COMMENT: ON ESTIMATING ELASTICITIES OF SUBSTITION

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1. Introduction

The literature that attempts to measure the labor market impact of immigration grew explosively in the past decade. This growth was accompanied by a noticeable methodological shift. The more recent studies no longer represent atheoretic attempts to measure the correlation between wages and immigration-induced supply shocks. Beginning with the introduction of the nested CES framework into the immigration context by Borjas (2003), the recent literature explicitly strives to estimate the various elasticities of substitution that are mainly responsible for the presence (or absence) of an impact of immigration on the native wage structure. The papers by Ottaviano and Peri (2011) and Manacorda, Manning, and Wadsworth (2011) in this issue represent additional efforts to estimate the relevant elasticities in the American and British contexts, respectively.

Despite the closer link between the empirical work and the underlying factor demand theory in recent work, there is still a great deal of disagreement on whether immigration has an impact on the earnings of native-born workers. As an example, Card (2009) argues that the effects of immigrants on US wages are small, whereas Borjas (2003) and Aydemir and Borjas (2007) suggest the opposite, that recent immigration has reduced US wages, particularly for low-skilled natives.

Card (2009) notes that there are two elasticities of substitution whose magnitudes are at the core of the debate. The first is the elasticity of substitution between similarly skilled immigrants and natives: “If immigrants and natives in the same skill group are imperfect substitutes, the competitive effects of additional immigrant inflows are concentrated among immigrants themselves, lessening the impacts on natives” (Card 2009, p. 2). The impact on natives could be further weakened if the elasticity

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of substitution between high school graduates and high school dropouts were high. Card (2009, p. 2) continues: "Workers with less than a high school education are perfect substitutes for those with a high school education. This conclusion is important because it means that the impact of low-skilled immigration is diffused across a wide segment of the labor market (the roughly 60% of the population who are counted as 'high school equivalents' workers) rather than concentrated among the much smaller dropout population." The joint findings of imperfect substitution between equally skilled immigrants and natives *and* perfect substitution between high school graduates and high school dropouts would then imply that the impact of immigration on the earnings of native-born workers was small, despite high immigration rates and downward-sloping labor demand curves.

There is already empirical disagreement on whether similarly skilled immigrants and natives are imperfect substitutes. Ottaviano and Peri (2011; hereafter, OP) find evidence of imperfect substitution in the United States, estimating an elasticity of substitution of around 20. In contrast, Jaeger (1996, revised 2007), Borjas, Grogger, and Hanson, (2008, 2010), and Aydemir and Borjas (2007) estimate an effectively infinite elasticity and conclude that equally skilled natives and immigrants are perfect substitutes.

There is less evidence on the value of the elasticity of substitution between high school dropouts and high school graduates. Although these two groups are often grouped together as "high school equivalents" in the wage structure literature, there are few explicit tests of this hypothesis. Goldin and Katz (2008) use national-level CPS data and find imperfect substitution between the two groups, while Card (2009), using a spatial correlation approach, concludes that the groups are perfect substitutes.

In this paper, we revisit the estimation of these elasticities in the US context. To focus squarely on modeling and estimation issues, we use the same data as OP. Nevertheless, our results are quite different. In terms of the elasticity of substitution between equally skilled immigrants and natives, we conclude that the OP data, correctly analyzed, imply that the two groups are perfect substitutes. In fact, by using a statistically valid set of regression weights and by defining the earnings of a skill group as the mean log wage of the group (rather than the unconventional log mean wage used by OP), we find that the OP data reveal an effectively infinite substitution elasticity. The evidence thus implies that native workers are exposed to adverse effects from immigration-induced increases in labor supply.

In terms of the elasticity of substitution between high school dropouts and high school graduates, our findings are more cautionary. Much depends on how one controls for (unobserved) changes in demand that may have differentially affected high school dropouts and high school graduates (an issue that does not arise in estimating immigrant-native substitutability, as in that case one examines wage changes within skill groups). The use of the CPS wage time series created by Autor, Katz, and Kearney (2008) and employed by Goldin and Katz (2008) leads to the conclusion that the two groups are imperfect substitutes as long as one accepts the ancillary assumptions about trends in relative demand that are maintained in those well-known studies. However, deviations from those assumptions yield unstable estimates. In some cases, the regressions imply negative (and significant) elasticities, leading to a rejection of the CES framework. Our take on the evidence, therefore, is simple: Proceed with care. The CES framework is sensitive to ancillary (and untestable) assumptions about changes in the relative demand for skill groups, complicating its use as a method for determining whether high school graduates and high school dropouts are perfect substitutes.

2. Data

We draw our data from the same sources as OP and use the same rules for constructing the sample extracts and defining the variables.[1] Our replication yields identical summary statistics as OP; the only exception being the data drawn from the 1960 Census, where our counts of workers and average wages in the various cells differ slightly.

We classify workers into the same four education groups (high school dropouts, high school graduates, workers with some college, and college graduates), and into the same eight experience groups (1–5 years of experience, 6–10 years, and so on). These education and experience categories define a total of 32 skill groups at a point in time.

We impose the same sample restrictions as OP on the individual-level Census observations. The sample includes individuals who are not residing in group quarters, are aged 18–64 (inclusive), worked at least one week in the calendar year prior to the Census, and have between 1 and 40 years of work experience (inclusive). The wage is initially measured as average weekly wage and salary income in the skill group; the count of workers in a cell is measured as total annual hours worked by all workers in that cell. We calculate wages and counts separately for immigrants and natives. OP intended to exclude the self-employed from the calculation of wages (since self-employed earnings include the return to capital and other non-labor inputs) and to include them in the count of workers (since the self-employed affect labor supply). While adapting OP's coding for our exercise, however, we noted that OP failed to exclude the self-employed from their calculation of weekly earnings due to a coding error. We follow OP's intended practice of excluding the self-employed from the wage calculation. Below, we describe how this discrepancy between our wage measure and OP's affects the results.

1. Specifically, we use data drawn from the 1960–2000 decennial Censuses, and the 2006 American Community Survey. We do not wish to imply that the OP construction of the sample is optimal. One particular feature creates a bias in favor of imperfect substitution between equally skilled immigrants and natives. The OP sample contains many native-born workers who are 18 years old, are seniors in high school, and (due to the timing of the Census) will receive their high school diplomas within 8 to 12 weeks. Nevertheless, these young adults are classified as high school dropouts. See Borjas, Grogger, and Hanson (2008) for details.

3. The Fragility of Within-Group Immigrant–Native Complementarity

We first test for perfect substitution between equally skilled immigrants and natives. Let L^n_{jkt} be total hours worked by persons in education group j, experience group k, year t, and nationality group n (F = foreign-born workers, D = native-born workers). The nested CES framework implies that the total labor input in an education–experience cell is given by $[\psi_{jkt}(L^F_{jkt})^\lambda + (1 - \psi_{jkt})(L^D_{jkt})^\lambda]^{\frac{1}{\lambda}}$, an Armington aggregator of the total supply of foreign- and native-born workers in that cell, with ψ_{jkt} being a technology parameter that captures the (potentially) time-varying relative productivity of immigrants and natives. The elasticity of substitution is $\sigma_N = 1/(1 - \lambda)$. The assumption that the wage equals the value of marginal product implies that we can test the perfect substitution hypothesis by relating the log relative wage of immigrants in a particular skill group to the log relative supply of immigrants in that group:

$$\ln\left(\frac{w^F_{jkt}}{w^D_{jkt}}\right) = \phi_{jkt} - \frac{1}{\sigma_N}\ln\left(\frac{L^F_{jkt}}{L^D_{jkt}}\right), \tag{1}$$

where $\phi jkt = \ln\,[\psi^{jkt}/(1 - \psi^{jkt})]$. A large value for σ_{N} suggests that immigrants and native workers are close to perfect substitutes, and relative wages would then be uncorrelated with relative quantities. We approximate ϕjkt with fixed effects for education–experience groups, education–year groups, and experience–year groups, whose inclusion we vary across specifications. As in OP, we estimate the regression separately in samples that include either all workers or only full-time workers, and either only men or men and women combined.[2]

Table 1 reports our estimation results for equation (1) using the Census–ACS data described earlier. Our column (1) of row (1) corresponds to column (1) of row (1) of panel A in OP's Table 2. In this specification, which is for male workers, the only regressor is the log ratio of immigrant and native hours worked in a cell. Our coefficient estimate for $-1/\sigma N$ is -0.046 (with a standard error of 0.008), as compared to OP's estimate of -0.053 (0.008). The difference is due entirely to OP failing to exclude the self-employed (as they had intended to do) in calculating the average weekly wage of the group. Omitting the self-employed causes the estimated elasticity of substitution to rise from OP's estimate of 18.8 to 21.7.[3]

Moving down column (1), we first address the definition of the weights used in the regression. OP weight the regressions by total employment in an education–experience cell. However, the motivation for weighting arises from the concern that there may be differences across observations in the sampling error of the wage ratio used as the

2. As in OP, the regression in the male sample correlates male wages to male employment counts, while the regression in the male-female sample correlates the average wage in the population to total (i.e. male plus female) employment counts.

3. From equation (1), a substitution elasticity of 20 implies that an increase in the relative supply of immigrant workers by 10 log points reduces the relative wage of immigrant workers by 0.48%; at an elasticity of 40 the wage impact is 0.23%, and at an elasticity of 50 the wage impact is 0.19%.

TABLE 1. Estimates of the elasticity of substitution between equally skilled immigrants and natives (σ_N).

	All workers				Full-time workers			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1. Log mean wages (OP)								
Male	−0.046	−0.008	−0.021	0.009	−0.056	−0.023	−0.037	0.044
	(0.008)	(0.012)	(0.014)	(0.025)	(0.006)	(0.007)	(0.011)	(0.026)
Male and female	−0.029	−0.009	−0.015	−0.030	−0.043	−0.027	−0.032	0.022
	(0.008)	(0.013)	(0.015)	(0.031)	(0.006)	(0.008)	(0.012)	(0.022)
2. Log mean wages, correct weighting								
Male	−0.030	−0.021	−0.013	0.006	−0.042	−0.036	−0.026	0.015
	(0.009)	(0.013)	(0.011)	(0.029)	(0.004)	(0.007)	(0.009)	(0.030)
Male and female	−0.021	−0.023	−0.011	−0.023	−0.037	−0.042	−0.027	0.002
	(0.009)	(0.012)	(0.012)	(0.030)	(0.005)	(0.007)	(0.009)	(0.025)
3. Mean log wage, correct weighting								
Male	−0.008	0.008	0.008	0.009	−0.033	−0.020	−0.009	0.030
	(0.017)	(0.021)	(0.013)	(0.034)	(0.005)	(0.007)	(0.009)	(0.031)
	[0.001]	[0.103]	[0.768]	[0.906]	[0.175]	[0.318]	[0.690]	[0.865]
Male and female	−0.002	0.004	0.001	−0.034	−0.029	−0.027	−0.015	0.006
	(0.015)	(0.018)	(0.012)	(0.036)	(0.005)	(0.007)	(0.009)	(0.024)
	[−0.005]	[0.038]	[0.787]	[0.920]	[0.163]	[0.261]	[0.758]	[0.938]
Fixed effects:								
Year	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Education × experience	No	No	Yes	Yes	No	No	Yes	Yes
Education × year	No	No	No	Yes	No	No	No	Yes
Experience × year	No	No	No	Yes	No	No	No	Yes

Notes: Robust standard errors are reported in parentheses, and are clustered by education–experience cells; adjusted R-squared reported in brackets. The OP weights in row 1 are total employment in the cell; the weights in rows 2 and 3 are given by the inverse of the sampling variance of the dependent variable.

TABLE 2. Elasticity of substitution between education groups.

	CPS		Census–ACS		
Groups:	High school graduates and:		High school dropouts and high school graduates		
Specification	1. High school dropouts	2. College graduates	3. National level	4. MSA level	5. State level
1. Linear trend, post-1992 spline	−0.135 (0.027) [0.968]	−0.619 (0.067) [0.940]	−0.260 (0.028) [0.995]	—	—
2. Linear trend only	−0.023 (0.012) [0.945]	−0.326 (0.043) [0.906]	−0.103 (0.025) [0.930]	—	—
3. Quadratic trend	0.101 (0.045) [0.954]	−0.535 (0.113) [0.912]	−0.008 (0.054) [0.970]	—	—
4. Cubic trend	−0.047 (0.012) [0.978]	−0.529 (0.096) [0.942]	−0.063 (0.053) [0.980]	—	—
5. Linear trend, best-fitting spline	0.119 (0.015) [0.986]	−0.604 (0.063) [0.942]	—	—	—
6. Period fixed effects	—	—	—	−0.024 (0.016) [0.760]	−0.069 (0.025) [0.554]
7. State/MSA effects × linear trend	—	—	—	−0.037 (0.026) [0.868]	−0.119 (0.011) [0.830]
8. State/MSA effects × linear trend and post-1992 spline	—	—	—	0.141 (0.074) [0.942]	−0.152 (0.028) [0.844]
Includes state or MSA fixed effects	—	—	—	Yes	Yes
Number of observations	43	46	6	1071	306

Notes: Robust standard errors are reported in parentheses, and are clustered at the MSA or state level in columns 4 and 5; adjusted R-squared reported in brackets. Column 1 uses the Goldin–Katz (2008) CPS data from 1963 to 2005; column 2 uses the Acemoglu–Autor (2010) CPS data from 1963 to 2008; and columns 3–5 use the sample of full-time male and female workers in the Census–ACS (from 1960–2006 in columns 2 and 4, and 1980–2006 in column 3). The best-fitting spline in the Goldin–Katz data begins in 1979, while the best-fitting spline in the Acemoglu–Autor data begins in 1993. The regressions reported in columns 1 and 2 are unweighted; all other regressions are weighted by the inverse of the variance of the dependent variable.

dependent variable. As defined by OP, the dependent variable in equation (1) is the log difference between the average wages of immigrant and native workers in a cell. The appropriate weight is the inverse of the sampling variance for an observation. Assuming independent sampling, this is given by

$$\omega_{jkt} = \left\{ \mathrm{var}\left[\ln\left(\frac{\bar{w}^F_{jkt}}{\bar{w}^D_{jkt}} \right) \right] \right\}^{-1} = \frac{N^F_{jkt}(\bar{w}^F_{jkt})^2 N^D_{jkt}(\bar{w}^D_{jkt})^2}{(\theta^F_{jkt})^2 N^D_{jkt}(\bar{w}^D_{jkt})^2 + (\theta^D_{jkt})^2 N^F_{jkt}(\bar{w}^F_{jkt})^2}, \quad (2)$$

where $(\theta^n_{jkt})^2$ is the variance of the underlying microdata of wages (in levels) for cell (j, k, t, n); $\bar{w}^n_{jkt}$ is the average wage for the cell; and N^n_{jkt} is the number of observations. Row (2) of column (1) in Table 1 shows that the coefficient estimate of $-1/\sigma N$ falls to -0.030 (0.009) simply by using the correct weights defined in equation (2), implying a substitution elasticity of 33 for men and 48 for the combined sample of men and women.

If the wage data have a common coefficient of variation, π, across all cells, the weights simplify to

$$\omega_{jkt} = \frac{1}{\pi^2}\left[\frac{N^F_{jkt} N^D_{jkt}}{N^F_{jkt} + N^D_{jkt}} \right]. \quad (2')$$

If $N^D_{jkt} = \gamma N^F_{jkt}$ for all j, k, and t, one obtains $\omega_{jkt} = \gamma\pi^{-2}(1+\gamma)^{-2}[N^F_{jkt} + N^D_{jkt}]$, the OP case where the weights are proportional to total employment in the cell.[4] However, this requires immigrants to be represented in equal proportion in all cells, in which case the regression in equation (1) would say nothing about the effect of immigration on wages.

Next, we address the issue of how cell-specific wages are calculated. As noted above, OP use the log of mean weekly earnings for a particular nationality, skill group, and year. The standard approach in the literature, however, is to use the mean of log earnings (e.g. Katz and Murphy 1992; Card 2001; Card and Lemieux 2001; Borjas 2003; Lemieux 2006; Autor, Katz, and Kearney 2008). By Jensen's inequality, the log mean wage and the mean log wage are not the same. The mean log wage corresponds to the geometric mean wage, which has the interpretation of a marginal product index in which weights are each worker's share of total hours worked. Log mean wages have no such interpretation. If the dependent variable in equation (1) were the difference in mean log immigrant and mean log native wages, the appropriate sampling weight is given by

$$\omega_{jkt} = \left[\frac{\sigma^2_{w,Fjkt}}{N^F_{jkt}} + \frac{\sigma^2_{w,Djkt}}{N^D_{jkt}} \right], \quad (2'')$$

where $\sigma^2_{w,njkt}$ is the variance in log wages for cell (j, k, t, n). Row (3) of column (1) shows that using average log wages and the correct weight yields a regression coefficient of -0.008 (0.016). The coefficient is no longer statistically significant, and

4. The OP weight is the total number of hours employed by immigrants and natives in the cell, rather than the total number of observations in the cell. The two would be perfectly correlated if Census sampling was proportional to the population and hours worked were constant across workers.

has an implied substitution elasticity of 125 for men and 500 in the combined sample of men and women. In short, the presumed complementarity between equally skilled immigrants and natives found in the simple specification favored by OP disappears when one uses a more common measure of the wage gap between immigrants and natives and applies a statistically valid sampling weight.

Finally, we turn to the issue of which fixed effects should be included in the regression model. Because the dependent variable in equation (1) is the difference in immigrant–native log wages, factors that affect immigrant and native labor demand equally are automatically removed from the equation. Remaining factors that affect immigrant–native relative wages include differences in the composition of immigrants and natives within skill groups and how these differences evolve over time (as well as how relative labor demand for the groups shifts as the sample composition changes). Consider immigrant and native high school dropouts. Native high school dropouts typically quit school during their high school years. In contrast, immigrants in the high school dropout category include sizable numbers of individuals with no or little schooling (reflecting differences in educational standards in the immigrants' countries of origin). Such compositional differences, which change over time with changes in immigrants' countries of origin, may produce a spurious correlation between the observed immigrant-native wage ratio and the observed immigrant–native employment ratio.

One solution to changes in within-group skill composition is to control for skill-group and period fixed effects. Columns (2) and (6) of Table 1 introduce the period fixed effects. In row (3), the regression coefficient is 0.008 (0.021) for all male workers and −0.020 (0.007) for full-time make workers, which implies effectively infinite substitution elasticities. Note that the adjusted R-squared rises from 0.001 to 0.103 in the regression for male workers, supporting the inclusion of the period dummies. Adding education–experience dummies, in columns (3) and (7), and year–education group and year–experience group controls, in columns (4) and (8), yields similar results and increases the adjusted R-squared to 0.920. In other words, there is ample statistical ground for including the fixed effects in the regression.

In sum, the results reported in Table 1 show that OP's finding of imperfect substitution between immigrant and native workers is fragile. Reading across the columns in Table 1, the simple inclusion of period fixed effects is generally sufficient to drive most of the regression coefficients to zero, yielding infinite substitution elasticities. Moreover, even if we do not include any fixed effects, we obtain effectively infinite substitution elasticities by correcting for errors in the OP specification: failure to exclude the self-employed from the calculation of mean wages, the use of log mean wages instead of mean log wages, and the use of inappropriate regression weights. Each of these errors has the effect of making the correlation between relative wages and relative quantities more negative, thus making substitution between immigrants and natives appear more imperfect than it really is.

4. Are "High School Equivalents" Equivalent?

The aggregation of workers into a manageable number of education groups is a crucial step in any empirical analysis of the impact of immigration. In the United States, immigration has increased the size of specific groups, such as high school dropouts and workers with post-college degrees. Hence the literature has focused on estimating the impact of immigration on these narrowly defined groups. In contrast, the wage structure literature (Katz and Murphy 1992) finds it convenient to discuss long-term trends in the returns to skills by examining the wage gap between two broadly defined education categories, high school equivalents (defined as an aggregate of high school dropouts and high school graduates), and college equivalents (defined as an aggregate of those with education beyond high school).

Card (2009), Ottaviano and Peri (2011) and Manacorda, Manning, and Wadsworth (2011) all suggest that the high school equivalence–college equivalence breakdown should be adopted in the immigration literature. Of course, whether such an adoption is warranted is an empirical question, depending on the value of the elasticity of substitution between high school dropouts and high school graduates.[5] The Armington aggregator that defines high school equivalents is $[\varphi_t L_{1t}^{\delta} + (1-\varphi_t) L_{2t}^{\delta}]^{1/\delta}$, where L_{1t} and L_{2t} give the labor supply of high school dropouts and high school graduates at time t, respectively; the elasticity of substitution between the two groups is $\sigma_E = 1/(1-\delta)$; and φ_t is time-varying technology parameter that captures the relative productivity of the two education groups.[6] Assuming that wages (w_{jt}) equal the value of marginal product, the elasticity of substitution is identified by the regression

$$\ln\left(\frac{w_{2t}}{w_{1t}}\right) = \rho_t - \frac{1}{\sigma_E}\ln\left(\frac{L_{2t}}{L_{1t}}\right), \tag{3}$$

where $\rho^t = \ln[\varphi^t/(1-\varphi^t)]$. Despite the superficial similarity of equations (1) and (3), they differ radically in terms of the data that one can use to estimate them. With 32 skill groups, there are 32 observations per year to estimate σ_{N} in equation (1). As the dependent variable in equation (1) is the difference in log wages *within* skill groups, demand and productivity shifters are removed from the regression.[7] In contrast, there is only one observation per year to estimate σ_E in equation (3) in national-level data. The dependent variable is the difference in log wages *between* skill groups, which implies that demand and productivity shifters remain in the regression. With only one

5. It also depends on the value of the elasticity of substitution among workers who have more than a high school diploma. This elasticity, however, plays a less crucial role in calculations of the wage impact of immigration in the United States.

6. In theory, the quantities L_{1t} and L_{2t} represent the number of *efficiency units* provided by all workers in each of these education groups. For example, if high school graduates in different experience groups are not perfect substitutes, the aggregation of the various groups into L_{2t} would make use of the estimated elasticity of substitution across experience groups (by using the appropriate Armington aggregator). As noted by OP (p. 22), the results barely change if we use instead the total number of hours worked by persons in the cell. We opt for the simpler specification to allow for easy replication of our results.

7. As we have argued, controlling for period effects may still be warranted if the composition of immigrants and natives within skill groups changes over time.

observation per year one cannot control for these terms nonparametrically; additional assumptions are required.

Most studies in the wage structure literature use the annual CPS to estimate σ_E.[8] Katz and Murphy (1992) introduce the assumption that the period effects can be approximated by a linear trend. More recent studies argue that there was a slowdown in the growth in demand for more skilled workers after 1990 (Autor, Katz, and Krueger 1998; Card and DiNardo 2002). As a result, Goldin and Katz (2008) and Autor, Katz, and Kearney (2008) expand the original linear trend specification by adding a spline beginning in 1992 (defined as a trend variable taking on the value of 0 in 1992, 1 in 1993, and so on).[9]

To analyze the sensitivity of the estimates of σ_E, we obtained the Goldin–Katz time series on relative wages and relative quantities for high school dropouts and high school graduates.[10] The coefficients reported in row 1 of the first column of Table 2 estimate the elasticity using the Goldin–Katz specification. The estimated coefficient is -0.135 (0.023), rejecting the hypothesis that the two groups are perfect substitutes. The implied elasticity of substitution is 7.4. Column 3 of the table re-estimates the model using the national-level Census–ACS data introduced in the previous section.[11] Although there are only six observations in the regression, the estimated coefficient is negative and significant. The implied estimate of σ_E is 3.8.

The remaining specifications in Table 2 are based on alternatives to the linear-trend-plus-post-1992-spline approximation to the relative change in demand for more skilled workers. The estimates of σ_E are sensitive to how the regression controls for the unobserved variation in relative demand. We first adopt the original Katz–Murphy linear trend assumption, excluding the post-1992 spline used in the later Goldin–Katz study. The estimated elasticity is now equal to 43.5 in the CPS data, in line with the conclusion that high school dropouts and high school graduates are highly substitutable. Ottaviano and Peri (2011, Table 5, column 2) also make use of the CPS data and impose this linear trend assumption. Their reported estimate of the regression coefficient is similar to that reported in row 2 of Table 2.

The next two rows of the table show the sensitivity of the elasticity estimates to the adoption of alternative trend specifications. The use of a quadratic trend leads to a rejection of the CES framework in the Goldin–Katz data. The implied elasticity of substitution is statistically significant and sizable, but it has the wrong sign. Similarly,

8. These studies, however, estimate the elasticity of substitution between high school equivalents and college equivalents, rather than the elasticity between high school dropouts and high school graduates.

9. Although the Goldin–Katz discussion specifically refers to the wage gap between high school dropouts and high school graduates, the Autor–Katz–Kearney analysis, which suggests the use of the same spline, refers to high school equivalents and college equivalents.

10. These are the data that underlie the analysis reported in Goldin and Katz (2008, Table 8.4, page 306). Autor, Katz, and Kearney (2008, pp. 322–323) provide a detailed description of the construction of the data. Goldin and Katz also use a few historical data points prior to 1963; we restrict our analysis to the post-1963 period, where modern CPS data are available. We are grateful to Larry Katz for providing us with the data file.

11. We use the mean log wage of natives as the measure of the productivity in the cell and use the correct sampling weight in the regression.

the use of a cubic trend leads to further instability. Row (5) shows the result from the best-fitting linear-trend-plus-spline specification. The best-fitting spline starts in 1979, which is not surprising in light of the wage trends in the underlying Goldin–Katz data.[12] However, the regression coefficient is positive and significant, at odds with the CES framework.

The sensitivity of estimates of σ_E to the assumed shape of the trend is not representative of what is usually found in the wage structure literature. Most studies estimate the elasticity of substitution between high school graduates and college graduates. To illustrate the robustness of those results, we use the Acemoglu and Autor (2010) wage and employment series and conduct the same set of sensitivity tests. Column 2 of Table 2 shows that the estimated elasticity between college equivalents and high school equivalents is fairly robust to functional form assumptions. In fact, the best-fitting spline begins in 1993, suggesting that it is unwise to transfer insights obtained from measuring the substitutability between two particular education groups to the estimation of the substitutability between two other groups. More importantly, the instability demonstrated in column 1 of the table is not inherent in the CES approach or in the use of time series data—rather, it is limited to estimates of the elasticity of substitution between high school dropouts and high school graduates, a parameter of particular interest in the US immigration context.

Card (2009) introduces an approach that would seem to avoid the pitfalls inherent in making untestable assumptions about the underlying trends in relative demand. He exploits variation in relative wages and quantities across local labor markets (rather than national wage trends) to estimate σ_E. He finds strong evidence of substitutability between high school graduates and high school dropouts. Column 4 of the table uses the 1980–2006 Census–ACS data to confirm Card's results by defining a cell in terms of metropolitan area r, education j, and time t. The benefit of this approach is that it would seem to eliminate the need to make arbitrary assumptions about the underlying trends in relative demand. Since there are now many observations in each period (for different localities), the regression can include period fixed effects. Row 6 shows that the regression coefficient estimated using MSA-based cells is -0.024 (0.016). The coefficient is not statistically significant, suggesting that high school dropouts and high school graduates are perfect substitutes.

However, there are likely to be geographic differences in the evolution of relative demand for high school dropouts and high school graduates and MSA and period fixed effects do not absorb such differences.[13] Hence, the need arises to make ancillary

12. There was little change in the wage gap between the two groups until about 1980, at which time it began to rise sharply (Goldin and Katz 2008, Table 8.3).

13. An analogous issue may arise in the estimation of the elasticity of substitution between equally skilled immigrants and natives. There may be trends that are specific to a particular education-experience cell, and that cannot be captured by the set of fixed effects included in the regressions reported in Table 1. The inclusion of interactions between the education–experience fixed effects and a linear trend in column 2 for the sample of working men leads to a coefficient of 0.007 (0.021). Further, adding interactions with the post-1992 spline leads to a coefficient of 0.010 (0.024). Thus, accounting for group-specific trends does not change the conclusion that equally skilled immigrants and natives are perfect substitutes.

assumptions about the MSA-specific trends in relative demand. Rows 7 and 8 of Table 2 illustrate what happens to the estimates when the MSA fixed effects are interacted either with a linear trend or with the Goldin–Katz specification. The adjusted R-squared rises sharply when these terms are added to the model. The estimated coefficients, as in the national-level data, are unstable, and in the case of the Goldin–Katz specification, reject the CES framework.

The last column of Table 2 reports results when we use states as the geographic definition of the labor market. The coefficient in row 6, based on a regression that includes only state effects and year effects, yields an estimated elasticity of 14.5, rejecting the conjecture that high school dropouts and high school graduates are perfect substitutes. It is noteworthy that the estimated elasticity becomes larger the smaller the geographic area that defines the labor market: it is 7.4 in the national CPS data (using the Goldin–Katz specification), 14.5 at the state level, and 41.7 at the MSA level. This finding mirrors the well-known result that wages and immigration are more closely linked the larger the size of the geographic labor market.

To conclude, the existing evidence on the elasticity of substitution between high school dropouts and high school graduates is sensitive to the assumptions one makes about the time path of unobserved shocks to relative demand and relative productivity for high school dropouts and high school graduates. The use of geographic variation introduces the need to control for the unobserved dispersion in the evolution of relative demand across localities, and reintroduces conceptual issues (that remain unresolved in the literature) about how local labor markets adapt to an immigration-induced supply shift. Table 2 teaches a simple lesson: Different assumptions yield different conclusions about whether high school equivalents are, in fact, equivalent. Whereas the CES framework is useful for analyzing the substitutability of labor within skill groups, as shown in Table 1, it is perhaps less useful for analyzing the substitutability of labor between these two particular skill groups, as seen in Table 2.

This may be an unsatisfying interpretation of the evidence. Nevertheless, we believe that despite the importance of determining the extent of substitution between high school dropouts and high school graduates, it may be unwise to reach *any* conclusion about the magnitude of the elasticity from CES-style relative wage regressions. The nature of the underlying trends in demand for the two groups cannot be established; the assumption of a linear trend (or a post-1992 spline) is arbitrary; and the introduction of geographic variation leads to a menu of pick-and-choose results depending on the level of geographic aggregation.

In an important sense, the results in Table 2 illustrate the limits of what the nested CES framework can teach us about the labor market impact of immigration. Aggregating or disaggregating education groups can make the estimated impact as small or as large as one would like. Yet, there is no convincing empirical evidence that indicates how best to pool. Put bluntly, the instability of the estimates of the elasticity that allow us to define a skill group of *high school equivalents* in the United States should serve as a word of caution in this context and in related studies.

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