.S. states extends the methodology of Davis, et al (1997). Suppose that factor-market equilibrium is given by equation (1). Davis, et al (1997) claim that if two regions have equal factor prices and use identical production technologies, the regions will also have identical unit factor requirements. In our case, in which we use value-added data rather than the gross-output data they use, the test of FPE they propose is equivalent to seeing whether for two regions, *i* and *j*,

$$(5)\ \mathbf{V}^{j} = \mathbf{C}^{i}\mathbf{X}^{j}.$$

That is, the test involves seeing whether we can predict factor endowments for region *j* by combining output in region *j* with unit factor requirements in some other region *i*. If the answer is yes, then the conclusion is that factor prices are equalized between *i* and *j*.

One problem with using equation (5) to test FPE is that it is a necessary, but not sufficient, condition for FPE. If the number of goods exceeds the number of factors (N>M), which is typically the case, there is output indeterminacy: for a given **V** and **C**, there is not a unique **X** vector which satisfies equation (1) (Ethier, 1984).[8] Following this logic, for a given **V** and **X**, there is also not a unique **C** matrix which satisfies equation (1). Suppose that equation (5) holds for two regions, *i* and *j*. For a given $\mathbf{C^i}$ and $\mathbf{V^j}$ we could arbitrarily change the elements of the $\mathbf{X^j}$ vector and still satisfy the proposed condition for FPE. Similarly, for a given $\mathbf{X^j}$ and $\mathbf{V^j}$, we could arbitrarily vary the elements of the $\mathbf{C^i}$ matrix and still satisfy the proposed condition for FPE. Satisfying (5) is not sufficient to determine whether FPE holds between two regions.

We propose a test for relative FPE which, while similar in spirit to Davis, et al (1997), is based on sufficient conditions for FPE. Let **B** be the MxN matrix of direct unit factor requirements, whose elements show the quantity of each primary factor that each industry uses directly to produce one real dollar worth of gross output; let **A** be the NxN input-output matrix, whose elements show the real dollar value of intermediate inputs each industry purchases from other industries to produce a dollar of gross output. Then $\mathbf{D=B(I-A)^{-1}}$ is the MxN matrix of total (direct plus indirect) unit factor requirements, whose elements show the quantity of each primary factor each industry uses in total to produce one real dollar worth of net output. A sufficient condition for FPE to hold between two regions *i* and *j* is that firms in the two regions use identical input requirements (Dixit and Norman, 1980), in which case,

$$(6)\ \mathbf{D^i = D^j}.$$

We cannot test (6) because we do not have state net-output data to construct **D** matrices. However, we do have **C** matrices for states, which shows unit factor requirements for value added. The two matrices are related, in that $\mathbf{C=B(I-A')^{-1}}$, and so **C** is a function of the same two matrices as **D**. Our test for FPE is to examine whether for any pair of regions *i* and *j*,

$$(7)\ \mathbf{C^i = C^j}$$

which has as a maintained hypothesis that (6) is satisfied.

[8] Bernstein and Weinstein (1998) find evidence consistent with output indeterminacy for Japanese regions but not for OECD countries, which they interpret to mean that output indeterminacy is more likely to arise where trade costs between regions are low. We also find evidence of output indeterminacy across U.S. states. Harrigan (1997) uses international data to estimate the impact of factor-endowment changes on output shares. We estimated specifications similar to Harrigan's on our state data but obtained very imprecise coefficient estimates, as would be consistent with output indeterminacy.

It is important to emphasize that (6) and (7) are sufficient, but not necessary, conditions for FPE. If there are increasing returns to scale, regional differences in production technologies, or externalities in production, then regional unit factor requirements may not be equalized, even if there is regional FPE. Equal unit factor requirements across regions requires not just equal factor prices, but also the absence of significant scale effects, externalities, or arbitrary cross-state differences in production technologies. In testing for FPE using (7), we are forced to assume that these additional effects are inconsequential for relative regional factor prices.

There are certain types of factor-productivity differences across states for which we can and do control. If there are Hicks neutral technology differences across states or if, within education categories, average worker ability varies across states, then labor quantities will not be measured in productivity equivalent units. In this case, observed factor prices may differ in two states even if the "true" factor prices for productivity-equivalent units are the same. Following Trefler (1993), we control for factor-specific but industry-neutral productivity differences between states by respecifying equation (7) as,

$$\mathbf{C^i = diag(\Pi^j)Cj} \qquad (7')$$

where $\mathbf{\Pi^j}$ is an Mx1 vector which converts factor quantities in region *j* into productivity equivalent units for region *i*. Equation (7') is a sufficient condition for relative FPE to hold between regions *i* and *j*.

Equation (7') highlights the advantage of using unit factor requirements, rather than direct data on factor prices, to test for FPE. There is abundant evidence that nominal wages vary across states (Coehlo and Ghali, 1971; Johnson, 1983; Montgomery, 1992). Regional nominal wage differences could be due to differences in unobserved worker abilities, differences in regional technologies, factor immobility, or other sources. Wage data alone give no insight into whether inter-regional wage differences violate *relative* FPE, or just absolute FPE. By exploiting variation across industries in unit factor requirements, we can test for relative FPE while controlling for factors that cause deviations from absolute FPE. Relative FPE is consistent with wage differentials across states, as long as these differentials are due to differences in technology or average factor quality that are uniform across industries. We allow wages to be relatively high in California, for instance, as long as this is due to factors in California being uniformly more productive in all industries (for whatever reason).

Over our sample period, there may have been many national shocks to preferences and technology, which produced national changes in factor price changes that were common across states. If conditions are such that relative FPE across states was maintained, state factor prices, and hence state unit factor requirements, should move in unison. We test for relative FPE by estimating (7') in first differences, on a factor-by-factor and state-by-state basis, as

$$(8)\ \Delta \ln(c_{mni}) = \alpha_{mi} + \beta \Delta \ln(c_{mn0}) + \eta_{mni}\ ,$$

where c_{mni} is the unit labor requirement for factor m in sector n in state i; c_{mn0} is the unit labor requirement for factor m in sector n in the control region 0; a_{mi} and b are coefficients to be estimated, where $a_{mi}=Dln(p_{mi})$ captures differences in productivity growth between region i and region 0 that are specific to factor m and uniform across industries; and h_{mni} is an error term whose structure is discussed below. Under the null hypothesis of relative FPE, b = 1.[9]

4b Estimation Issues

There are three important estimation issues that merit further discussion. A first issue is that some of the 40 sectors in our data include industry groupings that are not comparable across states. This problem is particularly severe in agriculture. Given differences across states in land quality and soil composition, states specialize in very different agricultural products. California and Florida, for instance, specialize in perishable fruits and vegetables, while midwestern states specialize in grains. Petroleum refining is another problem industry since some states, such as California and Texas, have petroleum reserves while most other states do not. With little or no overlap across states in the goods that are produced in these sectors, there is no reason to expect unit labor requirements to be the same, with or without FPE.

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9 In related work, Maskus and Webster (1999) compare U.K. and U.S. unit factor requirements as a means of testing the HO model, while allowing for cross-country differences in technology.

We control for this possibility in two ways. First, based on the above considerations we omit from the sample agricultural sectors (agriculture, agricultural services, tobacco) and petroleum refining. This leaves us with 35 sectors per state.[10] Second, we exclude from the control group states that specialize mainly in agriculture (plains states) or other natural-resource intensive activities (mountain states). We also exclude small (mainly southern) states from the control group, where sample sizes of individuals by education group and industry in the PUMS are very small.[11] The control group we use has the 15 largest U.S. states, which include the 12 states described earlier plus Connecticut, Indiana, and Pennsylvania. In 1990, these 15 states accounted 68% of U.S. GDP and 65% of U.S. employment. For a given state, the control region is the 14 *other* states in the control set, such that the control group varies across states.

A second estimation issue is classical measurement error in the independent variable, the unit labor requirement for the control region. Unit labor requirements for a given state are calculated by combining BEA data on state value added, BEA data on state industry employment, and PUMS data on the share of workers in a given state industry that belong to a given education group (see appendix). Each of these values may be measured with error. A compounding factor is that the average ability of workers by education group may vary across states. These problems may be partially ameliorated by aggregating across states in constructing unit labor requirements for the control region. Still, we remain concerned that both c_{mni} and c_{mno} are subject to errors in measurement, which will tend to bias the OLS regression coefficient b in (8) towards zero, and thus lead us to reject relative FPE when it is true.[12]

There are several options for addressing measurement error. Since we have a single regressor, one option is to estimate the "reverse regression" (Klepper and Leamer, 1984) by

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[10] Preliminary regressions revealed that investment banking was an extreme outlier whose presence in the sample caused very large changes in coefficient estimates for certain states (NY, NJ, and IL). We also exclude this industry from the sample.

[11] To concord PUMS data with BEA data, we must start with three-digit Census industries, which exceed 200 in number. Once we separate workers by education group and industry in the PUMS, we have cell sizes for small states in the low single digits. For this reason, we exclude small states from the sample.

[12] Time differencing data may tend to exacerbate measurement error. This problem tends to be less severe for long time differences, as in our case where we work with the time difference between 1980 and 1990. Our results confirm this intuition, as estimates of b from equation (8) expressed in levels or first differences are very similar.

making the $Dln(c_{mni})$ the independent variable and $Dln(c_{mn0})$ the dependent variable in equation (8). Asymptotically, the OLS estimate of b from (8) is a lower bound for the true value of b while the inverse of the OLS coefficient from the reverse regression is an upper bound for the true value of b. We estimate equation (8) and its reverse regression to determine whether the lower and upper bounds for b span the value of one. If measurement error is severe enough, however, the bounds may be so wide as to be uninformative.

A second option for addressing measurement error is to use instrumental variables (IV). Valid instruments are often difficult to find. There are few exogenous variables which are likely to be correlated with unit factor requirements in the control region, but not with those in the state on which an observation is taken. Accordingly, we use the current and lagged ranks of c_{mn0} as instruments for $Dln(c_{mn0})$. One concern is that if ranks are noisy instruments, as may be the case, IV may increase the standard errors of the coefficient estimates.

A third option is to use extraneous information on the variance of the measurement error to estimate equation (8) (Judge, et al, 1980). If we know the ratio of the variances of the "true" and observed values of $Dln(c_{mn0})$, then we can obtain a consistent estimate of b. We do not observe this ratio directly, so we approximate it using information on $Dln(c_{mnUS})$, the change in unit labor requirements for the United States as a whole. If we assume that this value is measured with zero error and that its variance equals the variance of the true value of $Dln(c_{mn0})$, then we can use the ratio of the variance of $Dln(c_{mnUS})$ to the variance of $Dln(c_{mn0})$ to measure the ratio of the variances for the true and observed values of $Dln(c_{mn0})$. In theory, this ratio ranges from zero to one, with higher values indicating that measurement error is less of a problem. Estimates from this errors-in-variables (EIV) approach equal OLS estimates when the ratio equals one.[13]

A final estimation issue relates to efficient strategies for estimating b in (8). For each state we

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[13] Asymptotically, $\beta_{EIV} = \frac{\beta_{OLS}}{\text{ratio}}$. But in our small samples this link need not hold exactly. Also, nothing in the data necessarily prevents the estimated ratio from exceeding one. For cases where this was the case we set the ratio equal to one.

[14] In California, for instance, SUR estimates of b range from 0.65 to 0.75 while OLS estimates are from 0.85 to 0.99. In general, the asymptotic properties of the SUR estimator apply as the number of observations per equation (which is 35 in our case) becomes large, not as the number of equations times the number of observations becomes large (Greene, 1997).

have four equations, one per labor type. The disturbance term in (8) represents measurement error in unit labor requirements and shocks to factor usage that are idiosyncratic to specific states. These disturbances are likely to be correlated across labor types for a given industry in a given state. Even under standard assumptions, OLS estimates of *b* in (8) will not be efficient. Efficiency is of great concern since, for a true *b* that is close but not equal to one, standard errors that are too large will cause us to fail to reject relative FPE when it is in fact false. Generalized Least Squares (GLS) techniques, such as the Seemingly Unrelated Regression (SUR) framework, are the standard approach to obtain efficient coefficient estimates in this context. One potential problem with the SUR estimator is it may perform poorly in small samples, as in our case with 35 observations per factor and per state. Unreported results bear out this concern. For several states, SUR estimates of *b* are much lower than OLS estimates.[14]

Our solution to this problem is to estimate (8) by OLS (or IV) for each state by stacking the equations for the four labor types, allowing *b* to differ by factor, and then correcting the standard errors for both heteroskedasticity and correlation of the errors across factors for a given industry (Greene, 1997). This approach may be somewhat less efficient than SUR, but we avoid the potentially grave small sample problems associated with this and related estimators. For our EIV specifications we adjust for measurement error separately for each factor in each state; accordingly, our EIV estimation treats each factor separately rather than stacking.

4c Estimation Results

If the null hypothesis of relative FPE is true, then this result should be abundantly clear in the data: changes in each element of a state's **C** matrix should equal changes in each corresponding element of the **C** matrix for the control group (up to some scalar constant). To demonstrate that in our data this is indeed the case, Figure 1 plots the data for California; data plots for the other immigration gateway states are very similar. Each graph plots, for one of the four labor types, changes in unit labor requirements for the control group of states on the horizontal axis and changes in those for California are on the vertical axis. To show how the data line up, the 45-degree line passing through the origin is also shown.

Figure 1 gives broad visual support for relative FPE. Under absolute FPE, in each graph all industries should lie exactly along the 45-degree line. Under relative FPE, all industries should exactly along *a* 45-degree line -- but not necessarily the one through the origin. In every graph there are clearly some industries off a 45-degree line, which may indicate measurement error. But the overall impression is that the large majority of observations appear consistent with relative FPE. For example, the drop in **C**s for high-school dropouts in California was uniformly smaller than in the control group, which confirms the finding in Table 6a. This is consistent with relative FPE, but not absolute FPE. For the other three labor groups in California the graphs also look consistent with relative FPE, and perhaps even absolute FPE.

Tables 7 through 9 report results for our OLS, IV, and EIV estimation, respectively. In Tables 7 and 8, for each state we regress the change in industry unit labor requirements on the change in industry unit labor requirements for the control group of states, where we allow each of the four labor types to have distinct constant terms and slope parameters. That is, we stack the regression in equation (8) for the four labor types within a state and estimate a separate *a* and *b* for each type. Standard errors are adjusted for heteroskedasticity and correlation in the errors across factors within an industry (Greene, 1997).

The regression results in Tables 7 and 8 include two sets of hypothesis tests. First, we test the null hypothesis that the regression slope coefficient, *b* in equation (8), equals one, on a factor-by-factor basis. We report the p-values for this test, which indicate the level of significance at which the null would be rejected. This is an initial indication of whether the correlation in unit factor requirements across states is consistent with relative FPE. The relative FPE hypothesis, however, implies that *b* equals one for all factors in a state. Accordingly, the second hypothesis we test is the joint null that the regression slope coefficient is one for all labor types in a given state. We also report the F statistic and the associated p-value for this test. Table 9 with our EIV results is structured similar to Tables 7 and 8. The main difference is that the EIV approach allows us to test for relative FPE only on a factor-by-factor basis because we adjust for measurement error separately for each factor. In Table 9, for each factor-state case "Reliability" indicates the ratio of the variance of $Dln(c_{mnUS})$ to the variance of $Dln(c_{mn0})$, our estimate of the ratio of the variances for the true and observed values of $Dln(c_{mn0})$ in (8).

For all specifications, we must pick a significance level to use for deciding whether to reject relative FPE. Since the goal is to determine whether the data are consistent with the null of relative FPE, we are particularly concerned about failing to reject the null when it is false (type II errors). We can raise the power of the test by choosing a higher significance level, but at the potential cost of rejecting the null when it is true (type I errors). To reconcile these competing objectives, we summarize test results for several different significance levels.

To begin, consider the OLS results in Table 7. Overall, we fail to reject the null hypothesis that slope coefficients equal one at reasonably high significance levels for the vast majority of state-factor cases. The results are somewhat weaker when we consider the joint null of unity for all factors in given states. It is clear in Table 7 that the relative FPE hypothesis does particularly poorly in two states, Georgia and Washington, neither of which, it is important to note, are gateway states for immigration. In both these states we reject the null of unity for three of the four factors and reject the joint null of unity for all four factors at any significance level. For the other 10 states, however, there is much stronger support for relative FPE.

For high-school dropouts, coefficient estimates range from 0.75 in Illinois to 1.13 in Massachusetts, with most estimates between 0.85 and 1. We fail to reject the null of unity for 9 of the 10 remaining states (5 of the 6 gateway states) at the 15% significance level. For high-school graduates, coefficient estimates range from 0.81 in New York to 1.28 in North Carolina. We fail to reject the null of unity for 8 of the 10 states (all gateway states) at the 25% significance level and in 9 of 10 states at the 10% level. For those with some college, coefficient estimates range from 0.79 in Ohio to 1.21 in North Carolina. We fail to reject the null of unity in 9 of 10 states (all gateway states) at the 15% level and in all 10 states at the 5% level. For college graduates, coefficient estimates range from 0.76 in Massachusetts to 1.05 in Illinois. We fail to reject the null of unity in all 10 states at the 20% level. Finally, for the joint null that slope coefficients for all four labor types equal one in a state, we turn to the penultimate column in Table 7. Excluding Georgia and Washington, we fail to reject the joint null of unity in 7 of 10 states (3 of 6 gateway states) at the 15% level of significance.

One troubling aspect of the regression results in Table 7 is that most slope coefficients are less than one. Since the control group of states is composed mainly of the other states in the table (plus CT, IN, and PA), we would expect that roughly half of the slope coefficients would exceed one and roughly half would be less than one. That most fall below unity is a possible indication of estimation bias due to measurement error.

Our first approach for handling measurement error is to estimate the reverse regressions of equation (8) for each factor-state case. In 44 of the 48 cases, the coefficient estimate from the reverse regression was smaller than that from the forward regression.[15] Given differences in regional size, we suspect measurement error to be more severe for state data than for the control-group data, so this comparison supports concerns about measurement error. In all 48 cases the reverse coefficient was less than one, which mean the forward-reverse-regression coefficients interval brackets one in 37 of 48 (77%) cases. The 11 cases where the interval lies strictly above one can easily be seen in Table 7: they are the 11 cases where the forward regression coefficient exceeds one. That the forward-reverse coefficient estimates span one in more than three-quarters of the state-factor cases is further evidence in support of relative FPE.

Results for our second approach for handling measurement error, instrumental variables, are in Table 8. Comparing the results in Tables 7 and 8, IV coefficient estimates are closer to one for most, but not all, state-factor combinations. The results improve most dramatically for the two problem states, Georgia and Washington. We fail to reject the null of unity for three of four factors at the 10% significance level in either state, though we still reject the joint null of unity for

[15] The four cases where the reverse coefficient exceeds the forward are all "problem" cases for us in that the forward coefficient is significantly less than one: high-school graduates in NY, high-school dropouts in IL, and both factors in GA (see Table 7).

both states. For the 10 remaining states, there is also stronger support for relative FPE. We fail to reject the null of unity for just 2 of 40 state-factor cases at the 15% level and we fail to reject the joint null of unity in just one of the 10 states at the 10% level. Overall, at least some of the improvement from IV comes from larger coefficient estimates, as would be consistent with measurement error: in 22 cases the IV estimate is larger than the OLS estimate.

Finally, Table 9 reports our results for EIV estimation, our third strategy for addressing measurement error. As expected, results improve somewhat relative to OLS. At the 10% (5%) significance level we reject the null of relative FPE in only seven (five) of 48 cases, and all these problem cases were such for the OLS estimation as well. In 20 of the 48 cases our methodology indicated no measurement error (i.e., Reliability equals one). The largest EIV adjustment relative to OLS was for college graduates in California (reliability = 0.927). These results further confirm support for the null of relative FPE.

4d Discussion of Estimation Results

In unreported results, we examined the sensitivity of our findings to alterations in the specification and to further restrictions on the sample of states and industries. The requirement that the log change in unit factor requirements are exactly equal across states is quite stringent. A less restrictive model would allow state and industry-specific shocks to technology that dissipate over time, as technology diffuses across states. The error correction model, which would modify equation (8) by adding the regressor $ln(c_{mni,\ t-1}) - ln(c_{mn0,t-1})$, is a natural specification of slow technology diffusion across states. In this framework, the change in unit factor requirements in a state is increasing in the change in unit factor requirements in other states, but decreasing in the difference of the initial levels. OLS and IV estimation of equation (8) including the regressor $ln(c_{mni,\ t-1}) - ln(c_{mn0,t-1})$, produced very similar results to those in Tables 7 and 8. We again found that the vast majority of slope coefficients are not statistically different from unity. As additional extensions, we experimented with dropping industries that produce mainly non-traded outputs (services, mining, construction), testing for FPE in other large states (CT, IN, PA), restricting the control region to be the six gateway states (NY, NJ, IL, FL, TX, CA), and weighting the regressions by the average industry share of state employment over the time period. The results for these modifications are very similar to those that we report.

Based on evidence in the literature, which shows substantial variation in nominal wages across regions within the United States, the prior of most researchers may be that we would easily reject relative FPE. Relative to this prior, our results are rather surprising. It is important to re-emphasize, however, that our results on relative FPE are consistent with nominal factor-price differences across states. What we find is that inter-state productivity differences are uniform across industries and therefore consistent with relative FPE. By allowing for factor-specific productivity differences across states, we have permitted unit labor requirements for a given labor type to vary between two states by a scalar constant. If this scalar constant changes over time, due to changes in average factor quality or neutral changes in technology that vary across states, then observed wage differentials will change over time. Recent work by Bernard and Jensen (1999) suggests that wage differences between states have changed over time. Our results suggest that any such changes are likely due to changes in technology or factor quality that are uniform across industries within states.

5 Conclusion

In this paper we examine whether state factor prices could be invariant to state-specific shocks to factor supplies. The motivation for this exercise is the apparent insensitivity of U.S. regional wages to large immigrant inflows during the 1980s. Following the logic of the Rybczynski Theorem from HO theory, if states are open to trade they can absorb shocks to relative factor endowments by altering their mix of outputs without any changes in relative factor prices. We have looked for evidence supporting this output-mix hypothesis using a newly constructed data set covering four factors, 40 sectors, and 15 states in 1980 and 1990.

We have two key findings. The first is that during the 1980s there were substantial shifts in relative labor supplies across U.S. states which were broadly matched by state output-mix changes. The northeast became more concentrated in highly-educated labor, southern states had a relatively large shift away from labor with low education levels, and California became relatively concentrated in both very high-skilled and very low-skilled labor. While changes in the size of the native labor force offset immigrant inflows in many states, in California a large influx of low-education immigrants maintained its relative supply of low-skilled workers. Matching these endowment shifts, we find that state output growth tended to be high in sectors that are intensive in expanding factors. California, for instance, had highest relative growth in industries intensive in either high-school dropouts or college graduates. Beyond these stylized facts, our accounting decompositions of HO factor-market equilibrium conditions indicate that both output changes and changes in unit factor requirements account for how states absorb changes in factor supplies. But changes in factor usage are relatively similar across states, which suggests that changes in state unit factor requirements are due largely to *national* shocks rather than state-specific wage responses to state-specific shocks.

The second main finding is that relative FPE holds across states for the vast majority of state-factor cases in our sample, including the gateway states for immigration. Relative FPE is "indirect" support for the output-mix hypothesis, in that relative FPE implies that the related states all experience common relative-wage responses, if any, to an endowment shock in any one state. This happens either through changes in the import or export of goods in response to output-mix changes or through inter-state migration and capital flows.

Overall, these results suggest that states adjust to immigration shocks through mechanisms other than changing relative factor prices across states. In closing, we make two comments on our findings to highlight possible areas for future research.

First, our results do not show a causal linkage between endowment changes and output changes. We instead have searched for conditions that are consistent with the output-mix hypothesis. In particular, our finding of relative FPE across states is silent on the source(s) of the adjustment process to endowment shocks. Changes in inter-state trade flows in response to output-mix changes or inter-state migration and investment flows are both plausible processes.

An important topic for future research is to identify the relative importance of these mechanisms for how states adjust to changes in factor supplies and other shocks.

Second, throughout the paper our unit of analysis has been individual states. We have not analyzed how the United States as a whole accommodates immigration inflows, or endowment shocks more generally. The evidence that state-specific endowment shocks do not trigger state-specific wage responses does *not* imply that the United States overall had no wage response to increased immigration. For the latter to be the case, any change in national output mixes from immigration would have to have had no effect on world product prices and thus not triggered Stolper-Samuelson wage effects. We have not addressed this issue, but we regard it as an important topic for future research.

Data Appendix

Output and Employment by Industry and State: We measure industry output at the state level as real value added in 1992 dollars. These data come from the United States Department of Commerce, Bureau of Economic Analysis (the raw data were obtained from http://www.bea.doc.gov/bea/regional/data.htm). We measure industry employment at the state level using total employment (all full and part-time workers) from the Regional Economic Information System of the Bureau of Economic Analysis (1969-1996 REIS CD ROM). For both output and employment, we use data for 1980 and 1990. The industries cover all civilian sectors of the economy. Data for 1980 were classified by the 1972 Standard Industrial Classification (SIC) code; data for 1990 were classified by the 1987 SIC code. We matched the 1990 data back to the 1972 SIC code, using the concordance from Wayne Gray which accompanies the National Bureau of Economic Research Manufacturing Productivity Data Base. The raw BEA data are available at the two-digit SIC level. In a few cases, concording data between years required us to aggregate several two-digit industry into a single sector (e.g., in our data the electrical machinery industry combines SIC industries 36 and 38). To concord BEA data to PUMS data (described below), we combined two-digit SIC industries into 40 sectors (listed in Table 4), which are a mix of one-digit and two-digit SIC industries, using the concordance given in, "The Relationship between the 1970 and 1980 Industry and Occupation Classification Systems," Technical Paper 59, Bureau of the Census, U.S. Department of Commerce.

State Labor Endowments by Education Category: We measure the total state labor force by four education categories: high-school dropouts, high-school graduates, those with some college, and college graduates. These data come from the 1980 and 1990 *Public Use Microsamples* (PUMS) of the *U.S. Census of Population and Housing*. An individual is counted as in the labor force if he or she is employed or unemployed but looking for work. We calculate state labor endowments by summing the population weights given in the 5% PUMS across all individuals that live in a given state, are part of the labor force, and belong to a given educational category.

Employment and Unit Labor Requirements by State, Industry, and Education Category: To calculate employment by state, industry, and education category, we combine data from the BEA and the PUMS. Beginning with the PUMS, we sum the earnings weights for all individuals who are employed (at work or with a job but not at work) in a given industry, work in a given state, and belong to a given educational category. We then use these totals to calculate the share of individuals in a given state and industry that belong to each education category. Multiplying these shares by total employment, as measured by the BEA, we obtain total employment by state, industry, and education category. To obtain unit labor requirements by state, industry, and education category, we simply take the ratio of employment by state, industry, and education category to value added by state and industry.

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Table 1a: U.S. State Labor Endowments, Levels in 1980 and 1990

State	Year	HSDO	HSG	SC	CG
United States	1980	25.0	34.8	22.5	17.7
	1990	18.1	29.9	29.6	22.4
New York	1980	23.6	33.1	22.0	21.2
	1990	17.3	28.4	27.3	27.0
New Jersey	1980	24.0	35.6	19.5	20.9
	1990	16.6	30.6	24.8	28.0
Illinois	1980	25.1	34.4	22.5	18.0
	1990	16.9	29.0	30.4	23.8
Florida	1980	25.9	34.1	24.0	16.1
	1990	20.3	29.3	30.6	19.8
Texas	1980	29.4	29.2	23.6	17.8
	1990	21.8	26.0	30.5	21.7
California	1980	21.5	28.1	30.2	20.2
	1990	20.2	21.5	33.8	24.5
Massachusetts	1980	21.0	34.1	22.9	22.1
	1990	14.5	27.9	27.6	30.1
Ohio	1980	24.2	41.0	19.5	15.2
	1990	16.6	36.5	27.7	19.2
Michigan	1980	23.4	37.7	23.3	15.6
	1990	15.9	31.4	33.3	19.5
North Carolina	1980	34.8	32.5	18.8	13.9
	1990	22.0	31.5	28.1	18.3
Georgia	1980	33.1	32.3	18.9	15.7
	1990	21.8	31.5	26.0	20.7
Washington	1980	17.9	34.4	28.2	19.6
	1990	13.1	26.4	36.3	24.2

Notes: Each cell reports the share of that state's total labor force (employed plus unemployed) accounted for by the factor in that cell. "HSDO" designates high-school dropouts; "HSG" designates high-school graduates; "SC" designates those with some college; and "CG" designates college graduates and beyond.

Table 1b: U.S. State Labor Endowments, Changes Over the 1980s

State	HSDO	HSG	SC	CG
United States	-6.9	-4.8	7.1	4.7
New York	-6.4	-4.8	5.3	5.8
New Jersey	-7.4	-5.0	5.4	7.0
Illinois	-8.3	-5.5	7.9	5.8
Florida	-5.6	-4.8	6.7	3.8
Texas	-7.6	-3.3	7.0	3.9
California	-1.3	-6.6	3.6	4.3
Massachusetts	-6.5	-6.2	4.7	8.0
Ohio	-7.6	-4.5	8.2	4.0
Michigan	-7.6	-6.3	10.0	3.9
North Carolina	-12.8	-1.0	9.3	4.4
Georgia	-11.3	-0.8	7.1	5.0
Washington	-4.8	-8.0	8.2	4.6

Notes: Each cell reports the level change from 1980 to 1990 in the share of that state's total labor force (employed plus unemployed) accounted for by the factor in that cell. "HSDO" designates high-school dropouts; "HSG" designates high-school graduates; "SC" designates those with some college; and "CG" designates college graduates and beyond.

Table 2: U.S. Immigrant Shares of Education Groups, 1980 and 1990

State	Period	HSDO	HSG	SC	CG
United States	1980	10.1	4.6	5.6	7.2
	1990	18.6	6.1	6.8	9.3
New York	1980	23.4	12.0	11.9	13.0
	1990	32.8	15.5	14.6	15.9
New Jersey	1980	17.4	8.6	8.9	11.2
	1990	25.2	11.2	11.5	15.1
Illinois	1980	13.7	5.0	6.1	9.3
	1990	22.5	7.2	6.8	9.7
Florida	1980	15.4	9.3	10.9	11.4
	1990	25.4	11.9	12.4	13.4
Texas	1980	11.9	4.0	4.2	5.2
	1990	25.7	6.1	5.7	7.9
California	1980	33.0	11.4	11.0	14.0
	1990	54.5	19.3	16.0	19.4
Massachusetts	1980	17.5	6.2	4.8	6.3
	1990	23.1	8.2	7.1	8.6
Ohio	1980	3.1	1.8	2.5	4.4
	1990	2.9	1.5	2.0	4.8
Michigan	1980	5.2	3.0	3.6	6.1
	1990	5.3	2.5	2.8	6.6
North Carolina	1980	0.9	1.3	1.7	2.7
	1990	2.0	1.3	1.9	3.8
Georgia	1980	1.1	1.7	2.6	3.1
	1990	3.8	2.3	3.1	5.2
Washington	1980	8.6	4.6	5.1	6.7
	1990	15.1	5.4	5.4	7.6

Notes: Each cell reports the share of that state's total labor force (employed plus unemployed) in that cell's education group accounted for by immigrants. "HSDO" designates high-school dropouts; "HSG" designates high-school graduates; "SC" designates those with some college; and "CG" designates college graduates and beyond.

Table 3: U.S. State Endowments of Natives and Immigrants, Change 1980-1990

State	Worker Type	HSDO	HSG	SC	CG
United States	Natives	-7.8	-5.1	6.4	3.9
	Immigrants	0.8	0.2	0.8	0.8
New York	Natives	-6.5	-5.2	4.0	4.3
	Immigrants	0.1	0.4	1.4	1.5
New Jersey	Natives	-7.4	-5.4	4.3	5.2
	Immigrants	0.0	0.4	1.1	1.9
Illinois	Natives	-8.6	-5.8	7.2	5.2
	Immigrants	0.4	0.3	0.7	0.6
Florida	Natives	-6.8	-5.1	5.5	2.9
	Immigrants	1.2	0.3	1.2	0.8
Texas	Natives	-9.7	-3.7	6.2	3.1
	Immigrants	2.1	0.4	0.8	0.8
California	Natives	-5.2	-7.6	1.5	2.4
	Immigrants	3.9	0.9	2.1	1.9
Massachusetts	Natives	-6.2	-6.3	3.8	6.8
	Immigrants	-0.3	0.2	0.8	1.2
Ohio	Natives	-7.4	-4.3	8.1	3.7
	Immigrants	-0.3	-0.2	0.1	0.3
Michigan	Natives	-7.2	-6.0	9.9	3.6
	Immigrants	-0.4	-0.4	0.1	0.3
North Carolina	Natives	-12.9	-1.0	9.2	4.1
	Immigrants	0.1	0.0	0.2	0.3
Georgia	Natives	-11.8	-1.0	6.8	4.4
	Immigrants	0.5	0.2	0.3	0.6
Washington	Natives	-5.2	-7.8	7.6	4.1
	Immigrants	0.4	-0.2	0.5	0.5

Notes: Each cell reports the level change in the share of that state's total labor force (employed plus unemployed) accounted for by the factor in that cell. "HSDO" designates high-school dropouts; "HSG" designates high-school graduates; "SC" designates those with some college; and "CG" designates college graduates and beyond.

Table 4: U.S. Industry Factor Intensity, 1980 and 1990

Industry Name	HSDO/CG 1980	HSDO/CG 1990	HSG/CG 1980	HSG/CG 1990	SC/CG 1980	SC/CG 1990
Agriculture	5.69	4.66	4.56	3.82	1.82	2.15
Agr. Services	2.11	1.90	1.41	1.49	1.20	1.58
Mining	1.62	0.91	2.21	1.76	1.25	1.31
Construction	3.86	2.47	4.68	3.68	2.29	2.52
Food Products	3.71	2.57	3.59	3.12	1.52	2.02
Tobacco	3.29	0.92	3.71	2.62	1.86	1.56
Textiles	9.49	6.41	7.20	8.05	1.76	3.41
Apparel	12.29	5.39	9.29	4.47	2.43	2.12
Lumber	8.69	4.79	8.06	6.28	2.84	3.49
Furniture	5.72	4.41	4.79	4.38	1.74	2.94
Paper	2.72	1.81	3.64	4.13	1.36	1.82
Printing	1.05	0.55	2.13	1.32	1.34	1.21
Chemicals	0.82	0.41	1.67	1.15	0.91	1.01
Petro. Refining	0.71	0.28	1.65	1.20	0.96	1.49
Rubber	2.59	2.73	4.05	4.40	1.28	2.85
Leather	14.25	4.04	14.25	5.93	2.63	2.03
Stone/Clay/Glass	4.81	1.75	6.74	3.17	2.63	1.94
Primary Metals	3.77	2.03	5.05	3.78	1.90	2.20
Metal Products	3.83	2.13	5.36	3.99	2.34	2.60
Machinery	1.76	0.80	3.43	1.74	1.78	1.63
Elec. Machinery	1.50	0.62	2.54	1.23	1.48	1.24
Tranport Equip.	1.71	0.73	3.14	1.70	1.60	1.61
Misc. Manuf.	3.12	1.58	3.91	2.02	1.80	1.68
Transport/Utilities	1.79	0.82	3.78	2.21	2.28	2.11
Wholesale Trade	1.47	0.65	2.57	1.49	1.73	1.45
Retail Trade	3.88	2.30	4.39	3.16	2.90	2.82
FIRE	0.41	0.22	1.71	1.02	1.37	1.37
Investment Finance	0.13	0.07	0.44	0.30	0.67	0.57
Lodging Services	4.67	2.30	4.18	3.07	2.61	2.74
Personal Services	5.70	1.70	8.02	3.30	4.32	2.69
Business Services	0.39	0.27	0.75	0.53	0.80	0.83
Auto Services	7.11	6.96	7.76	8.70	3.24	6.03
Repair Services	4.53	3.01	6.12	4.86	3.28	3.62
Entertainment	1.51	1.08	1.57	1.26	1.47	1.59
Health Services	0.69	0.33	1.18	0.77	1.20	1.22
Legal Services	0.04	0.05	0.37	0.24	0.43	0.43
Educ. Services	0.18	0.11	0.30	0.26	0.34	0.35
Social Services	0.53	0.37	0.74	0.65	0.77	0.82
Household Services	22.80	9.26	8.30	5.58	3.90	3.31
Government	0.55	0.23	1.49	0.96	1.18	1.37

Notes: Each cell reports the ratio of national employment of that cell's two factors for the industry and year of that cell. "HSDO" designates high-school dropouts; "HSG" designates high-school graduates; "SC" designates those with some college; and "CG" designates college graduates and beyond. Industry categories combine one- and two-digit SIC industries.

Table 5a: U.S. State Output-Mix Changes, 1980 to 1990

State	HSDO Weights	HSG Weights	SC Weights	CG Weights
United States	0.03	-0.16	-0.05	0.18
New York	-0.04	-0.14	-0.04	0.21
New Jersey	-0.33	-0.21	0.12	0.42
Illinois	-0.02	-0.18	-0.08	0.27
Florida	0.00	-0.07	0.00	0.06
Texas	-0.21	-0.20	-0.02	0.43
California	0.12	-0.09	-0.14	0.10
Massachusetts	-0.41	-0.21	0.12	0.50
Ohio	0.02	-0.14	-0.08	0.19
Michigan	0.13	-0.06	-0.18	0.10
North Carolina	-0.19	-0.04	0.03	0.19
Georgia	-0.13	-0.18	0.06	0.26
Washington	0.19	-0.10	-0.13	0.04

Notes: Each cell reports the weighted-average change in log real state value added across all 40 sectors, as defined by equation (1) in the text. Each row corresponds to a different state, and the weights vary across the four columns. The weights are an industry's share of state-wide employment for a given factor type, averaged over 1980 and 1990. To control for the fact that over the sample period some states grew faster than others, the value reported in each cell is normalized by subtracting off the weighted-average change in log real state value added using as weights total employment by industry (calculated as the average of the weights for the four labor types, such that values in each row sum to zero).

Table 5b: California Output-Mix Changes, 1980 to 1990

Industry Name	Annualized Output Growth	HSDO/CG Rank	HSG/CG Rank	SC/CG Rank
Machinery	4.32	25	26	29
Household Services	3.89	1	2	2
Apparel	3.86	3	16	18
FIRE	2.76	36	31	27
Textiles	2.54	11	19	24
Legal Services	2.32	40	40	40
Elec. Machinery	1.74	29	30	31
Construction	1.41	20	10	7
Wholesale Trade	1.41	22	24	20
Transport/Utilities	1.40	28	21	11
Lodging Services	1.29	17	14	13
Personal Services	1.16	15	6	4
Entertainment	1.14	27	32	25
Printing	1.02	31	27	28
Stone/Clay/Glass	0.92	16	8	12
Food Products	0.73	13	17	21
Government	0.71	37	34	26
Business Services	0.71	35	37	37
Agriculture	0.70	4	18	16
Retail Trade	0.49	18	11	6
Auto Services	0.39	6	1	1
Social Services	0.23	33	35	36
Chemicals	0.19	24	29	34
Agr. Services	0.12	14	22	23
Repair Services	-0.01	10	4	3
Rubber	-0.09	9	13	17
Misc. Manuf.	-0.18	21	20	19
Health Services	-0.20	34	36	35
Primary Metals	-0.36	8	7	10
Investment Finance	-0.39	39	38	38
Transport Equip.	-0.40	30	33	32
Educ. Services	-0.46	38	39	39
Metal Products	-0.69	12	9	9
Lumber	-0.80	7	3	5
Paper	-0.86	19	12	15
Mining	-1.07	26	25	30
Leather	-1.23	2	15	14
Furniture	-1.60	5	5	8
Petro. Refining	-2.54	32	28	33
Tobacco	-19.62	23	23	22

Notes: Each industry's output growth rate is the California annualized growth rate less the U.S. annualized growth rate, all in terms of real value added. For the factor-intensity measures, the ranks are constructed as the average of the ranks in 1980 and 1990. Within each ranking, lower (higher) numbers indicate more unskill-intensive (skill-intensive) industries.

Table 6a: Employment Decompositions for High-School Dropouts, 1980 to 1990

State	ΔV	$.5(C_0+C_1)\Delta X$	$.5\Delta C(X_0+X_1)$	$.5\Delta C_g(X_0+X_1)$	$.5\Delta C_I(X_0+X_1)$
New York	-6.01	3.11	-9.12	-8.51	-0.61
New Jersey	-7.41	3.62	-11.03	-9.78	-1.24
Illinois	-7.52	1.51	-9.03	-9.02	-0.01
Florida	-5.18	2.47	-7.65	-9.13	1.48
Texas	-8.39	1.32	-9.71	-11.40	1.70
California	-0.99	2.88	-3.88	-8.17	4.29
Massachusetts	-6.90	3.32	-10.22	-8.31	-1.91
Ohio	-7.15	1.78	-8.93	-8.87	-0.07
Michigan	-6.82	0.76	-7.58	-8.03	0.45
North Carolina	-12.82	4.46	-17.28	-14.75	-2.53
Georgia	-11.11	4.13	-15.24	-13.41	-1.83
Washington	-4.30	0.85	-5.14	-6.31	1.17

Notes: The decomposition for each state in this table follows equations (3) and (4) in the text. Column (1) shows the change in a given factor's share of total state employment, column (2) shows the contribution of changes in output to changes in factor employment, and column (3) shows the contribution of changes in unit labor requirements to changes in factor employment. Columns (4) and (5) further decompose column (3) into the contributions of generalized changes in unit labor requirements common across all states (column (4)) and changes in unit labor requirements that are idiosyncratic to a given state (column (5)).

Table 6b: Employment Decompositions for High-School Graduates, 1980 to 1990

State	ΔV	$.5(C_0+C_1)\Delta X$	$.5\Delta C(X_0+X_1)$	$.5\Delta C_g(X_0+X_1)$	$.5\Delta C_I(X_0+X_1)$
New York	-5.81	5.02	-10.83	-8.30	-2.53
New Jersey	-5.95	6.98	-12.93	-9.82	-3.11
Illinois	-6.40	2.14	-8.55	-8.80	0.26
Florida	-5.74	3.44	-9.18	-7.24	-1.93
Texas	-3.55	1.59	-5.15	-7.37	2.22
California	-6.94	3.30	-10.24	-5.86	-4.38
Massachusetts	-7.35	7.09	-14.43	-9.00	-5.43
Ohio	-5.78	3.15	-8.93	-10.83	1.89
Michigan	-7.79	0.89	-8.68	-9.32	0.64
North Carolina	-1.56	5.39	-6.95	-8.57	1.62
Georgia	-1.16	4.72	-5.89	-8.03	2.14
Washington	-8.59	0.91	-9.49	-7.44	-2.06

Notes: The decomposition for each state in this table follows equations (3) and (4) in the text. See notes to Table 6a.

Table 6c: Employment Decompositions for Some College, 1980 to 1990

State	ΔV	$.5(C_0+C_1)\Delta X$	$.5\Delta C(X_0+X_1)$	$.5\Delta C_g(X_0+X_1)$	$.5\Delta C_I(X_0+X_1)$
New York	5.72	4.36	1.37	4.84	-3.48
New Jersey	5.84	5.33	0.51	4.26	-3.75
Illinois	8.05	2.06	6.00	4.39	1.61
Florida	7.23	3.18	4.05	5.89	-1.84
Texas	7.64	2.06	5.59	4.15	1.43
California	3.30	4.29	-0.99	7.07	-8.06
Massachusetts	4.78	6.76	-1.97	4.98	-6.95
Ohio	8.73	2.07	6.65	4.18	2.48
Michigan	10.26	0.27	9.99	5.26	4.73
North Carolina	9.58	4.15	5.43	4.54	0.89
Georgia	6.96	3.94	3.02	4.30	-1.27
Washington	7.96	0.74	7.21	6.28	0.93

Notes: The decomposition for each state in this table follows equations (3) and (4) in the text. See notes to Table 6a.

Table 6d: Employment Decompositions for College Graduates, 1980 to 1990

State	ΔV	$.5(C_0+C_1)\Delta X$	$.5\Delta C(X_0+X_1)$	$.5\Delta C_g(X_0+X_1)$	$.5\Delta C_I(X_0+X_1)$
New York	6.09	4.89	1.20	3.37	-2.16
New Jersey	7.51	5.81	1.70	2.08	-0.38
Illinois	5.88	2.28	3.60	2.48	1.12
Florida	3.68	2.06	1.62	2.93	-1.31
Texas	4.30	2.29	2.00	1.61	0.39
California	4.63	3.37	1.26	3.40	-2.14
Massachusetts	9.46	7.62	1.84	3.36	-1.51
Ohio	4.21	1.87	2.34	1.96	0.38
Michigan	4.35	0.68	3.68	2.42	1.26
North Carolina	4.80	2.77	2.03	1.72	0.31
Georgia	5.32	3.25	2.06	2.25	-0.19
Washington	4.93	0.88	4.04	3.12	0.93

Notes: The decomposition for each state in this table follows equations (3) and (4) in the text. See notes to Table 6a.

Table 7: OLS Regressions Testing For Relative FPE

State	Quantity	HSDO	HSG	SC	CG	Joint Test	R Squared
New York	β	0.900	0.814	0.955	0.844		
	σ	0.083	0.056	0.065	0.135	3.12	
	p-value for β=1	0.235	0.002	0.495	0.256	0.027	0.887
New Jersey	β	0.968	0.854	1.032	0.991		
	σ	0.249	0.186	0.189	0.144	0.39	
	p-value for β=1	0.900	0.439	0.865	0.953	0.813	0.598
Illinois	β	0.748	0.976	0.979	1.047		
	σ	0.076	0.068	0.107	0.096	3.66	
	p-value for β=1	0.002	0.731	0.844	0.631	0.014	0.907
Florida	β	0.899	0.908	0.951	1.033		
	σ	0.136	0.120	0.189	0.162	0.37	
	p-value for β=1	0.463	0.448	0.798	0.839	0.830	0.635
Texas	β	0.969	0.979	0.946	1.018		
	σ	0.134	0.178	0.205	0.203	0.16	
	p-value for β=1	0.820	0.901	0.793	0.928	0.958	0.757
California	β	0.835	1.038	0.901	0.860		
	σ	0.111	0.154	0.131	0.135	3.01	
	p-value for β=1	0.147	0.804	0.454	0.307	0.031	0.763
Massachusetts	β	1.129	0.888	0.855	0.758		
	σ	0.187	0.126	0.144	0.202	0.94	
	p-value for β=1	0.494	0.383	0.320	0.239	0.454	0.804
Ohio	β	0.900	0.864	0.794	0.858		
	σ	0.074	0.118	0.121	0.126	1.42	
	p-value for β=1	0.184	0.258	0.096	0.266	0.250	0.822
Michigan	β	0.960	1.027	1.141	0.953		
	σ	0.145	0.091	0.102	0.142	0.53	
	p-value for β=1	0.786	0.766	0.176	0.743	0.715	0.803
North Carolina	β	0.853	1.278	1.211	0.953		
	σ	0.267	0.167	0.204	0.208	1.73	
	p-value for β=1	0.586	0.104	0.309	0.824	0.165	0.754
Georgia	β	0.763	0.743	0.780	0.669		
	σ	0.137	0.107	0.195	0.120	4.00	
	p-value for β=1	0.093	0.022	0.268	0.010	0.010	0.782
Washington	β	0.758	0.843	0.759	1.535		
	σ	0.137	0.094	0.165	0.193	4.47	
	p-value for β=1	0.086	0.103	0.155	0.009	0.005	0.725

Notes: These results come from estimating equation (8) using "stacked" ordinary least squares, with standard errors adjusted for heteroskedasticity and correlation in the errors across factors within an industry. For each state-factor case, the null of relative FPE is that β=1. For each state, the joint null of relative FPE is that β=1 for all four factors together. “HSDO" means high-school dropouts; "HSG" means high-school graduates; "SC" means some college; and "CG" means college graduates.

Table 8: IV Regressions Testing For Relative FPE

State	Quantity	HSDO	HSG	SC	CG	Joint Test
New York	β	0.925	0.820	0.968	0.916	
	σ	0.112	0.060	0.072	0.131	2.52
	p-value for β=1	0.511	0.005	0.656	0.523	0.059
New Jersey	β	0.643	0.771	1.024	1.030	
	σ	0.323	0.253	0.350	0.156	0.82
	p-value for β=1	0.277	0.373	0.946	0.849	0.520
Illinois	β	0.844	0.966	0.909	1.010	
	σ	0.060	0.069	0.156	0.114	1.84
	p-value for β=1	0.013	0.625	0.534	0.936	0.143
Florida	β	0.839	0.936	1.126	0.924	
	σ	0.161	0.097	0.155	0.199	0.79
	p-value for β=1	0.324	0.512	0.423	0.703	0.541
Texas	β	1.056	1.018	0.792	1.003	
	σ	0.161	0.215	0.228	0.236	0.69
	p-value for β=1	0.729	0.934	0.370	0.990	0.603
California	β	0.921	1.096	0.844	0.803	
	σ	0.105	0.164	0.153	0.178	2.09
	p-value for β=1	0.456	0.564	0.315	0.277	0.104
Massachusetts	β	0.942	0.940	0.797	0.684	
	σ	0.148	0.120	0.197	0.238	0.61
	p-value for β=1	0.703	0.622	0.312	0.194	0.660
Ohio	β	0.900	0.818	0.846	1.058	
	σ	0.115	0.126	0.181	0.210	0.63
	p-value for β=1	0.389	0.159	0.400	0.786	0.647
Michigan	β	0.857	1.011	1.162	0.837	
	σ	0.133	0.111	0.139	0.192	0.50
	p-value for β=1	0.290	0.925	0.252	0.402	0.736
North Carolina	β	1.026	1.230	1.316	0.901	
	σ	0.160	0.103	0.218	0.223	1.83
	p-value for β=1	0.871	0.029	0.156	0.659	0.146
Georgia	β	0.758	0.820	0.720	0.711	
	σ	0.104	0.119	0.272	0.146	3.21
	p-value for β=1	0.027	0.139	0.311	0.056	0.024
Washington	β	0.623	0.845	0.662	1.358	
	σ	0.170	0.103	0.243	0.270	2.34
	p-value for β=1	0.033	0.141	0.173	0.194	0.075

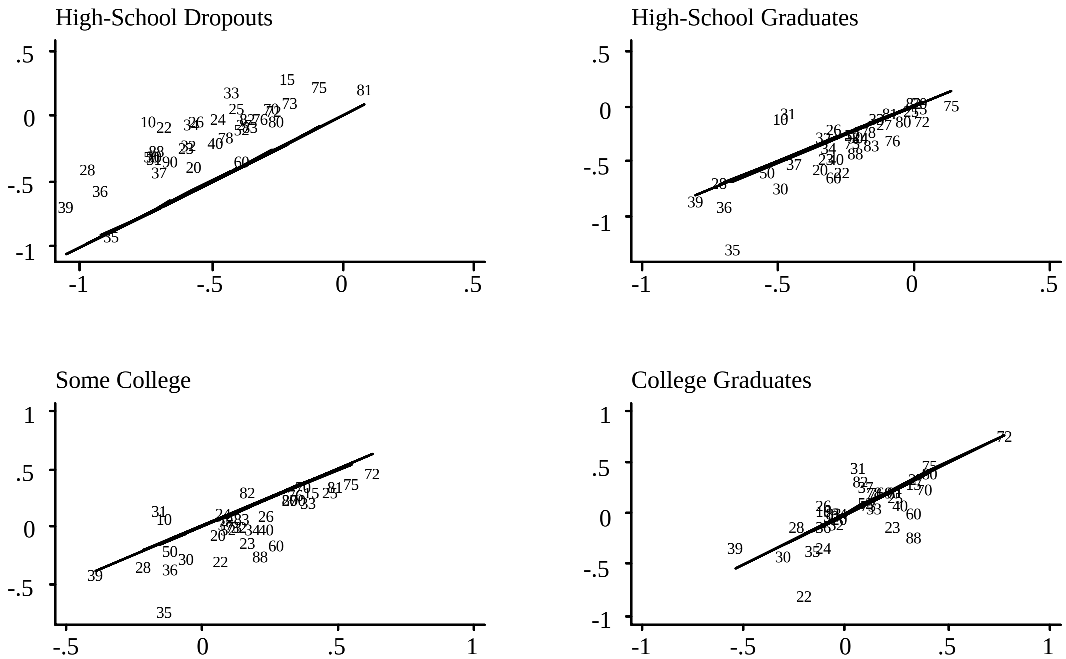
Notes: These results come from estimating equation (8) using "stacked" instrumental variables, with standard errors adjusted for heteroskedasticity and correlation in the errors across factors within an industry. The instruments are the current and lagged ranks of the regressor in levels. See notes to Table 7 for additional details.

Table 9: EIV Regressions Testing For Relative FPE

State	Quantity	HSDO	HSG	SC	CG
New York	β	0.921	0.854	0.965	0.878
	σ	0.082	0.067	0.074	0.143
	p-value for β=1	0.340	0.036	0.640	0.401
	Reliability	0.977	0.953	0.990	0.960
New Jersey	β	0.968	0.854	1.033	0.994
	σ	0.227	0.205	0.280	0.260
	p-value for β=1	0.890	0.483	0.908	0.983
	Reliability	1.000	1.000	1.000	0.997
Illinois	β	0.773	0.979	0.979	1.047
	σ	0.079	0.069	0.106	0.098
	p-value for β=1	0.007	0.766	0.843	0.637
	Reliability	0.967	0.997	1.000	1.000
Florida	β	0.901	0.917	0.957	1.043
	σ	0.148	0.173	0.195	0.226
	p-value for β=1	0.510	0.635	0.825	0.851
	Reliability	0.997	0.989	0.994	0.991
Texas	β	0.969	0.979	0.946	1.018
	σ	0.123	0.151	0.147	0.147
	p-value for β=1	0.806	0.889	0.715	0.901
	Reliability	1.000	1.000	1.000	1.000
California	β	0.857	1.038	0.935	0.928
	σ	0.101	0.134	0.128	0.141
	p-value for β=1	0.168	0.776	0.615	0.614
	Reliability	0.975	1.000	0.964	0.927
Massachusetts	β	1.129	0.889	0.874	0.774
	σ	0.149	0.145	0.148	0.176
	p-value for β=1	0.391	0.449	0.402	0.209
	Reliability	1.000	0.999	0.978	0.978
Ohio	β	0.912	0.891	0.826	0.866
	σ	0.091	0.094	0.117	0.193
	p-value for β=1	0.338	0.252	0.145	0.492
	Reliability	0.987	0.970	0.961	0.991
Michigan	β	0.960	1.039	1.141	0.953
	σ	0.161	0.090	0.097	0.203
	p-value for β=1	0.806	0.667	0.156	0.818
	Reliability	1.000	0.989	1.000	1.000
North Carolina	β	0.853	1.278	1.211	0.953
	σ	0.174	0.157	0.174	0.206
	p-value for β=1	0.405	0.085	0.234	0.823
	Reliability	1.000	1.000	1.000	1.000
Georgia	β	0.774	0.754	0.786	0.682
	σ	0.111	0.111	0.156	0.168
	p-value for β=1	0.049	0.034	0.178	0.066
	Reliability	0.986	0.985	0.993	0.981
Washington	β	0.759	0.848	0.766	1.535
	σ	0.168	0.149	0.171	0.199
	p-value for β=1	0.160	0.301	0.181	0.011
	Reliability	1.000	0.994	0.990	1.000

Notes: These results come from estimating equation (8) on each factor-state using errors-in-variables techniques. "Reliability" is the estimated ratio of the variances for the true and observed values of the regressor. See notes to Table 7 for other details.

Figure 1
Relative FPE for California:
Changes in Unit Labor Requirements 1980-1990, Control Group vs. California



Notes: Each graph plots the OLS regression of equation (8) for one of the four factors for California, where the control group is 14 large states other than California. Changes in log unit labor requirements for the control group are on the horizontal axis; changes in log unit labor requirements for California are on the vertical axis. The line in each graph is the 45-degree line passing through the origin.