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Abstract

The world's top destination country for migrant workers, the United States, is enacting a set of unprecedented taxes on high-skill immigration that include charging employers roughly $100,000 for each foreign worker they hire using the H-1B visa. The US government has relied on one academic study to design the tax and evaluate its impact on high-skill immigration under the H-1B quota, an impact the study estimates at zero by measuring the wage differences between H-1B and native workers. This paper precisely replicates the study's method and finds that its conclusion rests on three major flaws, each of which renders its results difficult to interpret. The study mismeasures the wage gap between H-1B and native workers, misinterprets any negative wage gap as reflecting markdowns rather than task specialization, and carries the wage results into a policy simulation whose central estimates are invariant to the wage gap: they predict an identical (null) effect on immigration for any H-1B wage gap including zero. Improved methods are required to offer regulators useful information on the effects of this large shift in immigration policy.

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1 Introduction

Governments worldwide limit the quantity of foreign labor supply with immigration quotas. While some economists have long proposed a shift toward regulating the *price* of foreign labor with immigration taxes, such taxes have been traditionally rare.[1] Recently that has changed. The government of the world's top migrant-destination country, the United States, has proposed per-worker taxes of $100,000 on its largest channels of high-skill immigration: the quota-capped H-1B visa (for college-educated specialist workers) and the uncapped OPT work permit ('Optional Practical Training', for foreign graduates of US universities).[2]

How will the new taxes affect high-skill immigration? Very little research directly evaluates these impacts. Evidence is important given the first-order effects of high-skill immigration in general, and via the H-1B visa specifically, on productivity growth in the United States (e.g. Peri et al. 2015; Bound et al. 2017; Mahajan et al. 2025; Abramitzky et al. 2026) and even globally (Khanna and Morales 2026).

This paper discusses fundamental problems in estimating the impact of these new taxes, problems instantiated by the sole academic study that the US government has used to design and evaluate them. Borjas (2026a) compares the relative starting salaries for foreign workers on H-1B visas to total labor income for a group of US native workers, finding a gap of −16 percent. That paper's method interprets any negative H-1B wage gap as a wage markdown, "payroll savings" for their employers due to visa-driven monopsony power (Manning 2003). It then simulates the effect of the enacted $100,000 H-1B tax on high-skill immigration, finding that the tax does not reduce employers' usage of the H-1B visa quota but only transfers their markdown rents to the government. Thus it concludes that the tax has little if any immigration effect.

This analysis focuses on that paper because it has uniquely and directly informed the taxes'

[1] See for example Becker (1987); Freeman (2006); Peri (2012, 17). Related proposals for price-based regulation of immigration focus on auctions and tradable permits (Orrenius and Zavodny 2010; Fernández-Huertas Moraga and Rapoport 2014; Auriol and Mesnard 2016; Orrenius and Zavodny 2020; Lokshin and Ravallion 2022).

[2] The OPT tax, targeted at international student graduates of US universities wishing to work temporarily in the United States after graduation and often en route to an H-1B work visa, has at the time of writing been approved by the White House (Proposed Rule by the Dept. of Homeland Security, RIN 1653-AB01, "Optional Practical Training Fees", review by the Office of Management and Budget OIRA concluded on September 11, 2026), but has not yet been publicly released.

creation. US President Donald Trump first proclaimed a $100,000 H-1B tax in 2025, and the 2026 update of the H-1B tax (to $103,265) rests its estimate of the tax's economic impact on that one academic study.[3] The rapid deployment of unprecedented policy change guided by this paper invites heightened scrutiny of the paper's methods.

The evidence presented here indicates that the study rests on flaws of three types:

1. *Measurement*: The method inflates wage differences between H-1B workers and comparable natives. This bias arises from errors in combining administrative data on *starting base salary* with survey data on *mid-tenure comprehensive wage income*, errors in assigning to immigrants the native returns to human capital, and errors in the imputation of missing data. The present paper shows how to compare workers with the same salary concept, the same tenure, and the same returns to human capital. The most conservatively negative corrected estimates yield a wage gap between H-1B workers and comparable natives that is statistically indistinguishable from zero, with a point estimate of −0.7 percent (±10.2 percentage points). That figure does not include an additional positive correction of +4.5 percentage points in order to compare workers of equal seniority (because some part of it may overlap with the correction for using different pay concepts). In the handful of occupation-industry cells with extremely high concentrations of H-1B workers, the corresponding gap is similarly a statistical zero with a point-estimate of −5.1 percent, which likewise does not incorporate any correction for seniority differences. The evidence is consistent with a small positive or small negative wage gap, which cannot be estimated exactly because some of the necessary corrections overlap.

2. *Comparison*: Wage gaps of any size do not constitute "savings" to employers (markdowns) when they are estimated by comparing workers of different productivity, such as due to task specialization. But the method assumes that any negative H-1B gap is exclusively a wage markdown, an assumption without which it can neither measure employers' "savings" nor predict their response to the tax. No basis is offered for this assumption, which is contradicted both by a large literature in modern labor economics and by evidence from the

[3]The September 2025 H-1B tax proclamation of $100,000 expired after one year (EOP 2025). Weeks before it expired, in August 2026, the tax was updated to $103,265 as a proposed permanent rule by DHS (2026), with Borjas (2026a) as the sole basis in academic research for its economic impact evaluation. The paper's author was a Senior Economist in the Executive Office of the President (CEA) through December 2025.

same datasets used in the paper, including evidence presented in the paper itself. The evidence is consistent with assignment of H-1B and native workers to differently-productive tasks within observed occupation-industry-city cells, a well-known problem (e.g. Dustmann et al. 2013) that is exacerbated by each layer of fixed effects for natives' endogenous sorting outcomes. This implies that the method underestimates employers' response to the tax and thus its effect on immigration (usage of the H-1B quota).

3. *Simulation*: The method predicts a zero effect of the tax on immigration by simulating employers' response, based on its estimated H-1B wage gap. Even if the wage gap were correctly measured and interpreted as a markdown, that prediction arises from a simulation that is fundamentally flawed. The same method's central demand scenario predicts an identical effect of zero at any measured wage gap. It predicts zero effect of the tax on immigration even if the wage gap is zero, that is, even if H-1B workers are paid identically to comparable natives, an outcome that is impossible under the method's assumptions. The method fails in this way because its central policy-relevant prediction is determined by parameters that are assumed without reference to any data. Such a prediction is difficult to interpret.

There is an urgent need for research that overcomes these flaws, because each separately throws into question the ability of the Borjas method to accurately inform policy at this time. This paper begins by summarizing the H-1B visa wage-gap literature and how the Borjas method proposes to use wage gaps to estimate the impact of the tax in Section 2. It explains the three major flaws above, one by one, in Sections 3, 4, and 5, and concludes in Section 6.

2 Evaluating the effects of a tax on the H-1B visa

The H-1B is a nonimmigrant visa that allows US employers to temporarily hire foreign workers in "specialty occupations" requiring at least a bachelor's degree or its equivalent, for an initial period of up to three years that is typically renewable once for a total of six years. It is the largest single channel for work-based high-skill immigration to the United States. Employers must attest in a Labor Condition Application (LCA) that the worker will be paid the greater of what the employer pays its similar US workers, or the prevailing wage for the occupation and

location, and that the hire will not adversely affect working conditions for similarly employed US natives. There is a statutory cap on the number of H-1B visas approved each year for initial employment, set at 85,000 since 2004: 65,000 for the private sector in general, and 20,000 more for workers who hold a graduate degree from a US institution.[4] When demand exceeds the cap, as it has in every year since 2014, a randomized lottery allocates the opportunity to petition for the 85,000 visas (see e.g. Mahajan et al. 2025).

Whether H-1B workers are underpaid relative to comparable US natives has been contested in a sizeable literature. Several survey-based studies find that H-1B holders earn the same as or *more* than observationally similar natives—including in information technology (Mithas and Lucas 2010; Lofstrom and Hayes 2011) and across STEM occupations more broadly (Rothwell and Ruiz 2013; Bagheri 2023). A contrasting strand of work finds H-1B wage penalties on the order of 10 percent or more: Costa and Hira (2020) find that 60 percent of H-1B positions are certified at wage levels below the local occupational median, which does not control for the individual-level determinants of earnings. Bourveau et al. (2025) use payroll data from a top accounting firm matched on office, position, and date, finding H-1B starting salaries roughly 10 percent lower than for US workers, but no negative effect of H-1B hiring on US worker wages—suggesting differences in task assignment within job title.[5]

2.1 Estimating employers' response to a tax: the Borjas method

The first-order effect of an H-1B tax on high-skill immigration is the tax's effect on employers' demand for H-1B workers. The most direct information publicly available about that effect is the observed behavior of US employers in response to the $100,000 H-1B tax imposed in September 2025.[6] Over the next eight months, US employers' petitions for H-1B visas subject to the tax

[4] Certain employers—including institutions of higher education and their nonprofit affiliates, nonprofit research organizations, and government research organizations—are exempt from the cap and may petition for H-1B workers at any time (Section 214(g)(5) of the Immigration and Nationality Act).

[5] A recent strand uses the H-1B visa lottery to study effects of H-1B workers on native employment at the firm level. Mahajan et al. (2025) find that firms allocated an H-1B worker by lottery exhibit either no reduced hiring of native workers or increased hiring, higher revenue, no decline in wages, and higher rates of firm survival, consistent with Kerr et al. (2015). While Mahajan et al. study entrants in the full H-1B lottery, Doran et al. (2022) study a small lottery conducted for an unrepresentative subset of firms that entered the lottery late in the annual H-1B visa cycle.

[6] The Trump Administration (DHS 2026) refers to the H-1B tax as a "fee", the term echoed by Borjas (2026a). The same paper describes it as a compulsory payment that is set in order to "maximize government revenue," which is the definition of a tax, in US law and in the field of public finance. These definitions are reviewed in the Appendix. A fee is a compulsory payment to just offset an agency's cost of providing a specific good or service of private value,

collapsed by 91.2 percent relative to the same period the previous year (USCIS 2026).[7] This observed short-term response, if interpreted as the slope of employers' demand curve, would imply a demand elasticity of −16 (Clemens 2026b, 26). Under such a response, the tax would reduce H-1B visa demand far below the binding visa cap, cutting realized high-skill immigration via the H-1B visa by about three quarters in the Borjas paper's moderate-demand scenario.[8]

Borjas (2026a) considers employers' long-term response to the tax, driven by fundamentals. The Borjas method proposes a novel simulation that yields a demand elasticity centering on −4. This would imply that the taxed demand for H-1B workers remains above the binding 85,000 cap so that realized high-skill immigration is unaffected.

The Borjas method's simulation uses individual-level data on H-1B workers and native workers. It begins by stacking the universe of 343,397 workers with approved H-1B petitions 2021–2024 (Bloomberg 2025) on top of a nationally representative sample of 581,648 full-time, year-round employed, college-educated US native workers from the American Community Survey (census) data in the same years. The method then estimates the conditional log wage gap between H-1B and native workers with the Mincer (1974) regression,

$$\ln w_i \;=\; \mathbf{X_i}\beta \;+\; \gamma \cdot \mathbb{1}\big(\text{H-1B}\big) \;+\; m_i \;+\; o_i \;+\; d_i \;+\; t_i \;+\; \varepsilon_i, \qquad \text{private sector only} \tag{1}$$

where X_i is a matrix of fixed effects for worker i's highest degree completed, age, and gender; the other regressors are fixed effects for metropolitan area of employment (m_i), occupation code (o_i), industry (d_i), and year (t_i), with the slope coefficients omitted for clarity; and ε_i is an error term. The coefficient estimate of interest is $\hat{\gamma}$, the conditional log-point wage gap between H-1B workers and natives. Because the method limits the sample to the private sector (dropping workers in the public sector and workers at universities or non-profits), this wage gap is measured within cells restricted by four outcomes of worker sorting: geographic location, occupation, industry, and sector.

not to raise general government revenue.

[7] The September 2025 $100,000 H-1B tax that produced this reaction was only charged to H-1B workers outside the country, that is, roughly half of cap-subject petitions. The August 2026 update of the tax to $103,265 is charged for all cap-subject petitions (DHS 2026).

[8] In the Borjas "moderate demand" scenario, employers demand 255,000 H-1B visas without a tax, subject to a binding quota of 85,000 visas. A 91.2 percent reduction in demand would imply demand of 22,440, that is, a 74 percent reduction in realized high-skill immigration via the H-1B.

The method then uses the coefficient estimates from wage regression (1) to predict what each H-1B worker would earn if their pre-market traits (education, age, gender) and sorting outcomes (city, occupation, industry, sector) were priced according to the *native* wage structure. The counterfactual "*if native*" wage for each H-1B worker h is

$$\ln \hat{w}_{Nh} \;=\; \mathbf{X_h}\hat{\beta}_{\mathrm{N}} + \hat{m}_N + \hat{o}_N + \hat{d}_N + \hat{t}. \tag{2}$$

The method then defines an employer's "payroll savings" (markdown) from hiring H-1B worker h as the difference between this predicted "*if native*" wage and the worker's actual wage,

$$\hat{\Delta}_h = \ln \hat{w}_{Nh} - \ln w_h. \tag{3}$$

This worker-level "savings" is the basis for all that follows. The method uses the "savings" (3) to simulate the demand for H-1B workers under different taxes. Suppose that firms, when taxed, hire a six-year H-1B worker (wage w_H) instead of an *equally productive* native worker (wage w_N) only when they thereby save at least enough money to cover the tax. That is, firms demand H-1B workers if and only if the "savings" (markdown) exceeds the tax:

$$\ln w_N - \ln w_H \geqslant \tau(T), \tag{4}$$

where τ is the tax T expressed as a percent of the six-year cost of employing the worker.

This reasonable condition faces two problems. First, for each H-1B worker, the wage for an *equally productive* native (w_N) is unobserved. The method addresses this with the assumption that natives and H-1B workers have identical marginal products, within city-occupation-industry-sector cells, provided that those natives earn more than H-1B workers. This equates to the strong assumption that natives and H-1B workers in those cells carry out identical tasks at identically productive firms. This assumes that when H-1B workers earn less than natives within cells, the gap (3) is entirely a wage markdown for H-1B workers relative to natives.

Second, the simulation of employers' response must grapple with the fact that the predicted "savings" (3) is negative for roughly a third of the H-1B workers studied: These workers' wage is *higher* than that of similar natives in the same city-occupation-industry-sector cell, but all of

them were hired in the years 2021–2024, when there was no tax. To reconcile these hires with the hiring condition (4), the method assumes that employers hiring these "negative savings" H-1B workers experience an unobserved productivity effect, modeled as a hidden cost $\pi_h \gtrless 0$. The method then sets that hidden cost for each H-1B worker so that it *at least* offsets their higher wage.

The hiring condition then traces out a demand curve for H-1B workers. Without a tax, all H-1B workers actually hired in the pre-tax years are hired in the simulation. As the tax rises above zero, the fraction of H-1B workers still hired at each tax level T falls to the fraction meeting the hiring condition adjusted for assumed hidden costs: $p(T) = \Pr\left[\hat{\Delta}_h \geq \tau(T) + \pi_h\right]$. From this curve, the paper concludes that at a tax $T = \$100,000$, high-skill immigration is unchanged. Even at "a fee near or even exceeding \$100,000 …all 85,000 capped visas would still be used" (Borjas 2026a), that is, under normal demand conditions, the tax will not affect realized immigration because demand for H-1B workers remains above the binding visa quota.

2.2 Replicating the method, and identifying minor and major errors

This analysis begins by reconstructing the dataset underlying Borjas (2026a) and replicating its results. Table 1 juxtaposes the estimates from the paper's core regression (1) with this replication. The first two columns of panel *A* show that, in this paper's regressions following the Borjas method, H-1B base salaries are 4.6 percent higher than average total wage income for natives, without controlling for anything but the year (and thus the price level), and 8.1 percent higher than total wage income of natives of the same highest degree, age, and gender.

The wage gap then shifts sharply negatively when one adds fixed effects for a succession of worker-sorting outcomes: city of employment (shift −10.5 percentage points, col. 3), occupation code (shift −11.5 percentage points, col. 4), and industry (shift −2 percentage points, col. 5). Panel *B* reports very similar results in the analogous quantile regressions (p50), implying that the results are not driven by extreme wage values.

Table 1 is a successful *verification* of the Borjas (2026a) results, following its method.[9] Starting

[9] Clemens (2017) defines a verification as the mechanical reproduction of the original paper's estimates using

Table 1: The conditional gap between H-1B starting base salary and native total wage income at average seniority: Replication of Borjas with corrections

	(1)	(2)	(3)	(4)	(5)
Fixed effects:	—	+demog.	+city	+occ.	+ind.
A. OLS					
Borjas (2026a, Table 2)	0.044	—	−0.023	−0.136	−0.161
Replication with identical method	0.0458	0.0809	−0.0241	−0.1389	−0.1592
	(0.0011)	(0.0013)	(0.0017)	(0.0023)	(0.0026)
Corrected (ZIP, occ., & ind. matching, cell-clustered SE)	0.0458	0.0809	−0.0346	−0.1467	−0.1594
	(0.0639)	(0.0704)	(0.0561)	(0.0396)	(0.0428)
B. Median regression					
Borjas (2026a, Table 2)	0.072	—	−0.013	−0.127	−0.152
Replication with identical method	0.0733	0.0910	−0.0142	−0.1308	−0.1512
	(0.0012)	(0.0013)	(0.0017)	(0.0023)	(0.0026)
Corrected (ZIP, occ., & ind. matching, cell-clustered SE)	0.0733	0.0910	−0.0243	−0.1382	−0.1516
	(0.0383)	(0.0340)	(0.0271)	(0.0375)	(0.0327)
N (*replication*)	925,006	925,006	925,005	924,998	924,998

"Demog." fixed effects are for highest degree completed, single year age, and gender. "City" fixed effects are for Core-Based Statistical Areas (CBSA). "Occ." and "ind." fixed effects are for occupation code and industry code respectively. In each panel, the first row reprints values in Borjas (2026a, Table 2) where available. The second presents a replication using substantively identical methods. The third presents the replication using standard errors corrected for correlation of the H-1B status indicator within city-occupation-industry cells (Abadie et al. 2023) via three-way clustering (Cameron et al. 2011), and correcting for the minor errors mentioned in the text.

base salaries in the H-1B dataset are 16 percent lower than average total wage income for natives who on average are mid-tenure, within the chosen city-occupation-industry-sector cells.

Simple corrections: The estimates using the Borjas method in Table 1 contain three errors that are easy to correct and do not substantially alter the point estimates. Estimates incorporating these simple corrections are shown in the "corrected" rows of Table 1. Two of these correct errors in how workers' jobsites were matched to city codes, and how occupation and industry codes from the administrative data (H-1Bs) were matched to codes in the census survey data (natives). These are detailed in the Appendix.

A third error that is simple to correct, but is consequential for statistical inference, is that the Borjas method's conventional robust, unclustered standard errors fail to account for the fact that H-1B status of workers is highly correlated within city-occupation-industry-sector cells. The standard errors in Borjas (2026a) are thus biased downwards. H-1B visa holders sort non-randomly into a narrow set of cities, occupations, and industries. Conditional on belonging to a

essentially identical methods on essentially identical data.

software-engineering cell in Silicon Valley, knowing that one worker holds an H-1B is strongly predictive that others in the same cell do as well. Conversely, knowing that any worker in a cell for a lawyer at a large construction firm in Oklahoma City does *not* hold an H-1B is strongly predictive that other workers in the same cell do not either. The corrected rows of Table 1 show standard errors with multi-way clustering by cell as modern applied econometrics requires in this case (Abadie et al. 2023).[10] The corrected 95 percent confidence interval on the 15.9 percent wage gap (panel *A*, column 5) is ±8.4 percentage points.

Major errors: The Borjas method nevertheless rests on a second class of errors that defy simple correction given its data and research design. The major errors relate to bias in how the method measures the wage gap in Table 1, how the method interprets that gap, and how the method uses that gap to predict employer response to an H-1B tax. Each of these three classes of error is sufficient, by itself, to call into question the ability of the method to predict real employers' response to the tax, and thus the effect of the tax on high-skill immigration. The rest of this paper details and demonstrates each of these major errors, one at a time, in the same data and framework as the original paper. The three classes of error are *Measurement*, *Comparison*, and *Simulation*.

3 Measurement: Biased estimates of the H-1B wage gap

The Borjas method requires predicting an unbiased counterfactual "*if native*" wage for each H-1B worker (2), using the measurements in Table 1. This cannot be done, because the wage concept, wage structure, and sample of H-1B workers in those regressions are different from those of natives, in ways that are unobserved. The resulting measurement produces first-order bias that inflates the predicted gap between actual and counterfactual H-1B wages.[11]

[10] Although immigration status varies across individuals—unlike a state-level policy variable that is constant within a jurisdiction—one cannot conclude that the "Moulton (1986) problem" is absent. Abadie et al. (2023) show that when "treatment" is highly correlated within cells, as it is in the present case, multi-way clustering is essential because the conventional unclustered standard errors used in the paper can severely overreject the null (Moulton 1990; Cameron and Miller 2015).

[11] The first study to note important methodological errors in the first version of the Borjas method was by He and Ozimek (2026), prompting changes in the version of the Borjas (2026a) analysis considered here.

3.1 The measurement counts native income that is omitted for H-1B workers

The Borjas method yields bias-inflated estimates of employers' "savings" from hiring an H-1B worker (3) because the counterfactual "if native" wage (2) is predicted using native wage data that include forms of income omitted from the H-1B wage data. The predicted "if native" wage includes bonuses and other supplementary pay beyond the base salary; the H-1B wage does not. This creates a biased, artifactual "wage" gap of about seven percentage points, even between native and H-1B workers whose total wage compensation is identical. US government auditors specifically warn against comparing wages from H-1B administrative data to wages from census surveys for this exact reason (GAO 2011, 82).

The administrative data source for H-1B wages is required by law to report only the base salary. The numbers reported there *must* omit any bonuses, payment-in-equity, or other nonguaranteed compensation paid to H-1B workers.[12] These forms of compensation are very common in the private-sector tech and finance firms where H-1B workers concentrate (BLS 2020, 2024, details in the Appendix). The measured wage income of US natives, in contrast, comes from the American Community Survey (2021–2024), which asks respondents to report "*wages, salary, commissions, bonuses, or tips from all jobs*" (Ruggles et al. 2025). Audit studies have confirmed that natives do typically report non-base wage compensation to the census as directed: Bonuses and payment-in-equity (including Restricted Stock Units) are included on the wage-income (W-2) tax forms that native workers receive annually, and there is no evidence that non-base wage income is systematically omitted from census responses.[13]

The Borjas paper's own tables reveal that its wage gap estimates are inflated by about seven percentage points via this mechanism. The text does not draw out this implication. The method

[12] The law governing the wage that employers report on the 'I-129' H-1B visa petition form and the Labor Condition Application specifies that "if the payment is contingent or conditional on some event such as the employer's annual profits, the employer must guarantee payment even if the contingency is not met" (20 CFR §655.731(c)(2)(v)). It also requires H-1B employees who work less than one year to be guaranteed a *pro rata* share of the annual compensation reported on the I-129 form. In practice this excludes annual bonuses and payments-in-equity such as Restricted Stock Units. Bonuses and stock payments that are guaranteed, regardless of firm performance or employee performance, are very rare among H-1B employers and in the tech industry generally. Moreover, employers that do use an annual bonus as part of compensation cannot move such bonuses to base salary just for H-1B employees, even if the bonus is largely expected to hit a minimum—as all similarly employed workers must be treated equally.

[13] Bee and Mitchell (2017, Table 1) find that average W-2 labor earnings for working-age adults differs by just 0.3 percent from the labor earnings reported by the same people on census survey forms. Important discrepancies exist for retirees and for non-wage income, especially transfers and pensions, that are irrelevant to the analysis here.

Table 2: The H-1B wage gap under a single wage concept, with proxy for unobserved H-1B status

	Top-10 H-1B occupation and industry		*Top-5 H-1B occupation and industry*		
	Borjas	*Replication, proxy H-1B*	*Borjas*	*Replication, proxy H-1B*	*Proxy non-H-1B (placebo)*
Wage gap	-0.094	-0.096	-0.126	-0.128	-0.121
	(0.009)	(0.031)	(0.010)	(0.024)	(0.014)
N	586,374	585,984	585,133	585,061	596,813
Implied pop.	—	511,868	—	398,250	1,830,835

Col. 1 is the printed estimate in Borjas (2026a, Table 7, col. 3), col. 3 is the printed estimate in Borjas (2026a, Table 7, col. 1). Estimates are of the paper's specification (1) within the ACS, 2021–2024 with fixed effects for year, education, age, gender, occupation, industry, and metropolitan area, for private-sector workers only. The "likely-H-1B" proxy is: noncitizen, arrived after age 18, within the six-year visa window on the paper's own working-years clock (Borjas footnote 27). *Top-10* (*Top-5*) restricts the sample of proxied H-1B workers and natives to the ten (five) detailed occupation codes *and* the ten (five) detailed industry codes where actual H-1B workers concentrate in the administrative data (Bloomberg 2025). Col. 2 replicates col. 1, col. 4 replicates col. 3. The placebo col. 5 keeps all else equal but replaces the "H-1B" workers identified by the proxy with "not H-1B" workers identified by the same proxy, for whom a visa-created markdown predicts approximately zero. *Implied population* is the survey-weighted total. Standard errors clustered three ways by occupation, metropolitan area, and industry (Cameron et al. 2011).

seeks to address the problem of incommensurable wages by repeating the wage-gap analysis entirely within the ACS, under a single wage concept, without using the Bloomberg (2025) administrative dataset on H-1B workers. This requires a method to identify likely H-1B workers in the ACS. The paper first identifies likely H-1B workers with reasonable criteria: foreign-born noncitizen college graduates, who arrived after age 18 but within the past six years, employed in the top-10 occupations and in the top-10 industries where H-1B workers are known to work.

Those results are transcribed and replicated in the first two columns of Table 2. The wage gap between natives and "likely H-1B" workers under a single wage concept in the Borjas results is −9.4 percent, compared to the gap under incompatible wage concepts of −16.1 percent (Table 1, col. 5). That is, incompatible wage concepts inflate the core estimates by 6.7 percentage points, which matches the magnitude of standard supplemental pay in the relevant occupations and industries.

The Borjas paper then shifts to another specification, restricting its proxy-identifier for H-1B workers in the ACS to the top *five* H-1B occupations and industries, instead of the top ten, and estimates a within-ACS wage gap of −12.6 percent. This result and its replication are shown in Table 2 columns 3–4. That analysis posits, but does not test, that the more restrictive top-five criterion accurately identifies H-1B workers in the census. Table 2 does test that assertion, with

a placebo comparison. If the "top five occupation/industry" restriction successfully identifies H-1B workers, then workers in the ACS who pass the other criteria (such as US arrival in the past six years) but are excluded by the top-five restriction must thereby be classified as *non*-H-1Bs. The last column of the table compares these ostensibly *non*-H-1B foreign workers to natives. The wage gap with natives is substantially unchanged, at −12.1 percent.

The placebo test is inconsistent with the Borjas paper's conclusion. If the strict "top 5" restriction correctly identifies H-1Bs, then the exercise has shown that H-1B status has no detectable relationship with foreign workers' wages relative to natives', because the gap is very similar for non-H-1Bs. This is the pattern to be expected if the H-1B wage gap arises from differential task assignment between immigrants and natives in general, and the generalized discount to foreign human capital in the US labor market, but is inconsistent with a visa-produced markdown. Alternatively, if the "top 5" restriction does *not* correctly identify H-1Bs, then the exercise is not useful for its purpose: to estimate the H-1B wage gap under a single wage concept. Neither of these is consistent with the paper's reading.

A gap of seven percent between bonus-inclusive and bonus-exclusive income is typical of the jobs and industries where H-1B workers are employed, as independently verified by a custom extract from the Bureau of Labor Statistics nationally-representative National Compensation Survey presented in the Appendix.[14]

Moreover, the finding of a seven-percentage-point bias can be independently verified in another dataset that the Borjas paper uses. Borjas (2026a, Table 4) reports that when natives' wage income is estimated with a smaller sample from the Current Population Survey (CPS, annual ASEC supplement), the conditional mean H-1B wage gap is −15.6 percent, very close to the −16.1 percent gap when comparing to natives in the annual American Community Survey (Table 1, Panel *A*). The wage income question in both surveys is the same: Both direct respondents to report annual income including "bonuses".

[14]The Appendix presents evidence corroborating all of these findings with hand-collected data on the compensation packages of junior software engineers at 16 of the largest H-1B employers. At the median firm, bonuses amount to 6.2 percent of the value of base salary, and payment-in-equity is an additional 4.5 percent not captured in the NCS/ECEC data (Employer Costs for Employee Compensation), though there is less evidence about what share of payment-in-equity is reported in census surveys.

Table 3: Correcting asymmetric wages: The H-1B wage gap with and without natives' supplemental pay

	Borjas, CPS ASEC incl. bonus	Replication: CPS ASEC incl. bonus	Excluding bonus (ORG)	Excl. bonus (ORG, same workers)
OLS gap	−0.1560	−0.1524	−0.0322	−0.0650
	(0.0100)	(0.0109)	(0.0068)	(0.0175)
Median regression gap	—	−0.1429	−0.0301	−0.0698
		(0.0107)	(0.0068)	(0.0173)
N, natives	34,563	34,563	64,660	15,727
N, H-1B workers		343,171	343,171	343,171
N, total		377,734	407,831	358,898

The median regression is more reliable because the ORG topcodes weekly earnings at a ceiling that sometimes binds in tech. occupations. Col. 1 transcribes Borjas (2026a, Table 4, row 2). All results in the table retain the Borjas method of robust standard errors, without correction for clustering. Col. 2–4 show the main specification (1) with full fixed effects, changing only how pay is measured for natives. Col. 2 replicates the printed result in col. 1, measuring native pay with the CPS March ASEC annual wage-income question (bonus-inclusive, identical to the concept in the ACS data used in the Borjas method), for ASEC respondents in earnings years 2021–2024 (survey years 2022–2025). Col. 3 uses the CPS ORG sample, with the outgoing-rotation "usual weekly earnings" response annualized (× 52 and bonus-exclusive, the concept closest to a guaranteed salary comparable to the H-1B administrative-data wage), using every outgoing-rotation respondent interviewed during the same earnings years, 2021–2024. Col. 4 restricts the ORG sample to ASEC respondents linked to their own outgoing-rotation interview.

But the CPS is a panel survey that asks *the same respondents* a different income question when they rotate out of the panel (CPS-ORG): It asks for their "usual weekly earnings". This logically excludes occasional bonuses and the survey does not direct respondents to include bonuses. Multiplying this response by 52 creates a bonus-exclusive annual wage income measure to compare to the bonus-inclusive annual wage income of the same individuals.

Table 3 presents this exercise. The first column transcribes the conditional mean gap between H-1B base salary and native wage income *including bonuses* measured by Borjas (2026a, Table 4), −15.6 percent. The second column (first row) closely replicates that finding, with a −15.2 percent gap, in the CPS-ASEC data. The third column switches to a bonus-exclusive measure of income (respondents to the CPS-ORG in the same years).[15] The gap between H-1B workers and natives, using the same bonus-exclusive wage concept, falls by 12 percentage points. Because there could in principle be compositional differences between CPS-ASEC and CPS-ORG respondents, the last

[15]The bonus-exclusive ORG wage income sample consists of every CPS outgoing-rotation respondent to the "usual weekly earnings" question during calendar 2021–2024 (the same years in which earnings is reported by ASEC respondents) passing the same criteria and with the same $34,780 real earnings floor, applied to annualized weekly earnings. Every CPS household goes through an outgoing rotation, so this is a nationally representative sample of workers responding to the bonus-exclusive wage income question over the same period, free of the CPS panel attrition that limits the sample that can be linked to their ASEC responses in the next column.

column restricts the CPS-ORG sample to bonus-exclusive income responses by *the same people* who responded to the bonus-inclusive ASEC income question in column 2.[16] The portion of the H-1B wage gap attributable to excluding bonuses contracts somewhat, to 8.7 percentage points (the difference between −0.152 in col. 2 and −0.065 in col. 4).

This pattern could not arise from topcoding of incomes in the CPS-ORG.[17] In the median regressions of the second row, which are robust to topcoding, the H-1B gap attributable to omitting bonuses is 7.3 percentage points (median gap, col. 4 minus col. 2). This independently corroborates the magnitude of the bias shown in the Borjas tables and BLS employer surveys.

3.2 The measurement assigns H-1B workers the native return to human capital

The Borjas method furthermore yields biased estimates of employers' "savings" from hiring an H-1B worker (3) by mispricing immigrants' human capital. The method to project H-1B workers' counterfactual "if native" wage (2) assumes that H-1B workers' human capital has returns identical to natives' human capital in the US labor market. Specifically, by using the native coefficient estimates on age and education (β_N) to predict H-1B workers' counterfactual wages, it assumes that foreign H-1B workers paid without a markdown would receive the same returns to each year of work experience as a native does, and receive the same returns to their higher-education degrees as natives do. This inflates the wage gaps in Table 1 with a first-order bias.

It is well established that the returns to foreign experience and foreign education are systematically lower, in the US and peer countries, than natives' returns to experience and education (Albert et al. 2026). Foreign education is discounted in high-income migrant-destination-country labor markets (Friedberg 2000; Bratsberg and Ragan 2002; Ferrer and Riddell 2008; Schoellman 2012; Mattoo et al. 2008), with the discount for college-educated immigrants around 10 to 20 percent (Arbeit and Warren 2013; Kaushal 2011; Zeng and Xie 2004). Foreign work experience is

[16] The native sample size is lower in col. 4 than col. 2 due to standard levels of attrition in the CPS panel.

[17] Weekly earnings in the CPS-ORG were topcoded at $2,884.61 weekly (annualized ~$150,000) through April 2023, which binds for 13–15 percent of the native sample from 2021 to 2022. Thereafter the Census Bureau instead replaces values above a dynamic threshold with their mean, yielding an effective ceiling near $500,000 that binds for under one percent of the sample. In the CPS-ASEC, annual wage income has no fixed cap. The Census Bureau protects very high values, and reported values in this sample are as high as $1.4 million.

discounted more severely, with a year of foreign experience typically priced at less than a third of destination-country experience (Friedberg 2000; Schaafsma and Sweetman 2001; Ferrer and Riddell 2008; Aydemir and Skuterud 2005), including for college graduates (Ferrer et al. 2006; Kaushal 2011; Oreopoulos 2009, 2011). The literature finds that this pattern primarily reflects real productivity differences, either arising from lower human capital, nontransferable human capital, or mismatch with tasks or employers at the destination.[18]

Table 4 shows the sharp discount to overseas experience that is misinterpreted by the core regressions of the Borjas analysis (Table 1). The first column simply reproduces the core estimate of the H-1B wage gap in Borjas (2026a) and this paper's replication of it. The second column replaces the H-1B indicator with a set of interactions between the H-1B indicator and all four education levels. Each coefficient estimates the conditional wage gap between H-1B workers and natives who hold the same degree. The third column splits the "H-1B × bachelor's" coefficient into age bands, by adding interaction terms for H-1B (bachelor's base group) × age-band indicator.

That age gradient in column 3 reveals that the H-1B wage gap widens sharply with age. For the youngest workers (age 21–25) there is no negative wage gap at all: the gap is positive and significant at +0.047. The gap becomes progressively more negative with workers' age. Why? This evidence is difficult to reconcile with the wage gap arising from markdowns due to employer market power: It is unclear why, if such markdowns are substantial, employers would have no such power over workers in their 20s. And any explanation that the age gradient arises from shifting education composition is inconsistent with the continuing inclusion of the education indicators and interactions from column 2 in column 3.

There is an obvious explanation for the absence of an H-1B wage gap for young workers, and the increasingly negative gap for older workers: that the overall H-1B wage gap in column 1 arises substantially from the fact that experienced immigrants in the US labor market receive

[18] Immigrants earn similar returns to measured literacy capital as natives do, suggesting real skill differences rather than differential pay for equal skill (Ferrer et al. 2006). Mismatch operates similarly: a physician driving a taxi produces less than a well-matched physician, whatever the taxi company's power over wages (Mattoo et al. 2008). The clearest exception is employer screening of foreign names and credentials (Oreopoulos 2009, 2011), but even that concerns employers' beliefs about what a worker will produce, not their power over the wages paid to a worker of known marginal product.

Table 4: Age profile of the conditional H-1B wage gap, and channel composition

	(1) Pooled	*(2) Education interactions*	*(3) Age-band split*	*(4) Channel composition*
Borjas (2026a, Table 2A, col. 5)	−0.161			
	(0.003)			
H-1B indicator	−0.159			
	(0.043)			
H-1B × bachelor's degree		−0.141		
		(0.046)		
H-1B × master's degree		−0.194	−0.087	−0.091
		(0.040)	(0.010)	(0.010)
H-1B × professional degree		0.031	0.163	0.163
		(0.067)	(0.061)	(0.059)
H-1B × doctorate		−0.099	0.063	0.068
		(0.042)	(0.032)	(0.032)
H-1B, age 21–25			0.047	0.135
			(0.021)	(0.024)
H-1B, age 26–30			−0.054	0.046
			(0.027)	(0.020)
H-1B, age 31–35			−0.151	0.080
			(0.046)	(0.024)
H-1B, age 36–40			−0.271	0.054
			(0.042)	(0.018)
H-1B, age 41–50			−0.301	0.053
			(0.053)	(0.022)
H-1B × consular share at that age				−0.447
				(0.058)
Joint F (equal gaps across degrees) [p]		12.7 [0.0000]	32.2 [0.0000]	
N		924,973		

All columns include fixed effects for education, age, and gender, along with city, occupation, industry, and year. The first row of col. 1 shows the printed result in Borjas (2026a, Table 2A, col. 5), and the second row shows a replication using the main specification (1). That specification imposes identical returns to education and experience for the immigrants and natives in the sample. Col. 2 relaxes the assumption of identical returns to education. A joint F test in the last row strongly rejects the hypothesis of equal gaps across all degrees collectively. Col. 3 also relaxes the assumption of identical returns to experience, and a similar joint F test at the bottom strongly rejects identical returns. (The bachelor's row is omitted because its coefficient is a linear combination of the coefficients shown.) Col. 4 adds a control for the consular-channel share of H-1B workers at each H-1B's age, interacted with H-1B status. The consular share is a generated regressor: A 50-replication occupation-cluster bootstrap puts its standard error at 0.080 rather than the analytic 0.058. Standard errors clustered three ways by occupation, metropolitan area, and industry.

lower returns to their experience than natives. This idea, that age is proxying for foreign experience, is tested in column 4 of Table 4. That column adds a constructed variable as a control: for each worker, the fraction of H-1Bs of the same age as that worker who arrived through consular processing (arriving directly from overseas, and thus much more likely to have worked abroad) rather than change-of-status (people already in the US on a different visa, almost always having studied or worked in the US for years).[19] As would be expected if age correlates with increasing foreign fraction of work experience, the negative age gradient from column 3 disappears in

[19]The standard error on this generated regressor is calculated by a cluster-bootstrap with 50 draws, to capture the effect of uncertainty in the true consular fraction at each age that would otherwise be unaccounted for.

column 4.[20]

An independent measure of the bias from mispricing foreign human capital can be obtained using well-established methods to price this capital in census data. Table 5 shows this exercise, carried out within the ACS data, to adjust the Borjas method's estimated wage gap by correctly pricing immigrant human capital at its observed, generalized discount relative to natives.

The first column of Table 5 uses the standard method of Friedberg (2000) to estimate the relative price of immigrants' human capital in the ACS data for all college graduates. Immigrants' work experience before year-of-arrival is assumed foreign experience, and otherwise US experience. Schooling is imputed to be foreign if, assuming continuous schooling beginning at age seven, it would have been completed before the immigrant's age at arrival.[21] In panel A of the table, immigrants' wage return to a foreign year of experience is 1.5 percentage points less than the return to a US year, with metro-area, year, and gender fixed effects. The next two rows estimate the gap in the overall returns to foreign degrees and US degrees, for undergraduate degrees and graduate degrees respectively.[22]

Moving down column 1 of Table 5, panel B estimates the overstatement of "likely H-1B" workers' counterfactual "if native" wages that would result from assuming that they receive US returns to foreign degrees ("education component") and foreign experience ("experience component"). Overall, in the all-college-graduates ACS sample, "likely H-1B" workers' "if native" wages would be overstated by +0.124 log points from the combined mispricing of their foreign education and foreign experience. These overstatements are estimated using the actual observed distribution of age and education in the H-1B administrative data.[23] Column 2 of Table 5 mirrors column 1 but with the addition of occupation and industry fixed effects, illustrating that some portion

[20] In principle, a similar pattern could arise from patterns in firm type by age and channel, such as if older workers are more likely to arrive by the consular channel *and* consular-channel workers are relatively more likely to work at lower-wage outsourcing firms (see Sparber 2026). This hypothesis is tested and rejected in the Appendix.

[21] The results are substantially invariant to the alternative imputation of Fortin et al. (2016), as shown in the Appendix, which imputes that a degree is foreign by the criterion age-at-arrival minus 6 $\geqslant$ years of education (along with a direct census question on degree location).

[22] The graduate degrees line shows a weighted average of the separate foreign-US return gaps for master's, professional, and PhD degrees, weighted by the relative prevalence in the H-1B population.

[23] To split actual H-1B workers' experience and education by US versus foreign, workers are split by arrival channel. Arrivals by consular processing are assumed to have foreign degrees and their potential work experience is assumed foreign. Those already in the US and changing status are assumed to have US degrees.

Table 5: The H-1B wage gap under a single wage concept, without imputing native human capital prices to immigrants, using proxy for unobserved H-1B status

	All college graduates	All college graduates	Top-10 cells	Top-5 cells
Borjas Table 7, printed	—	—	−0.094	−0.126
			(0.009)	(0.010)
This replication, same "likely H-1B" criteria	—	—	−0.096	−0.128
			(0.031)	(0.024)
A. Estimation of foreign experience and degree discounts:				
Return to a US experience year	0.053	0.047	0.048	0.045
	(0.003)	(0.003)	(0.005)	(0.002)
Foreign-year return, gap vs. US year	−0.015	−0.009	−0.012	−0.010
	(0.003)	(0.002)	(0.003)	(0.002)
Foreign-degree gap, bachelor's level (naturalized)	−0.124	−0.055	−0.046	−0.046
	(0.029)	(0.026)	(0.093)	(0.042)
Additional foreign-degree gap, advanced (H-1B mix)	0.018	−0.033	−0.076	−0.064
	(0.040)	(0.019)	(0.021)	(0.033)
Occupation and industry fixed effects	No	Yes	Yes	Yes
B. Implied overstatement at the H-1B workers' actual educ., age, & channel distribution:				
Education component	0.050	0.027	0.027	0.026
Experience component	0.074	0.044	0.062	0.050
Total	0.124	0.071	0.090	0.076
	(0.019)	(0.014)	(0.042)	(0.021)
C. Replicated wage gaps (second row above), net of overstatement:				
Adjusted likely-H-1B gap	—	—	−0.007	−0.051
			(0.052)	(0.031)
N (decomposition regressions)	606,793	606,790	36,049	22,489
N (naturalized-immigrant regressions)	654,657	654,654	38,403	23,667

Friedberg (2000) decomposition estimated on the "likely-H-1B" workers in the ACS with college only, detailed in the text. *Top 10* and *Top 5* cells as in Table 2. Panel B averages, over the H-1B workers' known distribution of age and education from the administrative data, each worker's mispricing. Panel C adds each column's overstatement from Panel B to the replicated wage gap in the second row, an estimate of the gap net of the returns differences evaluated at the actual H-1B workforce's education, age, and channel mix. Standard errors of the decomposition clustered three ways by occupation, metropolitan area, and industry. The bachelor's-level return gap between a foreign and a US degree is unidentified in the "likely-H-1B" sample (collinear with the group intercept) because the sample is college-only, so it is estimated via the same decomposition run on natives pooled with naturalized-citizen college graduates (by definition non-H-1B). The advanced-degree row averages the estimated marginal-degree return gaps weighted by the consular degree mix (0.225 master's, 0.007 professional, 0.018 doctorate).

of the discount to foreign human capital is realized through sorting between occupations and industries.

Column 3 of Table 5 restricts the sample to the paper's top-ten occupations and industries for H-1B workers, and column 4 restricts to the top five. Foreign human capital remains heavily discounted in those relevant occupations and industries. In panel C, column 3 shows that assuming US returns to foreign human capital, in the Borjas within-ACS specification of Table 2 (replicated at the top of Table 5), H-1B workers' counterfactual "if native" wage is overstated by +0.090 log points by mispricing their foreign human capital.

Allowing H-1B workers' foreign human capital to have the foreign returns typical of foreign workers in these jobs and industries therefore corrects the replicated Borjas wage gap of −0.096 (at the top of the table) to −0.007 (at the bottom) in the top-ten occupations/industries sample of column 3, with a 95 percent confidence interval of ±0.102. In the top-five sample of column 4, the same corrected H-1B wage gap is −0.051 and indistinguishable from zero. This is consistent with the independent evidence from Table 4: accounting for the well-established discount to foreign human capital, especially experience, the evidence cannot reject an H-1B wage gap of zero, nor can it reject a small negative or small positive wage gap.

Note also that the results in Table 5 simultaneously address the problem of incommensurable wage concepts discussed in subsection 3.1, because this within-ACS analysis uses the same wage concept for "likely H-1B" workers and native workers. Panel C of Table 5 shows that when the errors of using incommensurable wage concepts and assuming native returns to foreign human capital are corrected, the H-1B wage gap within metro-occupation-industry cells cannot be distinguished from zero, even in the narrow handful of cells where H-1B workers are most concentrated. This is consistent with the independent evidence of Table 4, that the H-1B wage gap is zero or positive for younger workers, and only exists via discounting the foreign work experience of older workers.

3.3 Seniority differences between the two samples

The Borjas wage gap estimates of Table 1 suffer from a separate bias because they compare the starting salaries of H-1B workers (with zero employer tenure) to the average wages of native workers (with 6.4 years of tenure on average, Clemens 2026a, Table 2). Even if the wage concepts included the same income streams, this timing mismatch would create an artifactual wage gap between H-1B and native workers because workers' wages tend to rise substantially with employer tenure (Altonji and Shakotko 1987; Topel 1991; Altonji and Williams 2005).[24]

[24]Economists have made recent advances in understanding why the wages of high-tenure workers are typically higher than those of low-tenure workers (e.g. Adda and Dustmann 2023), comparing explanations based on firm-specific human capital to explanations including an arrival of outside options that is spaced out across time. The mechanism for the seniority premium is irrelevant to this analysis: Comparing employers' costs of employing two different workers requires comparing the wages those workers are actually paid, regardless of the mechanism for the observed fact of the seniority premium.

The Borjas paper accepts this bias in its text. The current version incorporates a seniority adjustment (Clemens 2026a). The method estimates that this bias inflates the paper's own headline 16.1 percent H-1B wage gap by 3.6 percentage points. This, Borjas (2026a, 17) writes, "would imply a 'true' wage gap of about 12.5 percent". That corrected value is the wage gap that the paper accepts for the purpose of its policy simulation. The paper's abstract reports the uncorrected 16 percent figure.

Separately, the Borjas paper's estimate of the resulting bias is incomplete. It understates the bias by 1.1 percentage points. The problem arises from the fact that the Borjas method's sample intended to represent zero-tenure workers (like H-1Bs) in fact excludes workers with under one year of tenure. Because wages rise on average even over the first year of employer tenure, this understates the bias from ignoring tenure differences between new-hire H-1B workers and average-tenure native workers. The corrected estimate of seniority bias in the paper's estimates, 4.5 percentage points rather than the estimate of 3.4 (this paper's replication of the Borjas estimate of 3.6), is calculated in the Appendix.

3.4 Missing education values are not missing at random

The H-1B worker dataset provided to Bloomberg (2025) has large numbers of missing values for the crucial education variable (highest degree completed). In half the sample, 2023–2024, 35 percent of workers have unobserved education. The Borjas (2026a) method proceeds by imputation of the missing data, which is likely to produce bias. Its core findings come from assuming that all workers with missing education have bachelor's degrees, producing a major shift in the fraction of H-1B workers with bachelor's degrees in years 2023–2024 from 39.2 (observed) to 74.2 percent. The method argues that this large imputation is innocuous because various corrections that assume education is missing-at-random do not substantially alter the results.[25]

But they are not missing at random. The fraction of H-1B workers with observed bachelor's degrees is relatively stable at 31–39 percent of all records between 2021–2022 (no missing values) and 2023–2024, as it had been in previous years. But the fraction with observed master's degrees

[25]These are: dropping the workers with missing education, imputing the missing education levels randomly using the distribution of observed values, or assigning a separate education code for "missing". The validity of all requires the assumption that the process censoring education is independent of income conditional on education.

falls from 63 to 20 percent of all records while 35 percent switch to missing (shown in the Appendix). This makes it highly likely that master's degrees constitute most if not all of the missing education values. This has two implications. First, the Borjas method's core finding (Table 1) arises from imputing an education level to a large fraction of the sample that is almost certainly incorrect. Second, that incorrect imputation artificially inflates the H-1B wage gap. This is because the wages of immigrant workers with master's degrees, in the relevant jobs, are discounted more heavily with respect to natives than the wages of immigrant workers with bachelor's degrees (Table 5 above; see also Yang 2026). Many master's degrees from India, for example, have lower returns for Indian immigrants in the US labor market relative to Indian bachelor's degrees than natives receive for their master's degrees relative to their bachelor's degrees.

None of the missing-at-random imputation techniques in the Borjas method can address this bias. The only way to ensure that the bias is removed is to estimate the wage gap on the sample where education is fully observed: 2021–2022. In that sample, the conditional wage gap using the otherwise-identical Borjas method falls from −16.1 to −14.5 percent (shown in the Appendix). This is suggestive but not conclusive evidence that mass imputation of missing data in the Borjas method artifactually inflates its overall estimate of the wage gap by 1.6 percentage points. It is also hypothetically possible that the true H-1B gap happened to rise by 3.2 percentage points between 2021–22 and 2023–2024, which would yield the same pattern.[26]

In sum, errors of measurement in the Borjas study imply that, considering solely the corrections for incompatible wage concepts and mispricing foreign human capital, the H-1B wage gap is statistically indistinguishable from zero (Table 4 and Table 5). This conservative estimate omits any correction for the 4.5 percentage point seniority bias (which could double-count because bonuses rise with tenure) and omits any correction for the possible 1.6 percentage-point bias introduced by imputing missing education (which cannot strictly rule out alternative explanations).

[26] An average across a gap of 14.5 in 2021–2022 and a gap of $(14.5 + 3.2) = 17.7$ in 2023–2024 would yield 16.1.

4 Comparison: Artifactual productivity gaps within cells

The preceding section presented a range of evidence that the method of Borjas (2026a) mismeasures the H-1B wage gap within cells delimited by city, occupation, industry, and sector. This section highlights a separate and more profound flaw. This flaw arises in the *interpretation* of any given within-cell wage gap, whether correctly or incorrectly measured. The concern is more serious than simply that cells defined by worker-sorting outcomes can fail to control for productivity differences. The deeper flaw is that comparing workers within cells defined by worker-sorting outcomes can *create* productivity differences between the workers being compared, and thus an observed wage gap, even with a wage markdown of zero.

The Borjas method of predicting firms' response to an H-1B tax requires interpreting any negative H-1B wage gap within those cells as a pure wage markdown for workers of equal productivity, the "payroll savings" condition (3). Without that assumption, wage gaps by themselves contain no information about employer rents and thus no information about employers' response to the tax. But the method offers no justification for that assumption. The assumption is inconsistent with a wide range of evidence in modern labor economics, which finds that large differences in task assignment, and therefore marginal product, occur between workers within occupation codes (Autor and Handel 2013; Atalay et al. 2024), including between immigrants and natives within occupation codes (Tverdostup and Walter 2026). The assumption is furthermore inconsistent with the evidence below, which is instead consistent with within-cell wage differences dominated by productivity differences.

4.1 How fixed effects for sorting outcomes can manufacture productivity gaps

The method of Borjas (2026a) uses two different classes of fixed effects to generate the cells within which it compares H-1B and native workers. First it controls for three potential confounders, worker traits that plausibly correlate with productivity, that are standard in the research literature: indicator variables for highest-degree, single-year age, and gender. Controlling for these pre-market traits shifts the positive unconditional wage gap even further in the positive direction: H-1B workers earn 8.1 percent *more* than natives of the same education, age, and gender

(Table 1).

Second, the method uses fixed effects and a sample restriction to compare workers within cells defined by endogenous sorting outcomes: city of work, occupation, industry, and sector. Two of these (industry and sector) were added to the Borjas (2026a) analysis after its initial version (Borjas 2026b). But further saturation with fixed effects need not yield more accurate measures of a within-cell markdown. The self-sorting of native workers into and out of particular cities, occupations, industries, and sectors can respond to their productivity, particularly in technical occupations (Hanson and Liu 2018). For a closely analogous reason, the labor economics literature on male-female wage gaps cautions against interpreting the gaps as wage markdowns for women when occupation and industry fixed effects are included (e.g. Blau and Kahn 2017, Card et al. 2016): some or all of the wage gaps within some occupations and industries could arise from sorting endogenous to productivity (e.g. Goldin 2014). This raises concerns about interpreting the pattern of large negative shifts in the coefficient estimates of Table 1 as more and more worker-sorting outcomes are controlled for.

Because natives choose the place, job, industry, and sector where they work, this research design can do much worse than fail to control for productivity gaps that confound the estimation of markdowns. Fixed effects for endogenous sorting outcomes can create productivity gaps between workers within cells, without any markdown at all and without a generalized immigrant-native productivity gap. The problem is well known under two names, *bad controls* (Angrist and Pischke 2009, 64; Rosenbaum 1984; Cinelli et al. 2024) and *collider bias* (Elwert and Winship 2014; Montgomery et al. 2018).

To see how, consider a 32-year-old H-1B software developer with a master's degree from India, working at an outsourcing firm in Houston, at a starting base salary of $90,000. The Borjas regression (1) compares her to the average 32-year-old native software developer in computer systems design in Houston with a US master's degree, earning $120,000 of total wage income, and assigns her employer a "payroll savings" (markdown) of $\ln(120/90) \approx 0.29$. Now suppose that there is no markdown. The H-1B worker specializes in low-productivity software tasks like overnight-support or legacy codebase maintenance.[27] For every such H-1B worker, there

[27] Doran et al. (2022, 2507) study the task bundles (sub-occupations) in which H-1B workers specialize, within occupation codes. They conclude that for H-1Bs within the systems analysis and programming occupation, "The

Table 6: A schematic economy: within-cell wage gap with zero markdown

	Natives		Immigrants	
	Workers	*Wage = MPL*	*Workers*	*Wage = MPL*
Occupation A (lead vs. support bundles)	50	120	80	110
Occupation B (lead vs. support bundles)	50	80	20	60
Group average wage (no markdown)	100		100	
True immigrant-native % wage gap, no fixed effect	0			
Estimated immigrant-native % gap, occ. fixed effect	−13			

is a native with the same ability who was offered that task bundle at her $90,000 wage and declined it, a native who now appears in the data in the occupation, industry, sector, and city that she chose instead, earning approximately $90,000 *plus* the difference that made her decline the offer, and move cells to take up a more productive task bundle (e.g. with a marginal product of $105,000). The native's move responded to her unobserved determinants of wages, such as ability, ambition, network quality, and school prestige. The H-1B worker could not similarly move cells *to take up more productive task bundles* due to search frictions or limited portability.

A comparison of H-1B and native wages without the cell fixed effect *includes* the native who moved cells, so that the H-1B-native wage gap is a small premium reflecting the higher productivity of the tasks she switched cells to perform. But the cell fixed effect *deletes* from the comparison the native who switched cells. It only retains in the cell the native who beat the competition for the most productive task bundles in the cell (with the marginal product of $120,000).

This stylized example is not hypothetical. The H-1B visa was designed primarily for the purpose of creating the situation in the example. The visa is built to supply high-skill foreign workers to firms needing bundles of tasks performed in cells (cities, occupation codes, etc.) where the supply of natives to perform them is low. This equates to saying that those natives have higher-productivity, higher-paying options, within or outside the cell. Those who leave the cell did not have the opportunity to take up the highest-valued task bundles within the cell. The cell fixed effects remove them from the comparison.

[sub]occupations with the closest annual salaries to H-1Bs, adjusted by time and place, are not the creative professions of software engineers or even the higher-level technical professions such as programmers. Rather, they are lower-level supporting jobs, such as computer support specialists, technicians, and systems administrators. The wages are so much lower for these H-1Bs than the time-and-place-adjusted wages for creative professions that it is very unlikely that many of these H-1Bs are doing creative work."

Table 6 offers a toy example. All 200 workers in a schematic economy are paid exactly their marginal product, with no markdown. The average wage of the 100 natives is the same as the average wage of 100 immigrants. Natives split evenly between occupations A and B. Immigrants concentrate in the higher-paying occupation A, but in support rather than lead roles, maintenance rather than client-facing creative work. The raw comparison without an occupation fixed effect reports the true immigrant-native wage gap, zero. But with the fixed effect, comparing wages within occupations results in a gap of −13 percent. The fixed effect changed which natives each immigrant is compared to, *creating* the productivity gap in the comparison.

More formally, suppose that wages for worker i at firm f are set by $\ln w_{if} = X_i\beta + \psi_f + a_i + \tau_{if} - \mu_{if}$, where ψ_f is a firm pay premium (Abowd et al. 1999; Card et al. 2013; Song et al. 2019); a_i is unobserved worker productivity; τ_{if} is the value of the task bundle the firm assigns to the worker; and μ_{if} is a markdown. In a regression saturated with fixed effects for various cells (c), the coefficient estimate on the H-1B indicator in the core regression (1) within cells is

$$\hat{\gamma} = \underbrace{-\mathbb{E}[\mu \mid H]}_{\text{markdown}} + \underbrace{\mathbb{E}\big[\psi_f \mid H, c\big] - \mathbb{E}\big[\psi_f \mid N, c\big]}_{\text{firm sorting within cells}} + \underbrace{\Delta\mathbb{E}[a \mid c]}_{\text{selection}} + \underbrace{\Delta\mathbb{E}[\tau \mid c]}_{\text{task assignment}}, \tag{5}$$

where H workers hold H-1Bs and N workers are natives. The Borjas method sets the last three terms of the wage gap (5) to zero by assumption. This is inconsistent with the firm-effects literature, where the dispersion of firm premia among observably identical workers often ranges from 0.15 to 0.25 log points and workers sort on those premia. Outsourced work specifically carries premia 0.10 to 0.15 log points below premia at lead firms, for programmers but also for janitors and security guards (Goldschmidt and Schmieder 2017; Drenik et al. 2023; Card 2022).

4.2 Evidence that within-cell H-1B wage gaps are productivity gaps

The assumption that a negative wage gap (5) measures pure markdown is contradicted by evidence in the Borjas paper itself and by three further tests in its data.

Evidence that the wage gaps are mostly firm premia: Borjas (2026a, Table 3) presents estimates of the H-1B gap by firm, for the top 25 H-1B employers, as well as the average wage at each firm. The gap between firm-level H-1B wages and the *national* predicted "if native" wage varies

enormously, e.g. −0.002 at Google and −0.397 at Wipro. The paper reads these as wage markdowns, "payroll savings" that firms with larger gaps reap via market power. A simpler reading is that the different gaps represent different firm premia in equation (5). The H-1B workforce enters high-premium firms by competitive product-firm hiring, and enters low-premium firms through the outsourcing pipeline. Firm-level wage gaps, then, can spread from positive to deeply negative without any markdowns or "payroll savings" at all.[28]

These two readings have empirically testable implications for the wage gaps (5). If the wage gaps are firm premia, they must by definition correlate tightly with the overall productivity and thus pay level at the firm. If instead these gaps between firm-level H-1B pay and the *national* predicted "if native" wage are markdowns, as the paper interprets them, there is no reason for such a correlation. It is unclear why firms at lower pay levels would exert greater market power over workers in constant proportion to firms' pay level.

Consider the same data from the Borjas paper's Table 3, plotted in Figure 1a, with outsourcing firms highlighted. The same plot is extended in Figure 1b to all 1,486 firms with at least 30 H-1B visas in the administrative data (covering roughly 60 percent of H-1B workers), constructed to remove purely mechanical correlation.[29] The visa-weighted fit is strong ($R^2 \approx 0.5$), roughly linear across the H-1B wage distribution, and crosses zero from negative to positive in the middle of the distribution. All of these traits are consistent with firm premia: less-paid H-1B workers earn less because the firms they sort into are specialized in lower-productivity goods and services. Firm pay policy is firm-wide: relatively high-pay firms tend strongly to pay everyone more, janitors and accountants, natives and immigrants (e.g. Abowd et al. 1999).

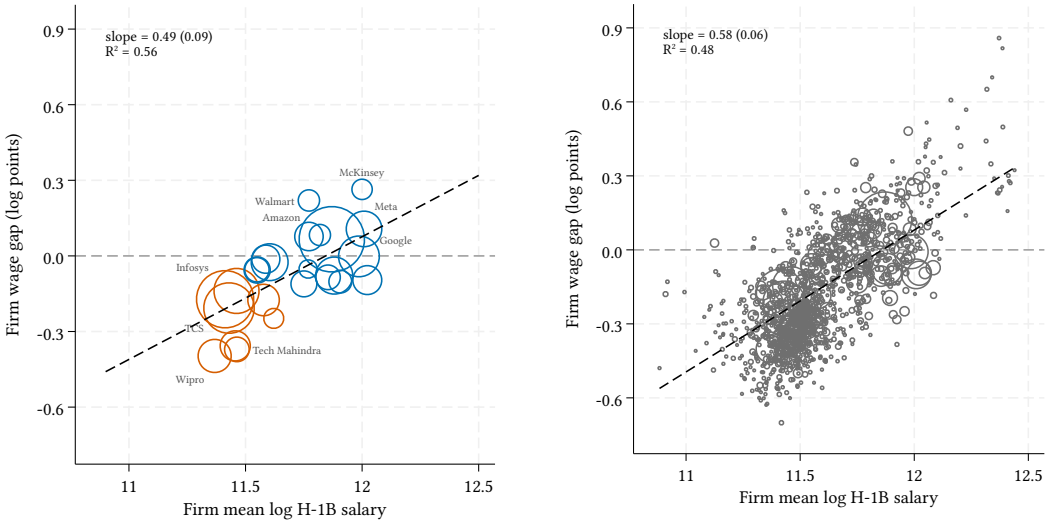
But none of these traits of Figure 1b are consistent with markdowns as a key driver of these firm-

[28] Wage gaps between natives and high-skill foreign workers on temporary work visas in Canada are mostly determined by firm premia, not within-firm wage gaps between comparable natives and immigrants (Kroft et al. 2026).

[29] For example, a small number of well-paid workers at some small firms, but not others, could induce correlation between the two variables in the plot mechanically. This is addressed in two ways by Figure 1b. First it is addressed by standard Bayesian shrinkage (Morris 1983; Chetty et al. 2014), which pulls each firm's estimated gap toward the all-firm average to a greater extent for firms with fewer workers, so that a small firm's noise-driven extreme number counts for less than a large firm's precisely measured one. Second it is addressed by a split-sample method: each firm's workers are randomly split into two samples, one of which is used to calculate the firm H-1B wage average and the other of which is used to calculate the pay gap. A single worker with an extreme pay value that influences one average cannot influence the other. The linear fit in Figure 1b is similar to the one in the data for large firms printed in Borjas (2026a, Table 3).

Figure 1: H-1B WAGE GAPS VS. H-1B WAGES, AT 25 FIRMS AND AT FULL SAMPLE

(a) *25 largest H-1B employers (Borjas Table 3)* (b) *Every employer with at least 30 visas*



Panel (*a*) shows the data printed in Borjas (2026a, Table 3), for the 25 largest H-1B employers. Bubble area is proportional to visas, orange color highlights outsourcing firms as classified in Borjas (2026a). The dashed line is the visa-weighted fit on firm mean log salaries. Panel (*b*) shows every employer with ⩾30 visas (1,486 firms), each circle one firm's empirical-Bayes-shrunken gap with area proportional to visas. In both panels, gaps are *not* within-firm wage gaps but firm-specific coefficients against the economy-wide conditional native benchmark, as in Borjas (2026a).

versus-national wage gaps. In the Borjas sense, the wage gap must be visa-specific and within-firm: each firm pays H-1B workers less than that firm itself would pay its own employees who are identically productive natives. The "gap" in the Borjas paper's Table 3 and in Figure 1 above is the firm-level H-1B pay minus an *economy-wide* benchmark. A firm-wide wage premium moves that gap one-for-one even if the firm treats *its own* H-1B workers and natives identically (zero markdown). Any rising within-firm markdown due to greater and greater market power has no reason to be a linear function of the firm's mean wage.

The markdown interpretation is likewise inconsistent with the right half of the picture, where the wage gap passes from negative to *positive*: reduced market power cannot push a wage above the counterfactual. A visa "effect" that gives employers power to mark down wages has that effect at all pay levels, and cannot cause employers to mark *up* wages. If the wage gaps in the figure were visa-induced markdowns, the scatter points would resemble a flat, loose cloud below

zero. But they exhibit a linear fit that crosses zero.[30]

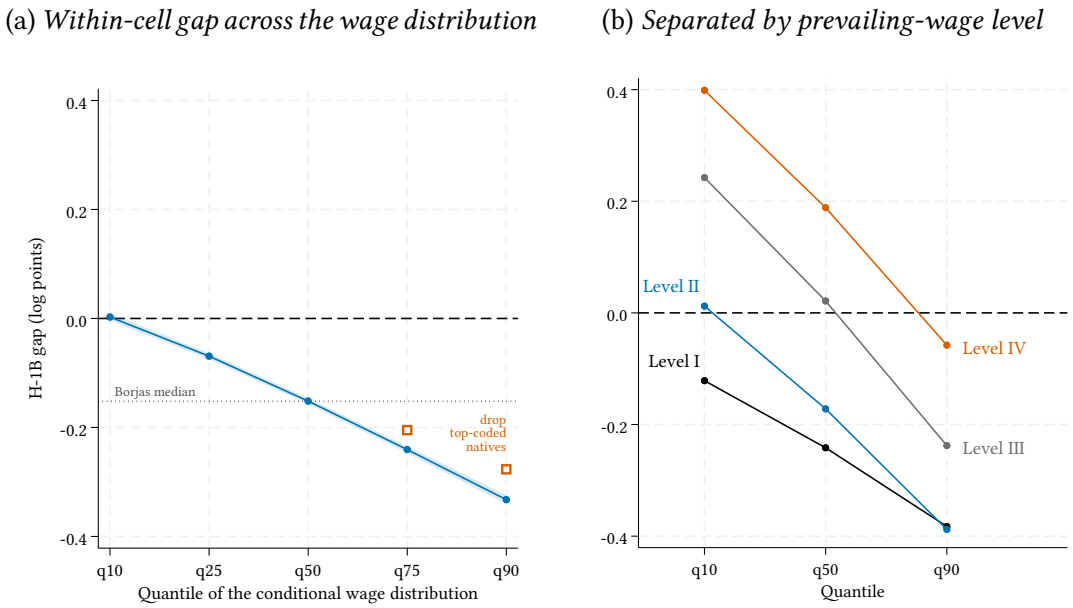
Two other points contradict the interpretation of the Borjas paper's gaps between firm pay and national benchmarks as a measure of markdowns. First, low-paying firms exist in similar proportions in all-native workforces, with a 0.15 to 0.25 log point dispersion in firm premia (Goldschmidt and Schmieder 2017; Drenik et al. 2023). Interpreting Wipro as a low-premium firm requires nothing beyond what labor economics knows about firms, but demonstrating markdowns requires showing that some workers within Wipro who do *identically-productive tasks* are paid less than others. Dostie et al. (2023) test the idea that immigrant-native wage gaps in general arise from immigrant-specific markdowns within firms, and reject it, both for immigrants overall and high-skill workers specifically.

Second, within-firm evidence exists for H-1B wages, but it does not support the Borjas interpretation. The Borjas paper highlights the finding of Bourveau et al. (2025), using payroll data from a single top US accounting firm, that the wage of newly-hired H-1B workers is 10 percent less than the wage of newly-hired natives with the same general occupational title at the same firm. But the same study finds that H-1B hiring by an office of the same firm causes either no decline or an *increase* in wages for natives of the same occupational title in the same office. If the gap were a markdown, hiring H-1B workers at less cost would tend to depress the wages of natives performing identically productive tasks. But both findings are consistent with specialization by H-1B workers, within job title, in lower-productivity tasks that complement the productivity of their native colleagues.[31]

[30] Why cannot markdowns simply occur entirely within firms, at rates that are coincidentally proportional to the wage level? Suppose a firm assigns worker i to task-bundle j yielding profit of $m_{ij} - w_i$, the difference between the match-specific marginal product and that worker's wage. It retains the higher-wage native (N) in task-bundle j, rather than the lower-wage H-1B worker (H) if and only if this is profitable: $m_{Nj} - m_{Hj} \geq w_N - w_H$, that is, the native's productivity advantage must cover her wage premium. If the two types of worker really have identical marginal product at certain task bundles $(m_{Nj} = m_{Hj},\ j \in J)$, the firm strictly prefers to assign those tasks to the H-1B worker and reassigns to the native other tasks where the native is more productive $(m_{Nj} > m_{Hj},\ j \in K)$, contradicting the premise of identical tasks.

[31] That very mechanism is well established in a long literature: Firms selected at random to hire H-1B workers do not hire fewer high-skill natives but exhibit higher rates of productivity growth and survival (Mahajan et al. 2025). Inflows of high-skill foreign STEM workers cause native high-skill workers to specialize in different and complementary tasks, orthogonally to occupation codes, that are relatively more intensive in interactive and communication skills (Peri and Sparber 2011; Lin 2019; Mayda et al. 2026). Wages for computer and engineering workers differ markedly by English language proficiency within occupation codes, consistent with task assignment that differs between immigrants and natives within occupation (Hunt 2015).

Figure 2: The within-cell H-1B wage gap across the wage distribution



Panel (*a*) shows the coefficient on an H-1B indicator variable in regressions otherwise identical to the Borjas (2026a) median regression (Table 1, panel B, col. 5) but at the range of quantiles p10, p25, p50, p75, and p90: +0.003 (0.003), −0.069 (0.003), −0.152 (0.003), −0.241 (0.003), −0.332 (0.004); N = 925,006. Orange squares re-estimate the upper quantiles excluding top-coded natives (−0.205 at q75, −0.277 at q90): ACS top-coding pulls the native upper tail down, thus biasing the top-end gap *toward zero*, making the main estimates conservative. Panel (*b*) shows otherwise identical results separated by prevailing-wage level, where the wage quantiles show quantiles of the *entire* wage distribution conditional on the standard fixed effects, *not* the within-level wage distribution.

Evidence from wage gaps by quantile: Separate evidence contradicts the Borjas paper's markdown interpretation of the estimated within-cell wage gap (5). Suppose that every H-1B worker in a cell is paid 16 percent below her marginal product, while natives are paid at their marginal product for the same tasks. If that is correct, workers should exhibit the same percentage gap at the 10th wage percentile, the median, or the 90th percentile: the H-1B wage distribution should slide the native distribution to the left. Now suppose instead that the within-cell wage gap comes from specialization in task bundles with different productivity. H-1B workers hold the cell's low-product bundles, natives extend into the high ones. The two distributions would differ greatly in shape: they would touch at the bottom, where the two groups overlap, but diverge at the top, where natives take on product-firm bundles where H-1B workers are underrepresented. This creates another empirical test of the markdown interpretation.

Figure 2a presents this test. It replicates and extends the Borjas paper's median regression (in

Table 1 above) to estimate the same within-cell gap at different quantiles. At the 10th percentile the within-cell gap is +0.003. The least-paid H-1B workers in a cell earn the same as the least-paid natives. The gap expands to −0.069 at the 25th percentile, −0.152 at the median (replicating the paper's estimate), −0.241 at the 75th, and −0.332 at the 90th. This test rejects the hypothesis of a within-cell H-1B wage distribution resembling the native distribution shifted to the left.

The observed pattern is inconsistent with a markdown story. A heterogeneous markdown could bend this profile, but consider what a markdown must do to fit the evidence: The markdown must be zero for the lowest-paid H-1B workers, those with the most limited resources and outside options. The markdown must rise to −0.332 log points for the highest-paid workers, those with the best outside options and most portable skills to take on high-productivity tasks. Monopsony power that rises steeply with the worker's wage goes against the main mechanisms that the monopsony literature proposes for markdowns (Manning 2003), and cannot arise from a visa held by everyone across the distribution.[32]

At first glance, it might seem that wage floors built into the H-1B program could generate the observed profile, compressing the wage gap more at the bottom than at the top.[33] But the evidence rejects such a reading. A wage floor that prevented markdowns where it is binding but not above that would make the wage-gap profile in Figure 2a exhibit a kink, hugging zero until wages were above the minimum, then slanting downwards. Instead the profile falls smoothly across the full wage distribution. More tellingly, a very similar downward-sloping profile is observed at every prevailing-wage level separately, in Figure 2b.[34] Different wage floors move the height of the within-cell gap (which controls for education and experience), but have little effect on the slope of the profile. The wage floor at Level I must bind at a much lower part of the *overall* wage distribution (around the 17th percentile), and the floor at Level IV must bind at a much higher part of the *overall* wage distribution (around the 67th percentile). If the slope of the profile arose

[32]To address concerns that topcoding of incomes in the native ACS sample could affect the result, the figure shows that dropping topcoded natives affects the slope of this profile only modestly.

[33]The Appendix compares H-1B wages to the prevailing-wage minimum for each worker. 68 percent of H-1B workers are offered wages above the legal minimum, with an average premium of 13 percent.

[34]Before employers petition for an H-1B visa, they must certify to the Department of Labor that they will pay at least the 'prevailing wage' for the occupation in the local labor market. The law requires that this floor be set at one of four experience-graded tiers (Levels I–IV) corresponding roughly to the 17th, 34th, 50th, and 67th percentiles of the government's most recent occupational wage survey (OEWS) for that occupation and area. The employer itself selects the level at filing, matching the position's required education, experience, and supervisory duties, indicating the job's seniority: Level I positions are entry-level while Level IV are the most senior and fully independent.

substantially from wage floors, the slope of the Level IV profile should be much flatter, lower in the wage distribution, than the Level I profile. Instead, all slopes are negative and of similar order.

It is furthermore difficult to explain how wage floors that are set to be binding at the typical native wage at Levels III and IV could generate large wage *premia* for H-1B workers over natives at the bottom of the wage distribution, as observed in Figure 2b. A straightforward interpretation of the figure is that the wage gaps to the right of the figure arise from native specialization in high-value task bundles where, because they are well-compensated, native supply is ample and few H-1Bs are recruited. That is, they arise principally from within-cell productivity differences rather than markdowns.

Evidence from wage gaps and native wages by H-1B prevalence: A final empirical test is inconsistent with patterns predicted by the Borjas paper's interpretation of negative within-cell wage gaps as markdowns. This test considers and rejects patterns that the markdown interpretation requires in the wage gaps and wage levels across city-occupation-industry cells with different prevalence of H-1B workers.

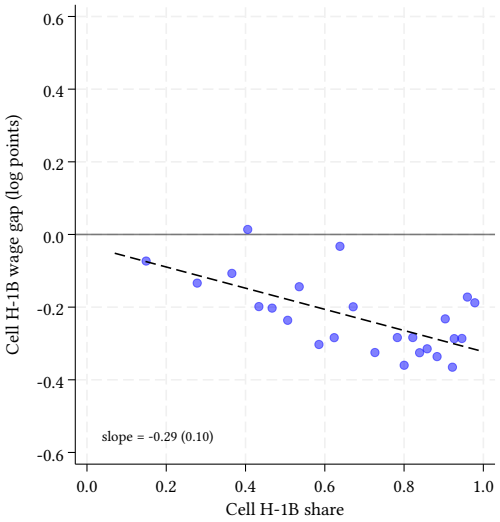
Figure 3a shows how the within-cell H-1B wage gap varies by the prevalence of H-1B workers in the cell.[35] The horizontal axis shows the H-1B share of all employment in each bin's average cell. The vertical axis shows the within-cell difference between average H-1B wage and average native wage. In cells where H-1B workers are more and more prevalent, the wage gap with natives in the cell grows more negative. Figure 3b presents a similar analysis of the absolute level of cell-level average native wages. Native wages do not fall, but might rise across cells with increasing H-1B prevalence.

Consider the predictions of the Borjas method's markdown interpretation of the wage gap. If

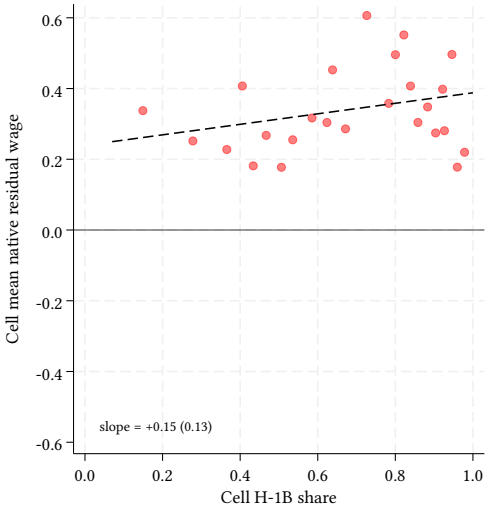
[35]The figure shows the 280 city-occupation-industry cells that contain at least 25 H-1B workers and 25 natives, and values for the 280 cells are averaged into 25 equal-weight bins along the share axis, for legibility. The dashed regression line is fit to the underlying cells, not the bins. Because the vertical axis is a difference of two means, the linear fit through the binscatter is weighted by $\frac{n_H n_N}{n_H+n_N}$, the inverse of the sampling variance of a within-cell difference in means, and the weight implicit in a regression of the wage on an H-1B dummy with cell fixed effects (Angrist and Pischke 2009, 76). Weighting by absolute cell size $(n_H + n_N)$ would be incorrect because e.g. a cell with 1,000 H-1Bs and 1,000 natives (size 2,000) does not contain ten times the information about their wage gap as a cell with 100 H-1Bs and 100 natives (size 200).

Figure 3: The wage gap and native wage level by H-1B prevalence in the cell

(a) *The gap steepens with H-1B prevalence*



(b) *Native wages do not fall with it*



Both panels show binned scatters across the 280 occupation × industry × metro-area cells with at least 25 workers of each group, and dashed lines are the weighted fits. Panel (*a*) shows the cell-specific H-1B gap versus the cell's H-1B share of employment. Panel (*b*) shows the cell-specific mean native residual wage (residualized on year, education, age, and gender) against the same share. The results are robust to a coarser definition of 707 occupation × metro cells (results not shown).

the H-1B visa lets employers underpay, that is a national institution, uniform across cells. That would imply a wage gap that is flat at some negative value, such as the paper's −0.16, in Figure 3a. If anything, a monopsony power explanation might make this relationship slope upwards: wage markdowns might be lowest in cells with the highest H-1B prevalence, where a high density and thick market of alternative sponsors erode wage-setting power. But the observed relationship is negative. A visa-driven monopsony story predicts nothing about native wage levels in Figure 3b.

Suppose instead that greater concentrations of H-1B workers bid down the wages in each cell, for both H-1B and native workers. Because the vertical axis of Figure 3a is the difference between those quantities, wage competition that depresses both of them likewise predicts a flat slope of that linear fit, but the observed relationship is negative. Wage competition would also require the native wage to fall with the H-1B share in Figure 3b, which it does not.

But interpreting within-cell wage gaps as productivity gaps is consistent with the evidence in the

figure. Cells with a high H-1B share are the cells whose low-productivity task mix natives often decline at the offered wage. Native wages are either flat or rising with H-1B prevalence because the natives who remain in high-share cells are those specialized, within those cells, in tasks that give them wages above their outside options. The negative slope of Figure 3a and the flat or positive slope of Figure 3b are inconsistent with the Borjas method's markdown interpretation.

5 Simulation: A test that cannot fail

A final major error renders the results of the Borjas (2026a) method difficult to interpret. The method used to predict the effect of the H-1B tax on employers' use of the H-1B visa quota, ostensibly based on the finding of a negative H-1B wage gap, cannot make useful predictions about the effect of the tax. This is because it predicts an employer response that is identical regardless of the estimated H-1B wage gap. The method's central demand scenario yields the same prediction even if the average H-1B wage gap is zero, that is, even if H-1B workers' average wages are identical to comparable natives' wages in the same city, occupation, industry, and sector. In fact, the test yields the same prediction *even if all information derived from data* is removed from the simulation. The results of the test are determined by assumed parameters rather than by any real-world observation.

This error stands apart from the errors of empirical estimation (*measurement*) and interpretation (*comparison*) discussed so far. The additional error would still render the Borjas method's predictions of employer behavior hard to usefully interpret even if the method correctly measured and interpreted the H-1B wage gap.

5.1 How the method simulates employer behavior

The Borjas (2026a) method uses its empirical estimates of the H-1B wage gap, interpreted as a wage markdown, as inputs to simulate the effect of an H-1B tax. The simulation finds that a $100,000 H-1B tax does not reduce labor demand enough to affect employers' usage of the 85,000-visa quota. This section explains how the simulation yields that conclusion.

Recall how the paper simulates firms' demand response to the tax (Section 2.1). It first assumes that any *positive* predicted "savings" $\left(\hat{\Delta}_h \geqslant 0\right)$ from hiring an H-1B worker consists entirely of a wage markdown for workers of identical productivity. It then assumes that any *negative* predicted "savings" $\left(\hat{\Delta}_h < 0\right)$ is illusory because it is offset by hidden productivity differences between natives and H-1B workers. The hiring condition (4) at each level of tax T is then empirically implemented by

$$\hat{\Delta}_h = \ln \hat{w}_{Nh} - \ln w_h \geqslant \tau(T) + \pi_h, \tag{6}$$

where the tax T as a fraction of the six-year hiring cost is $\tau(T) \equiv \ln\left(1 + \frac{T}{w_h e^{\pi_h} R}\right)$.[36] This says simply that at tax T, a given H-1B worker is still hired if the "savings" (3) exceeds the tax plus unobserved hiring costs. Two elements of the hiring condition must be estimated: The predicted "*if native*" wage ($\hat{w}_{Nh}$) comes from (2). And a hidden cost term must be imputed to each of the "negative savings" H-1B workers (π_h).

The method imputes hidden costs π_h to H-1B workers as follows. If the firm's "payroll savings" from hiring that worker is measured as negative (in equation (3), $\hat{\Delta}_h = \ln \hat{w}_{Nh} - \ln w_h < 0$), the method assumes that because every H-1B worker in the dataset was hired, that worker must carry a hidden negative cost $\pi_h \leqslant 0$ (productivity advantage) that caused the hire. That value is not observed or estimated, it is assumed from the premises that 1) the hire occurred and 2) the measured "savings" are exact, without bias or noise.

To simulate employer demand at each tax level, the method needs more than a per-worker bound. It needs a probability distribution for the hidden cost π_h in the population of potential H-1B hires. The method assumes a normal distribution: $\pi_h \sim N\left(\mu_\pi, \sigma_\pi\right)$. Neither parameter of the distribution comes from data. The mean is given assumed values $\mu_\pi \in \{-0.1,\ 0,\ +0.1\}$ that the paper asserts are plausible. The standard deviation $\sigma_\pi \equiv 0.194$ comes from the assumption that 99 percent of workers have hidden costs no larger than half a wage, positive or negative $\left(\Pr[-0.5 \leq \pi_h \leq 0.5] = 0.99 \text{ when } \mu_\pi = 0\right)$.[37] In short, the existence of the unobservable cost π_h and its entire distribution are set by assumption, using no moment of any data. Finally, the distribution of unobserved cost π_h is truncated from above at the "savings" $\hat{\Delta}_h$, to ensure that the employer's "savings" from hiring the worker remain positive even with a zero tax.

[36] R converts the six-year wage to present value at a 3 percent discount rate: $R = \sum_{t=1}^{6}(1+r)^{-t} = 5.42$.

[37] For a normal distribution, 99 percent of the mass lies within 2.576 s.d. of the mean. 2.576 is the 99.5th percentile of the standard normal, $\Phi^{-1}(0.995)$, leaving half a percent in each tail. Setting $2.576\,\sigma_\pi = 0.5$ gives $\sigma_\pi = 0.194$.

The simulation is then very simple. First, it estimates the "payroll savings" $\hat{\Delta}_h$ of every H-1B worker in the data by the "savings" condition (3).[38] Second, it assigns every H-1B worker a hidden cost consistent with having been hired: draw π_h from the assumed $N(\mu_\pi, \sigma_\pi)$ distribution truncated from above at the worker's $\hat{\Delta}_h$, so that every worker would have been hired under zero tax $\left(\pi_h \leqslant \hat{\Delta}_h\right)$ by construction. Third, for a given dollar-value tax T (whose log-point wedge is $\tau(T)$), the simulation retains workers as hired only if they meet the hiring condition (6). Finally, the second and third steps are repeated for 100 draws of π_h, at various levels of tax T.

5.2 The method's policy conclusion is invariant to the wage gap

The Borjas method interprets the fraction of workers retained at each tax as tracing out the demand-curve: employers' response to the tax. The method predicts employers' response to a tax of $100,000, under normal demand conditions (a "moderate excess demand" scenario where employers make 255,000 registrations in the lottery for 85,000 visas). The method estimates that employers would still demand 55 percent of H-1B workers, or about 140,000 workers in this scenario, well above the 85,000 cap. This is the basis for the central policy conclusion of Borjas (2026a), that even at "a fee near or even exceeding $100,000 … all 85,000 capped visas would still be used".[39]

Here the method encounters a major problem that is not apparent in the paper, shown here in Table 7. The Borjas method yields the same policy conclusion regardless of the average H-1B wage gap. The first row runs the Borjas simulation for the paper's headline wage gap of −16 percent, in the paper's central, "moderate" excess demand scenario. The second row replicates precisely the simulation conducted in the paper, in which the distribution of the wage gap is shifted so that the mean is 12 percent, adjusting for the measure of seniority bias used in the paper.[40] Employers continue to use the entire 85,000 visa quota, the basis of the paper's core

[38] Estimated savings greater than 50 percent or less than −50 percent are considered unreasonable and winsorized (that is, in log points, $\hat{\Delta}_h \in [-0.7, +0.4]$).

[39] This paper focuses on the "moderate" demand scenario of 255,000 in Borjas (2026a) visa-lottery registrations because it is already conservatively low. In all the years of the current lottery registration system's existence for which official counts have been published through and including March 2025, the number of registrations has exceeded 255,000 in *every* year, making it more reasonable to call 255,000 a "low" excess demand scenario in the absence of the tax (data in the Appendix). Registrations may have fallen below this in 2026 (official counts have not been published at the time of writing), but those reflect employer demand under the H-1B tax proclaimed in September 2025, and thus do not represent employers' demand in the absence of the tax.

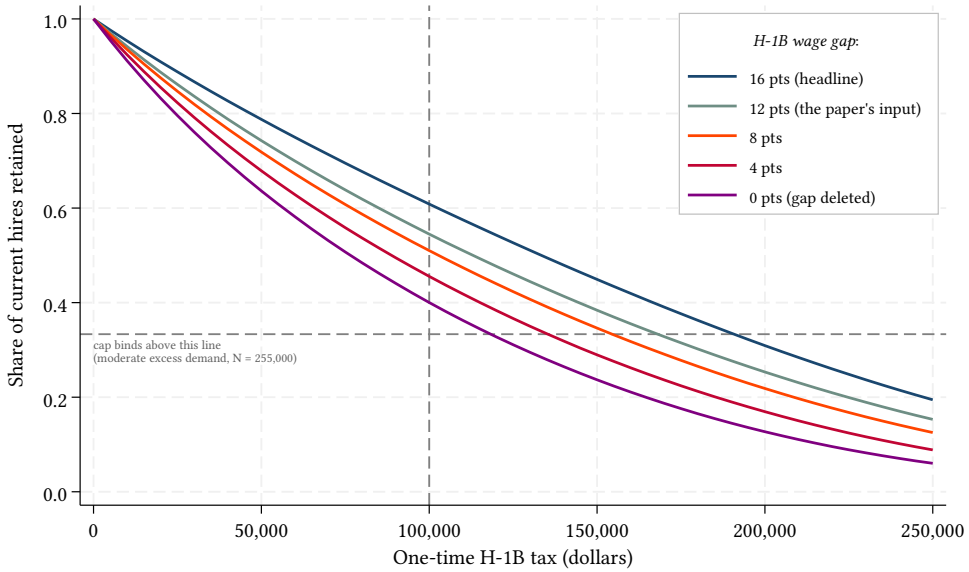
[40] The shift uses the paper's estimate that comparing native and H-1B workers at different levels of seniority biases

Table 7: The Borjas method's policy conclusion is invariant to the wage gap under normal employer demand conditions

True mean wage gap	*Effects of the enacted $100,000 tax*				
	Hiring rate	*Visas used*	*Revenue ($B)*	*Revenue-max. tax ($000s)*	*Arc elasticity*
16 pts (Borjas headline gap)	0.61	85,000	8.5	190	−3.3
12 pts (Borjas simulation input)	0.55 [≈0.55]	85,000	8.5	170 [170]	−4.0
8 pts	0.51	85,000	8.5	155	−4.4
4 pts	0.46	85,000	8.5	135	−5.2
1 pt	0.41	85,000	8.5	125	−5.8
0 pts (the estimated gap deleted)	0.40	85,000	8.5	120	−6.1

Every cell uses the Borjas (2026a) simulation method, and closely replicates the results printed in the paper (see Appendix), shown in square brackets. It retains the assumption of a hidden cost (π_h distributed normally with $\mu_\pi = 0$, $\sigma_\pi = 0.194$ and then truncated), in the Borjas method's central case of a "moderate excess-demand" scenario (255,000 registrants). Each row reruns the rebuilt tax simulation on $\hat{\Delta}_h$ estimates from the microdata after shifting every worker's measured payroll savings by a constant so that the mean equals the stated true gap (following the paper's procedure), applying the paper's capping of extremes at $[-0.7, +0.4]$. The 12-point row closely replicates the paper's printed findings using the same 12-point gap (in square brackets). Visas used are $\min(pN, 85{,}000)$, and revenue is the $100,000 tax times visas used. The arc elasticity is $\ln p/\tau$, where $\tau = 0.152$ is the log increase in six-year discounted employment cost that the tax imposes at the mean H-1B salary.

Figure 4: The Borjas method's simulated employer demand response to the tax under different H-1B wage gaps



See Table 7. The line for a 12-point H-1B wage gap replicates Borjas (2026a, Fig. 2A) for its central scenario of "moderate" excess demand, 255,000. The paper's simulations use a wage gap of 12 percent, while its headline finding is a 16 percent wage gap; the former but not the latter is adjusted for the paper's estimate of a 3.6 percentage-point seniority bias.

the wage gap by 3.6 percentage points, an estimate critiqued above (Section 3.3), rather than using its own headline finding of a 16 percent gap in its simulation.

policy conclusion. The subsequent rows of the table adjust the wage gap even further, down to zero.

At a wage gap of zero, where the existence of the H-1B program gives employers no "payroll savings" at all, the simulation nevertheless predicts that employers retain 40 percent of their untaxed demand, so that they still use all 85,000 visas available. Figure 4 replicates the Borjas method's plot of the employer response to the tax in the paper's Figure 2A, then shows what that figure looks like at various values of the H-1B wage gap. Under typical demand conditions ("moderate excess demand"), employers' predicted willingness to use all 85,000 available visas has nothing to do with the average wage gap. It is apparent from extrapolating the figure that under the method, employers would still hire all 85,000 workers even if the cost of hiring H-1B workers rose somewhat *above* the cost of hiring comparable natives, a result that contradicts the Borjas interpretation of the simulation results.

The Borjas paper's headline finding of a −16 percent wage gap (Table 1) is in fact irrelevant to the paper's central policy conclusion, as is any other estimate of the average wage gap. The Borjas paper's brief note that the policy conclusion "depends not just on the average wage gap, but also on the dispersion in payroll savings" (p. 31), does not hold. Under the central demand scenario, the principal policy-relevant conclusion that the cap binds (immigration is unaffected) is determined by the dispersion and is unrelated to the mean.

This matters because, as noted above, much of that dispersion arises from an assumed distribution of an unobservable parameter (π_h), without reference to data. That raises the question of how much, or if, the predictions of employer response to the tax rely on observed facts of any kind. The next section explores this.

5.3 The method's policy conclusion is determined without reference to data

To check the role of data in the paper's predictions, the first step is to derive an exact expression for the employers' tax-response curve (in the Appendix). What is needed is an expression for the probability that a given H-1B worker h is hired given 1) a tax T and 2) an assumed hidden cost π_h, in the hiring condition (6). For clarity, set aside for a moment the second-order effects of the

hidden cost π_h via the denominator of the tax term $\tau(T) \equiv \ln\left(1 + \frac{T}{w_h e^{\pi_h} R}\right)$, so that the proportional tax $\tau = \ln\left(1 + \frac{T}{wR}\right)$ for each worker is fixed by the dollar-valued tax T. Thus simplified, the tax-response curve is

$$p(T) \;=\; E_\Delta\left[\; \frac{\Phi\big((\Delta - \tau - \mu_\pi)/\sigma_\pi\big)}{\Phi\big((\Delta - \mu_\pi)/\sigma_\pi\big)} \;\right]. \tag{7}$$

The ratio inside the brackets is a conditional probability, using the cumulative distribution function of the normal (Φ). The numerator is the probability that the "hidden cost" π_h is small enough that an employer still hires the worker (condition (6)). The denominator is the probability that the "hidden cost" falls below the "savings" $\hat{\Delta}_h$, the only values admitted by the truncation step. The ratio is the conditional probability that the random draw of the hidden cost, from a distribution whose parameters and truncation are fixed by assumption, remains low enough to allow the worker to remain hired. The expectation of this probability requires averaging it over the observed distribution of the "savings" $\hat{\Delta}_h$.

This yields the simplified or first-order $p(T)$, the fraction of all H-1B workers hired without the tax who remain hired with the tax.[41] This first-order approximation of the hiring rate at a \$100,000 tax yields $p = 0.58$, very close to the corresponding Borjas estimate of 0.55 using the full simulation (including second-order feedback), in Table 7.

Now suppose that there is no "savings" at all, because every H-1B worker earns identically to the observably similar native: $\hat{\Delta}_h = 0$. This is more extreme than the exercise in Table 7, which assumed zero mean "savings" but allowed nonzero savings for some H-1B workers. Now there are no savings for anyone. The simplified hiring rate (7) at $T = \$100{,}000$ collapses to

$$p \;=\; \frac{\Phi\big((0 - 0.152)/0.194\big)}{\Phi(0)} \;=\; \frac{\Phi(-0.78)}{0.5} \;=\; \frac{0.217}{0.5} \;=\; 0.43. \tag{8}$$

In the full simulation including feedback effects, the corresponding hiring rate is 0.36.

This prediction of the model implies that employers who get zero benefit from hiring *any* H-1B

[41] The full response to the tax does not admit such a clear, closed-form solution, because it includes the feedback response: In the full response, employers' demand reacts slightly more to a given tax, because a high draw of the "hidden cost" π_h not only directly offsets the "savings" $\hat{\Delta}_h$ at a given tax τ but also reduces τ.

worker continue to hire 85,000 of them each year, so that immigration is unaffected, *even though they must pay $100,000 each* in order to save nothing on every hire. (In the central demand scenario considered in the paper, "moderate excess demand" of 255,000 registrations, even the 0.36 hiring rate means the quota remains oversubscribed by about 6,800 workers.)

This conclusion is guaranteed by the way the model is constructed, and by one parameter ($\sigma_\pi = 0.194$ in (8)). The model assumes that parameter, which is not a moment of any dataset. When the "savings" from H-1B hires is zero, the method does not conclude that they are marginal hires. It assumes instead that their observed wages must conceal some hypothesized and unobservable benefit. The method assigns this unobserved benefit to every hire observed when there is no tax. Then, when there is a tax, the method cannot estimate which H-1B workers save employers money (none do, by assumption). It estimates instead which H-1B workers' hidden, hypothetical advantage exceeds the tax. Because the distribution of the hidden advantage is fixed by assumption, the result is determined by assumption.

Put differently, the Borjas method assigns every worker a "bar" they must have cleared in order to get hired, set at their own "savings" $\hat{\Delta}_h$, then draws a hidden cost differential π_h below that bar. A worker's hire can survive the tax only due to the clearance between the worker and that bar, $\hat{\Delta}_h - \pi_h$. That clearance is what the tax can transfer to the government while still leaving the hire rational. Now suppose that correcting errors of measurement in the "savings" $\hat{\Delta}_h$ reduces every worker's bar by 0.10 log points. A useful simulation would conclude that hired workers have 0.10 log points less clearance to be taxed away. What the Borjas simulation does is assume that, therefore, the workers who cleared the bar must have had lower hidden costs π_h, so it shifts those lower too, because the draws of π_h come from beneath the new, lower bar. So clearance falls much less than 0.10 log points. This cycle continues all the way down to a wage gap of zero, ensuring by construction that there is always clearance to tax away, even when the conclusion has come to contradict its own premises because no clearance remains.

6 Conclusion

The Borjas method of evaluating the new high-skill immigration taxes is a rare case where a single academic study's novel method has directly guided major policy change with macroeconomic implications. Improved methods will better inform policy. Those improved methods will require approaches that remedy all three classes of flaw discussed here, because each separately makes the method's policy predictions difficult to interpret. The greatest promise for fruitful economic research at this time likely lies not in further assumption-laden simulations but in empirical observation of firms' real responses to the new taxes.

All of the discussion above addresses whether or not the Borjas (2026a) method can yield informative predictions of employers' response to high-skill immigration taxes. It does not address whether, even if the method correctly predicted employer response, the H-1B tax would be optimal. The study seeks to estimate a tax that would "maximize government revenue", not a tax that would maximize Americans' welfare. There are theoretical reasons to suspect that social welfare under an immigration tax that is set to maximize tax revenue could be generally lower than under welfare-optimal policies including wage floors and enhanced employer mobility for foreign workers, even with zero weight on non-citizen welfare (argued elsewhere, see Clemens 2026a). The strongest distributional case for the tax—that it generates revenue benefiting natives—is undermined by the typically native ownership of the firms bearing the tax, by adverse wage spillovers to native workers at the same firms, and by complementarity of foreign and native labor in production. A thorough economic analysis of the new high-skill immigration taxes goes beyond labor economics. It must examine the effect of the taxes on social welfare, not only on the volume of tax revenue. ■

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Online Appendix

Taxing High-Skill Immigration: Fundamental Problems in Evaluating Immigrant-Native Wage Gaps and the H-1B Visa Tax

Michael A. Clemens — October 2026

This Appendix presents derivations, supplementary results and robustness checks, and details of the dataset construction.

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1 Derivation of the employers' tax-response

Begin with the Borjas method's hiring rule set out in the main text. Employers continue to demand each H-1B worker h under tax T when the "savings" from doing so covers the cost of the tax and any hidden costs π_h,

$$\hat{\Delta}_h = \ln \hat{w}_{Nh} - \ln w_h \geqslant \tau(T) + \pi_h, \tag{A1}$$

where τ expresses the tax in terms of a log-point fraction of the total six-year cost to the employer, where a factor R converts the six-year flow into present value:[1]

$$\tau(T) \equiv \ln\left(1 + \frac{T}{w_h e^{\pi_h} R}\right). \tag{A2}$$

The hiring rule (Borjas 2026a, equation 8) becomes:

$$\pi_h + \ln\left(1 + \frac{T}{w_h e^{\pi_h} R}\right) \leqslant \hat{\Delta}_h. \tag{A3}$$

1.1 A simplified response curve compared to the full simulation

Notice that the assumed hidden cost π_h enters the hiring condition twice, on the left hand side and in the denominator of the tax term. This is because the log-point tax τ is estimated as a fraction of the overall hiring cost, which is assumed to include both the observed wage and the assumed hidden cost. For this reason there is no simple closed-form solution for the employer's response to the tax. It must be simulated from draws of π_h. Thus it is helpful to discuss the simplified result, ignoring the second-order effects of the hidden cost on the denominator of the tax term, which does admit closed form, before discussing the simulation results including that second-order effect.

That simplified effect, with the tax term (only) frozen at $\tau(\pi_h \equiv 0)$, is

$$\pi_h + \ln\left(1 + \frac{T}{w_h R}\right) \leqslant \hat{\Delta}_h. \tag{A4}$$

The difference between the simplified tax response (A4) and the full response (A3) is the second-order feedback effect of the "hidden cost" π_h on the tax term. The full simulation draws many values of π_h, recomputes the tax term τ at each individual worker's wage, caps extreme values, then averages the fraction of workers surviving the hiring condition (A3) across 100 replications.

For example, at tax $T = \$100{,}000$ and the mean wage, the log-wedge tax τ is $\ln\left(1 + \frac{100{,}000}{112{,}600 \times 5.417}\right) = 0.152$. The hidden-cost term π_h is drawn from the normal $N(\mu_\pi, \sigma_\pi)$, truncated from above at that worker's own measured "savings" $\hat{\Delta}_h$, so that the worker would still be hired without a tax (as all H-1B workers observed 2021–2024 were). The random draw of π_h is thus drawn from a truncated normal, meeting by construction the truncation rule

$$\pi_h \leq \hat{\Delta}_h. \tag{A5}$$

1.2 Solving for the employer response

In the simplified case, we can solve for a closed form of the employers' tax-response curve by using the probability that an H-1B worker with "savings" $\hat{\Delta}_h$ survives the hiring condition. This tax response curve must account for the facts that 1) the worker's hidden cost π_h is drawn from a normal with mean μ_π and standard deviation σ_π, and 2) this random draw is *already conditioned*: the truncation rule (A5) restricts

[1] R converts to present value at discount rate $r \equiv 0.03$: $R = \sum_{t=1}^{6}(1+r)^{-t} = 5.42$.

the hidden-cost draw to ensure that the original zero-tax hire was rational. So the probability of an H-1B worker's hire surviving the tax is a conditional probability. Conditional on the random draws of π_h lying below $\hat{\Delta}_h$, what fraction *also* lie below $\hat{\Delta}_h - \tau$? This is given by the standard conditional probability formula, with Φ as the cumulative distribution function of the normal:

$$P\big(\pi_h \le \hat{\Delta}_h - \tau \,\big|\, \pi_h \le \hat{\Delta}_h\big) \;=\; \frac{P(\pi_h \le \hat{\Delta}_h - \tau)}{P(\pi_h \le \hat{\Delta}_h)} \;=\; \frac{\Phi\big((\hat{\Delta}_h - \tau - \mu_\pi)/\sigma_\pi\big)}{\Phi\big((\hat{\Delta}_h - \mu_\pi)/\sigma_\pi\big)}. \tag{A6}$$

Because $\hat{\Delta}_h$ is unique to each H-1B worker, the *simplified* equation for the overall hiring rate arises from averaging the ratio (A6) over the distribution of "savings" $\hat{\Delta}_h$:

$$p(T) \;=\; E_\Delta\left[\, \frac{\Phi\big((\Delta - \tau - \mu_\pi)/\sigma_\pi\big)}{\Phi\big((\Delta - \mu_\pi)/\sigma_\pi\big)} \,\right], \qquad \tau = \ln\!\left(1 + \frac{T}{wR}\right). \tag{A7}$$

This simplified tax-response curve sets aside the feedback effect of the hidden cost π_h on the tax term. It shows in simple closed form the first-order effect driving the Borjas method's predictions of employer response to the tax. This simplified but transparent approximation is used in the main text alongside the less transparent but similar results of the full simulation.

2 Definition of taxes and fees

A compulsory and unrequited $100,000 per-head duty, without the explicit purpose of funding commensurate administrative costs, is a *tax* according to widely-accepted definitions in the law and in economics. *Black's Law Dictionary* defines a "tax" as "A charge, usu. monetary, imposed by the government on persons, entities, transactions, or property to yield public revenue" (Garner, ed 2024); the OECD (2025, 9) defines taxes as "compulsory unrequited payments to general government", and the IMF's definition is similar (IMF 2014). In the *Head Money Cases* (112 US 580 (1884)), the Supreme Court held that a per-head duty on immigrants could be a fee rather than a tax, only if tied to the government's cost of regulation. Neither the original H-1B tax (EOP 2025) nor its 2026 update (DHS 2026) documents or even claims that any service is being provided to H-1B visa holders of a value commensurate with $100,000.

The leading public finance textbook by Hyman (2010, 21) reads, "Taxes, the principal means of financing government expenditures, are compulsory payments that do not necessarily bear any direct relationship to the benefits from government goods and services received". The Congressional Budget Office (1993, 15) writes: "The legal distinction between fees and taxes is based on whether charges are levied for purely private benefits, or whether they confer public benefits that are independent of, rather than incidental to, private benefits. … The Congress may delegate to agencies the authority to levy charges that require private beneficiaries to pay for services that confer a public benefit, provided the Congressional intent to do so is clear and the guidelines for setting charges are adequately set forth in the legislation". In other words, again, a forced payment to the government is a tax if it funds the general treasury, not the provision of a specific service to a private user, unless Congress gives "clear" instruction otherwise in law.

3 Supplementary Results: Measurement

This section presents supplementary results and robustness checks related to the *Measurement* section of the main text.

3.1 Summary statistics, original vs. replication dataset

Table A1 compares the summary statistics for this paper's constructed replication dataset to the summary statistics printed in Borjas (2026a). The comparison is consistent with a successful and near-exact

Table A1: Summary statistics: this replication and Borjas (2026a), Table 1

	H-1B workers		Natives	
	Replication	*Borjas*	*Replication*	*Borjas*
Annual salary (1000s)	113.3	112.6	127.2	127.0
Log annual salary	11.58	11.58	11.54	11.53
Age	31.9	31.9	35.8	35.8
Male (%)	67.2	67.2	55.1	55.1
Bachelor's (%)	52.3	52.3	71.8	71.8
Master's (%)	41.8	41.8	20.9	20.9
Professional (%)	0.9	0.9	4.6	4.6
Doctorate (%)	5.1	5.1	2.7	2.7
Sample size	343,171	343,397	581,835	581,648

Table A2: The estimated H-1B wage gap: printed values of Borjas (2026a), Table 2, this replication, and the corrected data configuration

	(1)	(2)	(3)	(4)	(5)
A. OLS					
Borjas (2026a, Table 2)	0.044	−0.062	−0.023	−0.136	−0.161
Closest replication (Borjas geography/occ/ind, robust SE)	0.0458	−0.0627	−0.0241	−0.1389	−0.1592
	(0.0011)	(0.0017)	(0.0017)	(0.0023)	(0.0026)
Corrected (ZIP routing, tiered codes, 3-way clustered SE)	0.0458	−0.0741	−0.0346	−0.1467	−0.1594
	(0.0639)	(0.0485)	(0.0561)	(0.0396)	(0.0428)
B. Median regression					
Borjas (2026a, Table 2)	0.072	−0.026	−0.013	−0.127	−0.152
Closest replication (Borjas geography/occ/ind, robust SE)	0.0733	−0.0274	−0.0142	−0.1308	−0.1512
	(0.0012)	(0.0017)	(0.0017)	(0.0023)	(0.0026)
Corrected (ZIP routing, tiered codes, 3-way clustered SE)	0.0733	−0.0377	−0.0243	−0.1382	−0.1516
	(0.0383)	(0.0318)	(0.0271)	(0.0375)	(0.0327)
N (replication)	925,006	925,005	925,005	924,998	924,998

This table mirrors the core-result replication table in the main text, but with the progression of added fixed effects identical to the progression in Borjas (2026a, Table 2), demonstrating a successful replication.

replication.

This is verified with a near-exact replication of the core result (Borjas Table 2), in Table A2. The same table presents this paper's corrections of three minor errors in the Borjas analysis. One of these is the standard-error correction discussed in the main text.

The other two errors relate to errors in the construction of the dataset used in Borjas (2026a), correcting which does not substantially alter the coefficient estimates. These minor corrections arise from incorrect matching of workers to city codes, and incorrect translation of H-1B occupation/industry codes to the codes used for natives.

First, the Borjas analysis matches places-of-employment to cities by the employer's ZIP code using a crosswalk of ZCTAs (Zip Code Tabulation Areas) to city (CBSA) codes. But some major H-1B employers have an exclusively corporate ZIP code containing no residences and thus matching no ZCTA (e.g. Walmart's 72716). The Borjas analysis mis-assigns these to a state-wide pseudo-CBSA. The corrected estimates assign all H-1B workers to their true CBSA.

Second, the H-1B petition data contain occupation and industry codes that must be matched to the census codes of the native comparison sample, but the crosswalk used by the paper truncates industry codes, misses valid occupation codes of other vintages, and retains unmatched codes as catch-all categories, thereby treating "unmatched code" as a distinct occupation. For example, replicating the paper's results shows that a worker certified in SOC occupation 17-2071, *electrical engineers*, is assigned to the unknown-occupation code, because the Census occupation list carries that cell under the aggregated key 17-2070, electrical and electronics engineers, and the exact match finds no row for the detailed code. The correct procedure matches through the Census list's aggregated keys and its 2010-to-2018 crosswalk, which places the worker correctly in Census occupation 1410, *electrical and electronics engineers*. The replication likewise reveals that H-1B workers whose petitions list industry NAICS 513210 (the code the 2022 NAICS revision assigned to an industry previously coded 511210) are assigned to the unknown-industry code, because the truncated match is keyed to the older NAICS vintage and finds no entry beginning 5132. The correct procedure keys the lookup to both NAICS vintages.

3.2 Nationally-representative data on bonuses in total wage income

Counting supplemental pay for native workers (in survey data) but omitting it for H-1B workers (in administrative data) biases estimates from the Borjas method because supplemental pay like bonuses is a substantial fraction of pay for the relevant jobs. Table A3 reports nationally-representative estimates of supplemental pay in a custom tabulation of the Bureau of Labor Statistics (BLS) National Compensation Survey. Here, supplemental pay includes bonuses but excludes payment-in-equity, so the amounts in the table are a conservative lower bound on wage gaps that would arise artifactually from omitting both bonuses and payment-in-equity. Blank cells are those suppressed by BLS due to small samples. In H-1B intensive occupations and industries, typical bonuses (without equity) are by themselves in the range of 5–10 percent of base pay, for the mid-tenure workers in the native ACS sample used.

3.3 Firm-level data on bonus and equity compensation: leading H-1B employers

Data on typical bonuses actually paid to native workers in a single occupation—software engineer—were assembled for 16 of the firms that both employ the largest numbers of H-1B workers and employ substantial numbers of comparable US workers. Including firms that employ almost exclusively foreign workers would be uninformative about bonus payments received by native workers, the quantity of interest here.[2]

The firm-level data are gathered by *Levels.fyi Inc.*, a compensation platform that collects self-reported salary data from technology-sector employees, cross-validated with audits of pay statements and tax forms. Each compensation data submission records base salary, equity value, bonuses, job title, career level, and employer.

Table A4 reports average compensation for the lowest non-entry-level software engineers at 16 major H-1B employers, conservatively approximating the non-managerial US employees in the ACS sample. Cash bonuses at these firms are six percent of base pay. Top H-1B employers include firms (Amazon, Tesla) that pay incentives primarily in stock and accounting firms where cash bonuses are small. A conservative conclusion is that comparable native software engineers receive cash bonuses exceeding base pay by about six percent, excluding payment-in-equity.

It is not well documented in the research literature whether mid-level professional employees fully report, in response to census annual income questions, the portion of W-2 income that arrives as vested equity. But a substantial validation literature confirms that equity compensation is not categorically ex-

[2]Because this analysis is interested in bonuses paid to comparable US native workers reporting total wage income on the ACS, it excludes the firms whose employees are foreign nationals in their vast majority. These include Infosys, Tata Consultancy Services, and HCL Technologies. Variable pay received by the predominantly noncitizen workers at those firms is typically low. But almost all of the relevant employees at these firms would not appear in the subsample of ACS workers restricted to native-born US citizens, so including them in the analysis here would be incorrect.

Table A3: Supplemental pay as a percent of wages and salaries, for H-1B-relevant industries and occupations in nationally-representative data

Industry	Occupation	*All levels*	*Entry*	*Intermediate*	*Experienced*
All industries	All occupations	5.9	4.9	5.4	6.3
All industries	Computer and mathematical	6.3	—	5.0	6.1
All industries	Software developers	7.0	—	—	6.8
All industries	Architecture and engineering	7.1	—	7.1	6.1
All industries	Computer/IS managers	7.5	—	—	—
Information	All occupations	7.6	6.7	6.3	8.7
Information	Computer and mathematical	8.3	—	8.4	8.9
Information	Architecture and engineering	7.7	—	—	—
Finance and insurance	All occupations	10.4	5.6	8.3	11.3
Finance and insurance	Computer and mathematical	9.0	—	9.4	8.8
Finance and insurance	Software developers	9.5	—	12.9	9.3
Finance and insurance	Computer/IS managers	—	—	—	9.3
Prof./scientific/technical	All occupations	6.5	3.6	5.4	6.9
Prof./scientific/technical	Computer and mathematical	5.3	—	4.7	4.7
Prof./scientific/technical	Software developers	6.4	—	—	5.8
Prof./scientific/technical	Architecture and engineering	—	—	—	6.1
Prof./scientific/technical	Computer/IS managers	6.6	—	—	—
Computer systems design	All occupations	6.0	—	—	—
Computer systems design	Computer and mathematical	5.3	—	—	4.4
Computer systems design	Software developers	7.1	—	—	6.0

Custom tabulation prepared by the US Bureau of Labor Statistics from the nationally-representative National Compensation Survey / Employer Costs for Employee Compensation (ECEC): supplemental pay (premium pay plus nonproduction cash bonuses) as a percent of wages and salaries, private industry, full-time workers, average of 2021–2024. *Entry*, *Intermediate*, and *Experienced* correspond to NCS work levels (7–8, 9–10, and 11–12, respectively, for these occupations). Dashes mark cells suppressed by BLS due to limited subsample. The ECEC excludes stock and equity compensation, so every entry is a lower bound on the gap between base salary and total wage compensation.

cluded from census survey responses. Purcell (2024), linking CPS respondents to Social Security earnings records, finds that the median difference between survey and administrative earnings is near zero for prime-age workers, though top-quartile earners underreport by roughly 6 percent. The close correspondence between *average* survey and administrative earnings has been verified (Bollinger et al. 2019, 2025), where the administrative benchmark includes "employee gains from exercising nonqualified stock options" (Bureau of Economic Analysis 2023). Important discrepancies are concentrated among retirees and those earning property income, not working-age wage employees (Bee and Mitchell 2017; Bee et al. 2023).

A conservative conclusion from this literature is that the ACS-reported total wage income for US workers in Table A4 includes a substantial fraction of their payments-in-equity as well, though likely not all of it. At the median firm in the table, for the lower-level, non-managerial software engineers shown, payment-in-equity exceeds base pay by 4.5 percent. Assuming that just one third of that equity income is reported on census forms (1.5 percent of base pay), a conservatively low value in the context of the literature, this implies that US software engineers' cash bonuses and incentive payments-in-equity together exceed base pay at the median firm in the table by an amount exceeding 7.7 percent (= 6.2 + 1.5), but less than 14 percent. A more extremely conservative assumption would be that native workers in the occupations and firms that make them comparable to H-1B workers report *exclusively* variable pay that arrives as cash, and fail to report all payment-in-equity, which would imply that comparable native workers' total wage income is 6.2 percent higher than their base salary.

Table A4: Average compensation for the second career level for the occupation 'Software Engineer': Major H-1B employers that employ substantial numbers of US workers.

All dollar figures in $000s

Firm	Grade	Base	Bonus	(% Base)	Stock	(% Base)	$\frac{\text{Variable}}{\text{Base}}$, %
Amazon	SDE II (L5)	173	4	(2.3)	93	(53.7)	56.0
Apple	ICT 3	171	10	(5.7)	62	(36.5)	42.2
Google	L4	185	19	(10.1)	80	(43.4)	53.5
Intel	Grade 5	118	8	(6.7)	8	(7.1)	13.8
McKinsey	SE II	150	16	(10.7)	0	(0.0)	10.7
Meta	E4	182	21	(11.3)	110	(60.4)	71.8
Microsoft	60	142	9	(6.1)	26	(18.5)	24.6
Tesla	P2	153	1	(0.7)	48	(31.0)	31.7
Walmart	P2	122	5	(4.4)	14	(11.3)	15.7
American Express	SE II	118	7	(6.3)	2	(1.9)	8.2
Charles Schwab	55	107	9	(8.1)	0	(0.0)	8.1
Goldman Sachs	Assoc.	138	13	(9.2)	2	(1.7)	10.9
JPMorgan Chase	Assoc. (601)	121	17	(14.1)	0	(0.0)	14.1
Accenture	SE Analyst	99	1	(0.5)	0	(0.2)	0.8
Deloitte	Consultant/L2	109	3	(2.4)	0	(0.0)	2.4
EY	Senior SE	118	2	(1.9)	0	(0.1)	2.0
Average firm:				6.3		16.6	22.9
Median firm:				6.2		4.5	14.0

Workers' location is the United States (average across all cities). Percentages express the relevant pay component as a share of base pay and are computed from unrounded source figures. Sourced from Levels.fyi on February 25, 2026.

3.4 Testing whether the age profile of the wage gap proxies for firm type

The strong age gradient of the H-1B wage gap shown in the main text is consistent with the gradient arising from more and more discounted work experience, as larger fractions of that experience are acquired overseas. Table A5 shows the same pattern and rules out an alternative explanation. The first column splits the H-1B wage gap from a successful replication of Borjas (2026a, Table 2A, col. 5) into separate gaps for H-1Bs who come via change of status (already in US) versus consular processing (arriving from abroad), with the corresponding estimates printed in Borjas (2026a) in square brackets. Those who arrive directly from abroad (about four in ten workers) exhibit a much larger wage gap (as found by Sparber 2026). The second column shows that in the consular processing group, where greater age correlates closely with *foreign* experience, the age gradient in the wage gap is twice as steep as in the change-of-status group (a shift of −0.29 from under-30 to over-40 in the consular group, compared to a shift of −0.15 in the change-of-status group). The third column rules out a possible confounder: perhaps consular processing is simply proxying for a greater likelihood of employment at lower-wage outsourcing firms.[3] But this is inconsistent with the observed fact that the wage gap for each channel of arrival is substantially invariant to the type of employer.

3.5 The experience decomposition under the conservative imputation

Table A6 repeats the experience decomposition of the main text under the alternative degree-location imputation of Fortin et al. (2016), with the same column design. Whereas Friedberg (2000) imputes immigrants' degrees as US degrees when their age of arrival is 20–24, Fortin et al. classify a degree as foreign

[3] 'Outsourcing' firms are defined here as: Infosys, Tata Consultancy, Cognizant, Wipro, HCL, Capgemini, Tech Mahindra, and Larsen & Toubro

Table A5: The H-1B wage gap by arrival channel, age, and employer type

	(1) Channels	*(2) × age band*	*(3) × employer type*
Change of status (in US, mostly ex-F-1)	−0.115 (0.032) [−0.118]		
Consular processing (arrived from abroad)	−0.220 (0.043) [−0.219]		
Change of status, age under 30		−0.080 (0.025)	
Change of status, age 30–39		−0.152 (0.045)	
Change of status, age 40–50		−0.227 (0.051)	
Consular, age under 30		−0.057 (0.034)	
Consular, age 30–39		−0.229 (0.040)	
Consular, age 40–50		−0.349 (0.045)	
Change of status, direct employer			−0.114 (0.033)
Change of status, outsourcing employer			−0.144 (0.021)
Consular, direct employer			−0.215 (0.059)
Consular, outsourcing employer			−0.237 (0.016)
N		924,283	

"Ex-F-1" means people changing status from an F-1 international student visa to an H-1B work visa, whose vast majority received their highest degree from a US university. The Borjas (2026a, Table 2B, col. 5) specification with the H-1B indicator split by the I-129 petition's processing channel (column 1, the paper's printed value in square brackets), further interacted with age bands (column 2) and with employer type using the paper's classification (column 3). Standard errors, three-way clustered by occupation, metropolitan area, and industry, in parentheses.

by the criterion age-at-arrival minus 6 ⩾ years of education (along with a direct census question on degree location). Under this modification, Table A6 shows, the results do not substantially change.

3.6 Bias from unobserved tenure differences

The Borjas (2026a) paper's correction for seniority differences between new-hire H-1B workers and typical native workers in the ACS is a step in the right direction but incomplete. It understates the magnitude of the resulting bias by omitting natives with the lowest tenure, the only natives truly comparable to new-hire H-1B workers.

The omission of low-tenure workers arises as follows. The paper measures the seniority premium for native incumbents over native new hires in the Current Population Survey panel by linking the respondents to the Job Tenure Supplement (where respondents in the panel are asked, each January, how long they have been with their employers) to the ASEC (where the same people are asked, each March, their wage income *last calendar year*). Like the ACS sample used in the main analysis, the CPS sample retains only workers who worked 50+ weeks last year. Consider who in such a sample can have under one year of tenure in January yet 50 weeks of earnings the previous year: only someone who changed employers without a substantial gap. The reported income from the previous year must combine the low-tenure current job and the previous job they left at mid-tenure. Thus the paper drops the 574 workers with under a year of tenure.

But this creates a problem. The seniority premium in the CPS is thereby measured relative to workers with one to one and a half years of tenure, not relative to zero tenure, which is approximately the tenure of initial-employment H-1B workers. The Borjas paper defines the incumbent fraction of workers (0.886) as the share with more than 1.5 years of tenure (defining these as senior), and compares them to workers that have 1–1.5 years of tenure (incorrectly defining these as new hires).

The H-1B wage in the administrative data is not a retrospective wage for workers who have been on the job for over a year. It is an instantaneous wage at the moment of hiring. And US workers' wage at the moment of hiring *is* observed, in the same dataset: the January CPS supplement records a current,

Table A6: The H-1B wage gap under a single wage concept, without imputing native human capital prices to immigrants: Alternative (Fortin et al. 2016) imputation

	All college graduates	All college graduates	Top-10 cells	Top-5 cells
Borjas (2026), Table 7, printed	—	—	−0.094	−0.126
			(0.009)	(0.010)
This replication, same "likely H-1B" criteria	—	—	−0.096	−0.128
			(0.031)	(0.024)
Return to a US experience year	0.053	0.047	0.048	0.044
	(0.003)	(0.003)	(0.005)	(0.002)
Foreign-year return, gap vs. US year	−0.014	−0.009	−0.013	−0.011
	(0.002)	(0.001)	(0.002)	(0.002)
Foreign-degree gap, bachelor's level (naturalized)	−0.124	−0.055	−0.046	−0.046
	(0.029)	(0.026)	(0.093)	(0.042)
Additional foreign-degree gap, advanced (H-1B mix)	−0.026	−0.013	−0.004	−0.005
	(0.008)	(0.005)	(0.005)	(0.004)
Occupation and industry fixed effects	No	Yes	Yes	Yes
Implied overstatement at H-1B workers' own education, age, and channel distribution:				
Education component	0.055	0.025	0.020	0.020
Experience component	0.071	0.045	0.064	0.056
Total	0.126	0.070	0.084	0.076
	(0.017)	(0.013)	(0.040)	(0.020)
Replicated wage gaps (second row above), net of overstatement at the I-129 distribution:				
Adjusted likely-H-1B gap	—	—	−0.013	−0.052
			(0.051)	(0.031)
N (decomposition regressions)	606,793	606,790	36,049	22,489
N (naturalized-immigrant regressions)	654,657	654,654	38,403	23,667

Notes: See the corresponding table in the main text.

weekly wage. This allows the correct comparison of average-tenure workers to workers with 0–1 years of tenure.[4]

Table A7 presents this correction. The first column simply reprints the result of Borjas (2026a): correcting for seniority bias, the wage gap between H-1B workers and native workers by the Borjas method is −12.5 percent. The second column replicates that result with high precision. In column 2, the "seniority wage step" is the coefficient on the "senior" indicator variable in a regression of log annual wage income on the full control set (year, education, age, gender, metro, occupation, and industry), in the ASEC frame: after truncating workers with tenure under one year, and comparing workers with tenure over 1.5 years to workers with 1.0–1.5 years of tenure. The third column uses the same regression specification in the unrestricted January supplement data, where the wage is log(52 × usual weekly earnings), and workers with <1 year of tenure are included. This compares workers with tenure over 1.5 years (the Borjas definition of 'senior') to workers with tenure under one year (the correct definition of new hires). Column 3 also includes an indicator variable for workers with 1.0–1.5 years of tenure.[5]

[4] Accounting for the fact that some initial-employment H-1B visa workers have already been at the same employer for a short time on the Optional Practical Training work permit, Clemens (2026, Section 3.2.1) estimates that average employer tenure among initial-employment H-1B workers is 0.6 years. This accords with the 0–1 year comparison group in Table A7.

[5] The use of bonus-exclusive ORG wages (January Job Tenure Supplement, EARNWEEK2) in column 3 is appropriate. Bonuses grow with tenure, so a bonus-inclusive measure would wrongly fold part of the pay-concept wedge into the post-one-year seniority step. The bonus-exclusive ORG measure prevents the two corrections from double-counting

Table A7: The gap between H-1B starting base salary and native starting total wage income (same tenure)

	(1) *Borjas (2026)*	(2) *Replication of Borjas*	(3) *Correction*
Tenure frame	ASEC, year-round		January suppl., unrestricted
Definition of senior (years of tenure)	>1.5	>1.5	>1.5
Definition of new hire (years of tenure)	1–1.5	1–1.5	0–1
Seniority wage step (incumbents − new hires)	0.041	0.038	0.049
	(0.033)	(0.034)	(0.023)
Incumbent share	0.886	0.889	0.889
Seniority bias (wage step × share)	0.036	0.034	0.045
		(0.030)	(0.023)
Wage gap with Borjas method	−0.161	−0.159	−0.159
	(0.003)	(0.003)	(0.003)
Seniority-corrected wage gap	−0.125	−0.126	−0.114
		(0.030)	(0.023)

Col. 1 transcribes Borjas (2026a, Table 5, col. 2). The seniority premium is an indicator for natives > 1.5 years at their current employer, estimated in the CPS ASEC linked to the January Job Tenure Supplement, with the bias equal to the incumbent share times the premium and the corrected gap equal to the bias added to the ACS headline estimate. Col. 2 closely replicates that result. Col. 3 changes *only* the comparison group: "new hires" are defined as workers with tenure below one year, not 1–1.5 years as the paper defines them, measured in the January supplement frame (weekly earnings × 52, waves 2018–2024, full-time private for-profit workers, same controls). The weekly earnings of a worker with 0–1 year of tenure are the current job's wage by construction. Including the government and nonprofit workers (whose tenure profiles are steeper) who are dropped by the paper's restriction to private-only, raises the sub-year step to 0.072. Bias and corrected-gap standard errors combine the two surveys' errors under independence.

The result implies that the core result of a −16.1 percent H-1B wage gap in the Borjas paper, adjusted to compare workers of equal seniority (all else equal), is −11.4 percent rather than the −12.5 percent that the paper itself considers the "true" gap for the purpose of simulating employers' response to the tax. The correction arises from including the low tenure workers truncated from the paper's estimates of the seniority premium. This adjustment is necessary because only workers of equal seniority can convey accurate information about employers' relative costs of employing them.

3.7 Bias from dropping or imputing unobserved education

The problem of missing data in the Bloomberg (2025) dataset was not introduced by FOIA delivery, but is a feature of the underlying USCIS database. For example, USCIS reports that in its internal data, the education level is "unknown" for 32 percent of approved H-1B petitions in FY2023 (USCIS, Characteristics of H-1B Specialty Occupation Workers Fiscal Year 2023, Annual Report to Congress, March 6, 2024; Table 6, p. 47). In FY2024 this prevalence of missing education levels in USCIS's internal database was 10.1 percent (USCIS, Characteristics of H-1B Specialty Occupation Workers Fiscal Year 2024, Annual Report to Congress, April 29, 2025; Table 6, p. 44). These numbers describe missing data for all approved petitions, cap-subject or not, and are thus not directly comparable to the missing data prevalence in the Bloomberg dataset. The USCIS *Characteristics* reports from 2021 and 2022, in line with the Bloomberg data from those years, do not report substantial numbers of missing education values. USCIS (2026, 3) states that the missing data arise from transition to a new recordkeeping system in FY2023, which may have differentially

the bonus channel.

affected the records of H-1B workers educated outside the US.

Table A8 shows that in half the H-1B sample (FY2023–2024), 35 percent of workers have a missing education value, which the paper imputes by assuming that all workers with missing education hold a bachelor's degree. Because the fraction of H-1B workers with observed bachelor's degrees is stable across FY2021–2024 (and in prior years), but the fraction with observed master's degrees collapses in FY2023–2024, a large fraction of the H-1B workers with unobserved education are therefore likely to hold master's degrees. The decision to assume they all hold bachelor's degrees causes a very large shift in the educational composition of the sample used in the Borjas estimates. The Borjas (2026a) imputation shifts the fraction of the sample with a bachelor's degree in FY2023–2024 from 39.2 percent (observed, Table A8) to 74.2 percent (observed and imputed, Table A8b).

It is not possible to impute the level of education from other information in the H-1B petition (I-129) files. In particular, H-1B workers of various education levels can be found among the winners of both the basic "B" and master's "M" lotteries. Dropping workers with missing education data has the potential to create bias of unknown sign and magnitude, because the education field may not be missing-at-random with respect to wage levels. It is simply not possible to know the education level of the H-1B workers for which it is unreported, and how their true education level relates to their wage.

Table A9 shows that all else equal, the wage gap found by Borjas (2026a) under imputation of missing education levels is 1.6 percentage points larger than the wage gap in the years where education is fully observed (2021–2022), in both the OLS and median regressions.

4 Supplementary Results: Comparison

This section presents supplementary results and robustness checks related to the *Comparison* section of the main text.

4.1 H-1B employers set wages for most workers above the legal floor

Figure A1 and Table A10 show that H-1B employers do set the wages for most H-1B positions, rather than simply pricing most workers at the legally-required floor. The average premium of employers' H-1B offers over the legally-required 'prevailing wage' floor is 13 percent at outsourcing firms, where 66 percent of H-1B hires are offered above the minimum, within the same occupations and metros. At other firms the average premium paid to H-1Bs above the floor is 10 percent, and 68 percent of H-1Bs are offered above the legal minimum. The average premium paid above the legal minimum is only somewhat different between entry-level workers (Level I, 14 percent) and the most senior workers (Level IV, 19 percent). The wages of the large majority of H-1B workers, at all levels of seniority, are set by firms rather than by the government.

5 Supplementary Results: Simulation

This section presents supplementary results and robustness checks related to the *Simulation* section of the main text.

5.1 Successful replication of the Borjas simulation analysis

To verify that this analysis successfully replicates the Borjas (2026a) simulation method with high precision, it shows the replication of Borjas (2026a, Figure 2) in Figure A2, and of its Table 8 in Table A11. The figure shows sensitivity of the results to the assumed mean of the "hidden cost" parameter π_h, not sensitivity to the wage gap estimate $\hat{\gamma}$.

Table A8: The H-1B worker degree mix: fully observed years against imputed years

(a) *All records: the education field as recorded, and as estimated*

	FY2021–22, as recorded	FY2023–24, as recorded	FY2023–24, as estimated
Bachelor's degree (%)	31.3	39.2	74.2
Master's degree (%)	62.8	19.7	19.7
Professional degree (%)	0.9	0.9	0.9
Doctorate (%)	5.0	5.1	5.1
Education field missing (%)	0.0	35.0	0.0
N	175,878	167,293	167,293

(b) *The distribution of observed education values*

	FY2021–22	FY2023–24, education observed	FY2023–24, as estimated
Bachelor's degree (%)	31.3	60.3	74.2
Master's degree (%)	62.8	30.4	19.7
Professional degree (%)	0.9	1.4	0.9
Doctorate (%)	5.0	7.9	5.1
Education field missing, imputed bachelor's (%)	0.0	—	35.0
N	175,878	108,712	167,293

Percent of H-1B workers in the paper's baseline (I-129 form) estimation sample at each level of education, by fiscal-year period. The education field is essentially fully observed in fiscal 2021–22 and missing for roughly a third of records in fiscal 2023–24. The Borjas (2026a) method imputes a bachelor's degree to all missing records. Panel (*a*) includes every record in the denominators, with the missing records as a row: the first two columns report the education field as recorded, and the third reports it as the paper's estimation uses it, with the missing records imputed to bachelor's. Panel (*b*) reports the distribution of *observed* values, which is all records during FY2021-22. The middle column describes records from the FY2023–24 period with education actually observed. Comparing panel (*b*)'s first two columns isolates the change in the observed degree mix across cohorts, comparing its last two shows how the paper's imputation shifts it.

5.2 Hand-calculation with simplified first-order Borjas tax-response equation

The main text reports an estimate of the H-1B hiring rate under a $100,000 tax using the simplified Borjas hiring rate equation to predict the response to the tax, an approximate but easy-to-follow replication of the paper's result of $p(T) = 0.55$ in order to clarify how that result is generated. Table A12 shows the full calculation, which gives an approximate answer by averaging at five points in the "savings" distribution reported in the paper rather than integrating over the entire distribution.

Replicating the Borjas results requires some distribution of $\hat{\Delta}_h$ in (A7). After the seniority adjustment, the mean used in the paper is 0.120. Its standard deviation is not reported, but the paper reports the 1st percentile (−0.609) and the 99th (+0.737), which under normality represent 2.326 standard deviations either side of the mean. The implied standard deviation is $(0.737 - (-0.609))/(2 \times 2.326) = 0.29$. In this paper's replication the observed value is 0.29. So this analysis assumes $\hat{\Delta}_h$ distributed normally with mean 0.120 and s.d. 0.29 (capped, following the method, at $[-0.7, +0.4]$).

Borjas (Fig. 2) estimates that the $100,000 tax yields a hiring rate $p(T)$ of about 0.55 in the paper's central scenario ($\mu_\pi = 0$, $\sigma_\pi = 0.194$). Even the simplified tax-response curve (A7) comes close to replicating this result. Approximate the answer not by integrating across the entire distribution of $\hat{\Delta}_h$ but by averaging

Table A9: The Borjas-method gap in years with no missing education

	(1)	(2)	(3)	(4)	(5)
A. OLS					
Borjas (2026a), printed (**pooled 2021–2024**)	0.044	—	−0.023	−0.136	−0.161
2021–22 only, Borjas configuration (robust SE)	0.0618	0.0783	−0.0243	−0.1329	−0.1450
	(0.0016)	(0.0023)	(0.0027)	(0.0037)	(0.0040)
2021–22 only, corrected SE and dataset construction	0.0618	0.0783	−0.0348	−0.1403	−0.1435
	(0.0621)	(0.0667)	(0.0506)	(0.0309)	(0.0341)
B. Median regression					
Borjas (2026a), printed (**pooled 2021–2024**)	0.072	—	−0.013	−0.127	−0.152
2021–22 only, Borjas configuration (robust SE)	0.0898	0.0889	−0.0132	−0.1231	−0.1357
	(0.0015)	(0.0023)	(0.0027)	(0.0036)	(0.0040)
2021–22 only, corrected SE and dataset construction	0.0898	0.0889	−0.0232	−0.1300	−0.1344
	(0.0392)	(0.0386)	(0.0297)	(0.0315)	(0.0268)
N (2021–22)	454,948	454,948	454,947	454,939	454,939

The first row is the same as in the core-result table of the main text. The second row is identical but for restricting the sample to years where H-1B workers' education is observed (2021–2022). The third row presents the same estimates with correct standard errors and minor corrections of dataset construction as in the main text.

Table A10: Offer premia over the prevailing-wage floor, by firm type and wage level

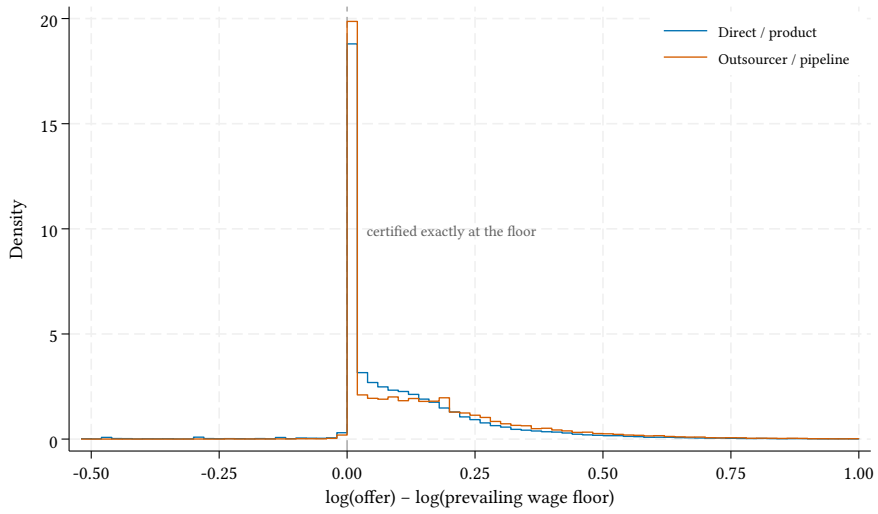
	Mean premium	*At floor (%)*	*Median*	*Within-firm SD*	*N*
Direct / product employers	0.101	32.0	0.052	0.102	168,214
Outsourcer / pipeline employers	0.131	34.3	0.068	0.119	182,055
Prevailing-wage Level I	0.140				89,357
Prevailing-wage Level II	0.096				190,996
Prevailing-wage Level III	0.138				40,893
Prevailing-wage Level IV	0.186				16,685

The offer premium is the log offered wage minus the log prevailing-wage floor for each certified position in the merged I-129 visa petition/LCA data for H-1B workers. The firm types are from the "H-1B-dependent" classification (named outsourcers, plus large firms with a Level I–II share of $\geqslant$0.6 or a third-party-worksite share of $\geqslant$0.5). *At floor* is the share of positions certified within half a log point of the floor. The bottom rows report mean premium by the position's assigned prevailing-wage level, holding level-specific floor fixed within each row. The LCA's own secondary-entity flag corroborates this classification: 93,176 positions work at third-party sites against 257,050 direct, and the above-floor premium holds within both segments (0.139 direct, 0.054 third-party).

at five points: the 10th, 30th, 50th, 70th, and 90th percentiles of the savings distribution.[6] Evaluating each of the five hiring probabilities with the simplified rate (A7), then averaging, yields 0.58. This is very close to the paper's full simulated response of 0.55, which includes second-order effects via individual-level variation in τ_h and feedback effects of "hidden cost" draws through the tax term.

[6]For the paper's mean 0.12 and standard deviation 0.29, those five "savings" values are $\Delta = 0.120 + 0.29 \times \{-1.28, -0.52, +0, +0.52, +1.28\} = \{-0.25, -0.03, 0.12, 0.27, 0.49\}$. That last value of +0.49 is then capped at +0.40, following the paper's capping at $[-0.7, +0.4]$. The tax at the mean salary \$112,600 is $\tau = \ln(1.164) = 0.152 = \ln\left(1 + \frac{100{,}000}{112{,}600 \times 5.417}\right)$.

Figure A1: Offer premia over the prevailing-wage floor, by firm type



The distribution of the difference between ln(offer) and ln(prevailing wage). 350,269 certified positions, direct and product employers in blue, outsourcing and pipeline employers in orange. Approximately two thirds of the mass sits above the floor, as in Table A10.

Table A11: Replication of Borjas (2026a, Table 8) simulation

Scenario	*Tax ($000s)*	*Hiring rate*	*Visas (000s)*	*Revenue ($B)*
A. Replication of Borjas (2026), Table 8, panel A ("low excess demand", 170,000)				
$\mu_\pi = -0.1$: this replication [Borjas]	160 [164]	.46 [.45]	78 [77]	12.5 [12.6]
$\mu_\pi = 0.0$: this replication [Borjas]	140 [140]	.41 [.42]	70 [71]	9.8 [10.0]
$\mu_\pi = +0.1$: this replication [Borjas]	115 [117]	.39 [.39]	67 [66]	7.7 [7.8]
B. Replication of Borjas (2026), Table 8, panel B ("moderate excess demand", 255,000)				
$\mu_\pi = -0.1$: this replication [Borjas]	210 [210]	.33 [.33]	83 [85]	17.5 [17.8]
$\mu_\pi = 0.0$: this replication [Borjas]	170 [170]	.33 [.33]	84 [85]	14.2 [14.5]
$\mu_\pi = +0.1$: this replication [Borjas]	135 [135]	.33 [.33]	84 [85]	11.3 [11.5]
C. Replication of Borjas (2026), Table 8, panel C ("high excess demand", 340,000)				
$\mu_\pi = -0.1$: this replication [Borjas]	240 [246]	.26 [.25]	85 [85]	20.4 [20.9]
$\mu_\pi = 0.0$: this replication [Borjas]	200 [204]	.25 [.25]	85 [85]	17.0 [17.3]
$\mu_\pi = +0.1$: this replication [Borjas]	165 [166]	.25 [.25]	84 [85]	13.8 [14.1]

The table compares this paper's reconstruction of the simulation to the results printed in Borjas (2026a, Table 8), in square brackets, verifying a successful replication with high precision.

5.3 Borjas excess demand scenarios compared to historical demand

Figure A3 shows that the number of lottery registrations assumed in the "low" and "moderate" excess demand scenarios in Borjas (2026a) are lower than the number of registrations in any year of the existence of the current lottery registration system. Only the "high" excess demand scenario exceeds the actual number of registrations in some (two) years of the electronic registration system's six-year history. It would be inappropriate to define these 'normal' demand scenarios (in the absence of the tax) by employer

Figure A2: Replication of Borjas (2026a, Fig. 2)

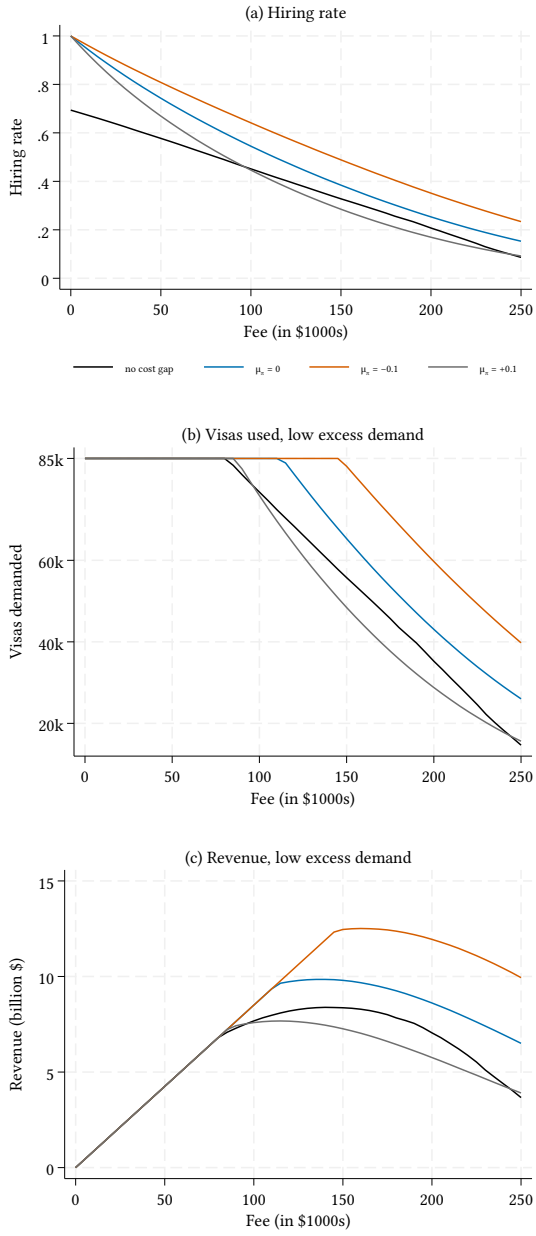
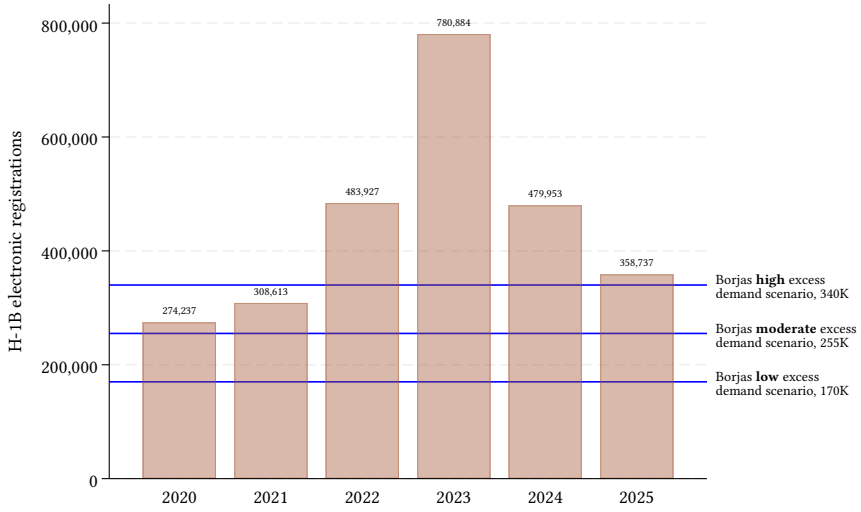


Table A12: The hiring rate at the enacted tax, replicating Borjas (2026a) approximately to illustrate the method

Worker	Δ	$\frac{\Delta - 0.152}{0.194}$	$\Phi(\cdot)$	$\frac{\Delta}{0.194}$	$\Phi(\cdot)$	*Survival ratio*
10th percentile	−0.25	−2.08	0.019	−1.30	0.097	0.19
30th percentile	−0.03	−0.95	0.172	−0.17	0.434	0.40
50th percentile	0.12	−0.16	0.435	0.62	0.732	0.59
70th percentile	0.27	0.62	0.732	1.40	0.920	0.80
90th percentile	0.40 ∗	1.28	0.900	2.06	0.980	0.92
Average (the approximate hiring rate at $100,000):						0.58

Five representative workers at the 10th through 90th percentiles of "savings" distribution in Borjas (2026a) (normal, mean 0.120, s.d. 0.29). * The distribution is then capped at the paper's $[-0.7, +0.4]$ range, which censors the 90th-percentile worker's savings at 0.40. Col. 3 standardizes each worker's savings net of the enacted tax's log-point cost (0.152 at the mean wage) by the assumed cost-gap spread $\sigma_\pi = 0.194$, while col. 5 standardizes the savings alone. By the simulation's truncation rule, each worker's probability of still getting hired despite the $100,000 tax is the ratio of col. 3 and col. 5. The exact simulation, which restores the feedback effect, gives 0.55, replicating the result of Borjas (2026a) at ≈0.55.

Figure A3: H-1B cap registrations since electronic registration began, by calendar year 2020–2025 (cap years FY2021–FY2026), before major H-1B policy changes of late 2025



From US Citizenship and Immigration Services (USCIS), "H-1B Electronic Registration Process," Historical Data. https://www.uscis.gov/working-in-the-united-states/temporary-workers/h-1b-specialty-occupations/h-1b-electronic-registration-process, Accessed September 28, 2026. The first year of the electronic registration system was cap year FY2021; the number of electronic lottery registrations is not comparable to the numbers of on-paper petitions from previous years. Generally, registrations for e.g. FY2026 are submitted in March of the preceding calendar (e.g. 2025) year for employment beginning on or after October 1 of that preceding calendar year.

demand from 2026 (cap year FY2027) and after, because demand in those years could plausibly respond to the advent of the H-1B tax in September 2025.

6 Dataset construction

The data sources are essentially identical to those used by Borjas (2026a), as evidenced by the near-exact replications in Table A1, Table A2, and elsewhere. Exceptions include the custom tabulation by BLS. The data sources are:

1. *H-1B I-129 petition microdata* (FY 2021–2024), obtained from USCIS under FOIA and released by Bloomberg (2025).
2. *DOL LCA disclosure files* (FY 2017–2025), from the DOL/ETA performance-data page, https://www.dol.gov/agencies/eta/foreign-labor/performance: annual for FY 2017–2019, quarterly for FY 2020–2025. Accessed February 10, 2026.
3. *American Community Survey* 2021–2024 one-year samples, from IPUMS USA (Ruggles et al. 2025).
4. *IPUMS CPS* 2018–2025 (Flood et al. 2025): January Job Tenure Supplement files (employer tenure JTYEARS, collected in even years; weekly earnings EARNWEEK2), March ASEC files (2022–2025), and outgoing-rotation weekly earnings (EARNWEEK), linked across interviews by CPSIDP.
5. *CPI-U* (monthly, all items), from BLS, https://www.bls.gov/cpi, used to build effective-status-date deflators to 2025 dollars. Accessed July 19, 2026.
6. *BLS National Compensation Survey*, custom tabulation: mean hourly employer costs for wages and salaries and for supplemental pay, by industry (NAICS), occupation (SOC), and work level, all establishment sizes.
7. *GEOCORR 2022* (ZCTA→PUMA and PUMA→CBSA), from Missouri Census Data Center, https://mcdc.missouri.edu/applications/geocorr2022.html, both assigned by plurality population. Accessed January 24, 2026.
8. *2018 Census occupation crosswalk*, from Census Bureau, https://www.census.gov/topics/employment/industry-occupation/guidance/code-lists.html, maps 2010- and 2018-vintage SOC codes and Census 2010 occupation codes to 2018 Census occupation codes. Accessed February 14, 2026.
9. *2023 industry crosswalk* (IND/INDNAICS), from IPUMS USA, https://usa.ipums.org/usa/volii/indnaics.shtml, maps NAICS codes to Census industry codes. Accessed July 19, 2026.
10. *UDS Mapper ZIP-to-ZCTA crosswalk* (2022), from Chris Prener's repository, https://github.com/chris-prener/uds-mapper, maps non-ZCTA ZIPs to the nearest ZCTA. Accessed February 12, 2026.
11. *IPUMS PUMA 2010-to-2020 population crosswalk*, from IPUMS USA, https://usa.ipums.org/usa/volii/pumas20.shtml. Used to harmonize PUMA vintages. Accessed February 13, 2026.

6.1 H-1B Data Construction

The raw Bloomberg (2025) dataset contains all H-1B lottery registrations, selections, and petitions for FY2021 through FY2024. Observations without an approved first decision, a certified LCA match (merged many-to-one on DOL case number), or a positive wage are dropped. LCA duplicates across quarterly files are resolved by keeping the most recent record. The primary wage is BEN_COMP_PAID (total annual I-129 compensation, complete across all fiscal years). An alternative (ANNUAL_WAGE_ALT) is built from WAGE_AMT/WAGE_UNIT where available (FY 2023–2024), annualizing non-annual rates, with fallback to BEN_COMP_PAID. Education is coded from ED_LEVEL_DEFINITION and BEN_EDUCATION_CODE, but 35% of FY 2023–2024 records have both fields blank. Replicating the summary statistics of Borjas (2026a) requires imputing all missing education values as bachelor's, and imputed values are flagged. ~370 records with observed sub-bachelor's education are dropped. Gender is BEN_SEX. Age is fiscal year minus birth year. Worksite ZIP is the first five digits of WORKSITE_ZIP. LCA variables carried forward: six-digit SOC code, NAICS code, prevailing-wage level (1–4), and full-time indicator. Firm-level analyses group records by employer identification number (FEIN), and the 25 largest H-1B employers are ranked by record count.

6.2 Crosswalks

Several different crosswalks are required to translate occupation, industry, and geographic codes between administrative and census data. These are:

Occupation. The Census workbook maps 2018- and 2010-vintage SOC codes to 2018 Census occupation codes; wildcard SOC codes (e.g. 15-113X) are expanded into all specific codes before merging, duplicates resolved with 2018-vintage priority, and residual unmatched codes assigned to broad "all other" Census codes by two-digit major group.

Industry. LCA NAICS codes map to Census industry codes through the INDNAICS crosswalk, harmonized to 2018 Census codes, with a four-digit NAICS fallback for unmatched six-digit codes.

ZIP to PUMA. GEOCORR 2022 maps each ZCTA to the PUMA with the largest population share (2020 delineation). H-1B worksite ZIPs that are not ZCTAs (~6,700 corporate/PO Box codes) get missing PUMAs; the UDS Mapper crosswalk routes these through the nearest ZCTA back to GEOCORR, recovering ~5,300. Records with no usable geography (178) are dropped.

PUMA vintage. The ACS uses vintage-2010 PUMAs for 2021 and vintage-2020 for 2022–2024; H-1B PUMAs are vintage-2020. The IPUMS crosswalk translates ACS 2021 codes forward to vintage-2020 (PUMA_CONSISTENT) and ACS 2022–2024 and H-1B codes back to vintage-2010 (PUMA2010). One-to-many mappings are resolved by plurality population.

Metro area. The corrected assignment (METRO) uses state × PUMA on the ACS side and the worksite-ZIP routing above on the H-1B side, with CBSAs from GEOCORR PUMA-to-CBSA where CBSA geography is needed. A replication variant (METRO_PW) reproduces the Borjas (2026a) geography: CBSA censored to the ACS-identified set, remaining records assigned to state FIPS pseudo-metros. Replication tables use METRO_PW, while corrected results use METRO.

6.3 Sample Restrictions, Wage Deflation, and Stacking

Borjas (2026a) documents several restrictions on the raw data, all of which are followed exactly as printed in the Borjas paper as the replication is constructed. The ACS comparison sample (2021–2024, pooled) is restricted to: native-born US citizens; bachelor's or higher, salaried, employed, age 21–50, full-time (≥35 hrs/wk), year-round (≥50 wks), with positive, nonmissing wage income (topcoded values retained), and a $34,780 annual wage floor. H-1B restrictions (post-LCA-merge): full-time (LCA field), age 21–50, no-geography records dropped, the same wage floor, 99.95th-percentile wage cap (≈$1.2M), and observed sub-bachelor's education dropped. Wages in both samples are deflated to 2025 dollars with the monthly CPI-U, at the paper's effective status date. H-1B workers receive weight 1, while ACS respondents receive PERWT. After stacking, BASELINE_SAMPLE flags all H-1B (343,171) plus native-born ACS respondents (581,835). Naturalized citizens and other foreign-born ACS respondents are retained with flags. The experience decomposition uses naturalized citizens with observed age at migration.

6.4 CPS Samples

The Borjas paper likewise documents several restrictions on the raw Current Population Survey data used to get the analytic dataset. These CPS restrictions mirror those used for the ACS: native-born, bachelor's or higher, salaried, employed, ages 21–50, full-time (⩾35 hrs/wk), annualized earnings above the same wage floor. Earnings are deflated to 2025 dollars with the monthly CPI-U. No year-round restriction is applied (monthly files lack annual weeks worked). Pre-2020 occupation codes (Census 2010) map to Census 2018 via the occupation workbook. CPS geography is state × metro area (METFIPS), H-1B records are assigned CBSAs via GEOCORR. The seniority analysis uses January Job Tenure Supplement files (2018, 2020, 2022, 2024 weekly earnings EARNWEEK2, annualized × 52; weight JTSUPPWT), linked by CPSIDP to ASEC records (2022, 2024) where the design requires both tenure and annual earnings. The pay-concept

comparison prices the same worker under two earnings questions: ASEC annual wage income (survey years 2022–2025, bonus-inclusive, the ACS concept) and outgoing-rotation usual weekly earnings annualized (earnings years 2021–2024, bonus-exclusive, earner-study weights), both for all respondents and for ASEC respondents linked to their own outgoing-rotation interview.

6.5 BLS Employer-Cost Tabulation

The data in Table A3 are unpublished estimates from the Bureau of Labor Statistics, National Compensation Survey / Employer Costs for Employee Compensation, a custom tabulation prepared by special data request. The estimates were provided by BLS Associate Commissioner Jennifer Coleman and delivered July 13, 2026. The tabulation reports mean hourly employer costs by NAICS industry, SOC occupation, and work level, private industry, full-time workers, all establishment sizes, averages of 2021–2024. The pay-concept wedge uses two cost categories: wages and salaries, and supplemental pay (premium pay plus nonproduction cash bonuses). The ECEC excludes stock and equity compensation: https://www.bls.gov/ecec/overview/ncs-data-requests.htm.

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