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Immigrant-Native Wage Gaps and Immigration Tariffs: Examining the Case for an H-1B Visa Tax

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March 2026

Abstract

The US government in 2025 imposed a $100,000 tax on each high-skill foreign worker entering with an H-1B work visa. The only public economic justification calculates the tax to offset an estimated wage penalty for H-1B workers relative to US natives. But this estimate suffers from substantial bias. Reexamining the same data shows that H-1B workers receive a modest wage premium relative to comparable natives, roughly 6 percent on average—inconsistent with any wage penalty—when using equivalent wage concepts and comparing workers of the same age, gender, education, and tenure, in the same occupation and local labor market. I trace most of the discrepancy to four methodological choices that inflate the prior estimate: 1) undisclosed imputation of missing data, 2) pooling of non-contemporaneous years, 3) a definition of local labor markets contradicting standard economic practice and US law, and 4) failure to consider H-1B workers' low job tenure. The remaining discrepancy arises from comparing incompatible wage concepts for H-1B versus native workers. Beyond measurement, the theory of public economics implies that a revenue-maximizing immigration tax reduces welfare relative to alternative policies, even with zero weight for immigrant welfare.

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1 Introduction

Some economists have long proposed substantial taxes on immigrant labor. Migration taxes might offset voters' concerns about costs of immigration, and allocate visas to the most efficient workers and employers (Becker 1987; Freeman 2006; Peri 2012, 17; Sharma and Sparber 2024).[1] Such taxes have been rare in practice; most governments regulate migration primarily through quotas. But in September 2025, the US Administration created a new, large migration tax by presidential proclamation.[2] As written, the proclamation requires employers to pay $100,000 per head for the entry of most new workers in its flagship program to admit high-skill temporary foreign labor, the H-1B visa. This tax is an unprecedented policy experiment. Its economic effects and justification will draw close scrutiny, given that research has linked H-1B workers to rising productivity, innovation, and economic growth (Peri et al. 2015; Chen et al. 2021; Khanna and Morales 2025).

This new migration tax is obliquely called, in the presidential proclamation, not a tax but a 'payment'. This is because the statutory authority for the policy (8 U.S.C. §1182(f)) contains only the authority to bar entry to aliens—which might be interpreted to create an authority to bar those failing to make an *ad hoc* payment. The statute does not create any authority to 'tax' migrants, which would require a separate act of Congress. But a compulsory and unrequited $100,000 per-head duty, without the explicit purpose of funding commensurate administrative costs, is a *tax*

[1] The theoretical case for pricing immigration entry originates in the optimal-tariff analogy applied to international factor movements (Ramaswami 1968; Calvo and Wellisz 1983; Jones and Coelho 1985; Clarke 1994; Chang 1998). A second strand develops concrete market-design mechanisms—auctions, tradable permits, and property-rights unbundling—for allocating visas (DeVoretz 2008; Chiswick 2009; Orrenius and Zavodny 2010; Orrenius et al. 2013; Casella and Cox 2018; Orrenius and Zavodny 2020; Lokshin and Ravallion 2022), while Fernández-Huertas Moraga and Rapoport (2014) extend the logic to tradable quotas across destination countries. Stark et al. (2017) formally compare entry fees with quotas, and Guerreiro et al. (2019, 2020) characterize the optimal immigration policy in a general-equilibrium framework with heterogeneous workers. Finally, visa pricing as an instrument to undercut migrant-smuggling networks is analyzed by Auriol and Mesnard (2016); Auriol et al. (2023).

[2] Proclamation 10973 of Sep. 19, 2025: Executive Office of the President, "Restriction on Entry of Certain Nonimmigrant Workers", 90 FR 46027, September 24, 2025. The proclamation states that "the entry into the United States of aliens as nonimmigrants to perform services in a specialty occupation under section 101(a)(15)(H)(i)(b) of the INA, 8 U.S.C. 1101(a)(15)(H)(i)(b) [that is, H-1B visas], is restricted, except for those aliens whose petitions are accompanied or supplemented by a payment of $100,000," subject to limited exceptions (§1). It specifies that the payment is made by employers: "The Secretary of State shall verify receipt of payment ... and shall approve only those visa petitions for which the filing employer has made the payment" (§2). A month after the proclamation, USCIS clarified that the tax "does not apply" to aliens who are already in the United States and seek change-of-status to H-1B from another visa, such as an F-1 student visa ("H-1B Specialty Occupations", Accessed Feb. 20, 2026).

according to widely-accepted definitions in the law and in economics.[3] A compulsory fee for a firm importing services provided by a foreign worker is a tax, just as a compulsory fee for a firm importing goods provided by a foreign firm—that is, a trade tariff—is a tax.

The H-1B tax is designed to raise employers' cost of hiring foreign workers, but little information has come to light about how it was designed. The Proclamation states that "relatively low-wage workers in the H-1B program…are detrimental to American workers' wages and labor opportunities". It thus justifies the tax on a threefold claim: that H-1B workers earn lower wages than equally productive US natives, that a tax on each H-1B worker raises public welfare by some criterion, and that a suitable tax rate is $100,000 per worker. The Proclamation does not explain how the White House arrived at the $100,000 figure.

Only one public document from a White House economist has emerged to present a case for the $100,000 tax. That study ties the amount of the tax to an estimation of wage gaps between H-1B workers and comparable native workers. This is an analysis by Borjas (2026), who was an official in the Executive Office of the President when the tax was created, a study released immediately after he left the government. Borjas writes, "*The average H-1B worker earns about 16 percent less than an American worker with the same education, age, gender, occupation, and who works in the same locality. Since the average salary of these high-skill workers exceeds $100,000, the average payroll savings accruing to a firm that wins an H-1B visa in the lottery are large: nearing $100,000 over the six-year employment term.*"[4] The implied tax, which Borjas describes as a tax to "redistribute the payroll savings now enjoyed" by H-1B employers, coincides exactly with the target (H-1B migrant workers) and amount ($100,000) of the tax created while Borjas, a major figure in immigration economics, was working within the White House advising immigration

[3] *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 it as "a compulsory unrequited payment to the general government"; the IMF's definition is similar (IMF 2014). In the *Head Money Cases* (112 U.S. 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. The H-1B tax proclamation made no such link. Borjas (2026) explicitly describes the tax as set to "maximize government revenue," which is inconsistent with the legal definition of a "fee."

[4] Borjas was a Senior Economist at the Council of Economic Advisors from March through December 2025, and did "extensive behind-the-scenes work on Trump's overhaul of the H-1B visa system for highly skilled workers that added a $100,000 fee" (Gurley 2026). Borjas (2026) does not mention any such role, nor that the White House enacted a tax matching the magnitude and incidence of the tax he recommends, while he was there. The study was released in February 2026, four months after the tax was ordered and one month after Borjas left the government.

policy.[5] Because of the obvious contextual relevance of this study to a major policy experiment, the methods used by this study merit careful consideration.[6]

In this paper, I critique the methods used to calculate $100,000 as the level of the H-1B visa tax. I first measure the wage gap between H-1B workers and comparable US natives. I use data on individual H-1B workers recently made available through the Freedom of Information Act by Bloomberg (2025), and compare them to individual US natives from anonymized census data. I then discuss several challenges that prior analysis has faced when measuring and interpreting the same wage levels with the same data. I document several flaws in the calculation of the relative wages of H-1B workers and thus in the $100,000 H-1B tax proposal. Finally, I discuss what can and cannot be learned about optimal immigration taxes from comparing immigrant and native wages.

I use administrative and census data on individual workers to place bounds on the wage gap between H-1B workers and comparable natives. On average, the *base salary* of H-1B workers is 1.2 percent less than the *total wage income* of US natives with comparable education, age, gender, occupation, and employer tenure, working in the same local labor market—a gap that cannot be statistically distinguished from zero. Interpretation of this wage gap is complicated by the fact that the two wage concepts differ between the administrative data for H-1B workers and the census survey data for US natives. The measure of H-1B workers' incomes includes exclusively the base salary of their one and only job. The measure of US natives' incomes includes income beyond the base salary—notably bonuses and payments-in-stock that are common in the tech industry. It furthermore includes income from all jobs, including any second job. This difference mechanically requires the *base salary* estimates for H-1B workers to fall below the slightly more inclusive *annual wage income* estimates for US natives.

It is straightforward to bound the resulting bias using nationally-representative compensation data and firm-level data on variable pay from the employers who account for most H-1B hires.

[5]Borjas is described as "the leading economic scholar on immigration" by Card and Peri (2016), and as "the founder of the modern study of immigration" by Katz (2025).

[6]I restrict this analysis to the first version of the Borjas (2026) paper, released February 9, 2026, because only that version—issued immediately after Borjas's direct involvement with H-1B policy inside the White House (Gurley 2026)—could be informative about the rationale used at the time the tax was created. Subsequent revisions may reflect information or analysis unavailable at the time the tax was designed, and thus cannot speak to the rationale that guided the policy.

For native workers comparable to private-sector H-1B workers, a conservative estimate is that variable pay and second-job income amount to more than 7 percent of base salary on average. This makes the evidence inconsistent with *any* wage penalty for H-1B workers, relative to comparable native workers, accounting for differences in wage concept and employer tenure. The evidence is exclusively consistent with a small wage premium for H-1B workers on average, relative to natives, in the neighborhood of 6 percent, with substantial uncertainty in that point estimate. This migrant-native gap is not informative about any gap between H-1B workers' wages and their own marginal revenue product, which could differ from that of natives.

Even the finding of a 1.2 percent wage gap between H-1B workers and US natives, with their two different wage concepts, differs sharply from Borjas's estimate of 16 percent using the same two incompatible wage concepts. This is despite using the same H-1B worker dataset, and comparing it to census data on US natives, to estimate the same target quantity. By closely replicating and then revising his analysis, I trace the entire discrepancy to four choices, each of which inflates his estimate: (1) an undisclosed and erroneous imputation of missing education levels for more than a third of H-1B workers in FY2023 and 2024; (2) comparing four years of H-1B data (FY2021–2024) against a single year of US native data (2023), for no stated reason, rather than straightforwardly comparing within the same years; (3) the unique decision to define local labor markets as Census Public Use Microdata Areas—units so small that (e.g.) metro New York City alone is split into 153 unconnected "markets," with H-1B workers assumed to compete only with natives whose *residence* lies in the handful of narrow neighborhoods where H-1B *worksites* are concentrated, ignoring all natives who commute even short distances; and (4) failing to consider that comparable US natives have over six years of tenure on average, and commensurately higher wages for this reason alone (Altonji and Shakotko 1987; Topel 1991; Altonji and Williams 2005), but almost no new H-1B employees have such tenure at the sponsoring firm.

Collectively these four choices explain why Borjas finds that the *base salary* of H-1B workers is 16 percent lower than the *total wage income* of comparable US natives rather than finding a wage gap of 1.2 percent that is statistically indistinguishable from zero. Further correcting this wage contrast to compare H-1B *base salary* to native *base salary* eliminates the gap entirely. This is a highly consequential disparity, because Borjas's wage gap estimate is the quantitative foundation on which the $100,000 magnitude of the tax proposal rests, a proposal identical in magnitude to the actual tax enacted while Borjas was a White House official, and on which future revisions of policy to determine the "prevailing wage" of comparable US workers may be based.

This finding implies that the magnitude of the H-1B tax described by Borjas (2026) suffers from a severe upward bias. The bias arises from unreported imputation of impossible values for missing data, from errors, from unconventional assumptions, and from comparing a more comprehensive wage measure for natives to a more limited wage measure for H-1B workers. Inserting the corrected wage gap estimates into the optimal tax model of Borjas would imply an optimal tax of zero at most, or a small subsidy.

Finally, setting aside empirical questions about relative wages, I discuss the public economic theory that connects the foreign-domestic worker wage gap to optimal immigration taxes. Borjas (2026) interprets any gap between the wages of H-1B workers (from immigration forms) and comparable US natives (from census data) as a rent that accrues to employers—a wedge between H-1B workers' wage and their marginal revenue product. This wedge is assumed to directly imply the magnitude of an optimal tax—"the size of the fee that would maximize government revenue" by redistributing rents ("the payroll savings now enjoyed by ... firms") to the government (p. 4–5). This concept of optimal taxation is highly nonstandard in the public economics literature. That literature generally defines optimal taxes as taxes that maximize social welfare (or at least native welfare), rather than taxes that maximize the amount of money collected by the government. Moreover, social welfare under a tax set to maximize government revenue is 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. The strongest distributional case for the tax—that it generates revenue benefiting natives—is undermined by native ownership of the firms bearing the tax, by adverse wage spillovers to native workers at the same firms, by complementarity of foreign and native labor in production, and by adverse selection among employers when heterogeneous firms compete for fixed visa slots.

The paper proceeds as follows. Section 2 summarizes the H-1B visa program and recently-available microdata on H-1B workers. Section 3 compares H-1B workers' wages to US natives' wages, noting challenges of comparing equivalent workers with equivalent wage concepts, and bounds the wage gap between H-1B workers and comparable native workers on average. Section 4 conducts a replication exercise to trace the causes of prior discordant estimates. Section 5 steps back from any particular estimate of the wage gap and challenges the theoretical case for imposing an immigration tariff in the amount of any given immigrant-native wage gap. Section 6 concludes.

2 The H-1B high-skill foreign worker visa, and data sources

The H-1B is a nonimmigrant visa that allows US employers to temporarily employ foreign workers in "specialty occupations" requiring at least a bachelor's degree or its equivalent. Created by the Immigration Act of 1990, the program was designed to channel high-skill foreign labor into sectors where domestic supply was perceived to be insufficient (Public Law 101-649, 104 Stat. 4978). An employer seeking to hire an H-1B worker must first file a Labor Condition Application (LCA) with the Department of Labor, attesting that the worker will be paid greater of what the employer actually pays its similar US workers, or the prevailing wage for the occupation and location, and that the hire will not adversely affect the working conditions of similarly employed US natives. The employer then submits the Department of Labor-certified LCA and a Form I-129, Petition for a Nonimmigrant Worker, with supporting documentation, to USCIS, requesting classification of the beneficiary in H-1B status for an initial period of up to three years, renewable once for a total of six years.

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 set of studies, typically using employer payroll records or administrative filings rather than surveys, 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. (2024) use payroll data from four accounting firms 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 marginal product.[7]

2.1 Regulation of H-1B visa workers

Congress in 1990 set the annual cap on new H-1B visas at 65,000 for the private sector.[8] A provision of the H-1B Visa Reform Act of 2004 added a further 20,000 visas reserved for beneficiaries who hold a master's degree or higher from a US institution, bringing the effective cap for private-sector petitions to 85,000 per year.[9] When the number of registrations exceeds the cap, USCIS conducts a random lottery to determine which petitions it will accept. Since FY2020, the basic-cap ("B") lottery has been held before the master's-cap ("M") lottery. This means the odds increased that registrations for beneficiaries with US advanced degrees were selected in the B lottery, since all US advanced degree holders are now in that draw; US advanced degree holders not selected in the 65,000-slot regular draw have a second chance at selection in the 20,000-slot master's draw (84 Fed. Reg. 888, January 31, 2019).

Once selected, the sponsoring employer files the Form I-129 petition. If approved, the beneficiary may apply for an H-1B visa at a US consulate abroad or, if already present on another nonimmigrant status, receive a change of status. The petition data—obtained through Freedom of Information Act requests by Bloomberg—record detailed information about each proposed job, including occupation, worksite location, and offered salary, and constitute one of the primary data sources used in this paper.

[7] 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, higher 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.

[8] 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).

[9] Since fiscal year 2014, demand has consistently exceeded supply, triggering the lottery process described below. In FY2026, for example, USCIS received eligible registrations for approximately 336,000 unique individuals from about 57,600 unique employers, most seeking just one H-1B worker, for 85,000 available slots.

2.2 Data sources on H-1B and comparable US natives

This study uses publicly-available sources of anonymized individual records on the wages of H-1B nonimmigrant workers and US natives. This section details those sources and their key limitations.

2.2.1 *H-1B petition records*

The H-1B data used in this study come from administrative records released by US Citizenship and Immigration Services under a Freedom of Information Act request filed by Bloomberg News, which obtained the records via the Freedom of Information Act (Bloomberg 2025, June 2025 update). The data cover all cap-subject H-1B lottery registrations, selections, and petition adjudications for fiscal years 2021 through 2024, corresponding to the lottery draws held each spring from 2020 to 2023 for private-sector employers.[10] Each observation represents a lottery registration for an individual foreign worker, linked to one sponsoring employer.

For registrations that advanced to a petition, the dataset is designed to include the individual worker-specific base salary per year, as well as the beneficiary's education level, age, gender, and nationality. A case number permits linking each petition to the corresponding Labor Condition Application in the Department of Labor's public performance data, identifying the ZIP code of the migrant's worksite (not only the address of the sponsoring firm) and the detailed occupation code. Essentially all workers in the dataset are new employees at the sponsoring firm; H-1B workers petitioning to continue employment for an additional three years at the same firm are not included. The wage reported on the I-129 is a guaranteed base salary that does not include bonuses or payments-in-equity.[11]

[10]Because cap-exempt employers (primarily higher ed and nonprofit research organizations) file petitions outside the lottery, they do not appear in these records.

[11]The law governing the wage that employers report on the I-129 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 §655.737(c)). 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 an expected minimum—as all similarly employed workers must be treated equally.

While the Bloomberg dataset is considerably richer than any previously available on H-1B workers, it has important limitations. Most consequentially, the education field is missing for large shares of H-1B workers in the later fiscal years—approximately 38 percent in FY2023 and 32 percent in FY2024. This is not a quirk of the FOIA delivery, but a feature of the underlying database from which USCIS created the data extract.[12] It is not possible to impute the level of education from other information in the 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. Simply 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.[13] 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. Thus the analysis below focuses on the years when education is fully observed for H-1B workers, 2021 and 2022, while also exploring the robustness to including the years of corrupted data, 2023 and 2024.

2.2.2 *American Community Survey*

The comparison sample of US natives is drawn from the single-year American Community Survey (ACS) public-use microdata files for 2021–2024, from IPUMS USA (Ruggles et al. 2025). The ACS is the largest household survey in the United States, sampling approximately 3.5 million addresses per year. It records annual wage and salary income, detailed occupation and education, and demographic characteristics. I restrict the ACS sample to native-born US citizen, full-time, full-year, salaried workers aged 21–50 who hold at least a bachelor's degree—the closest feasible US native analog to the H-1B population, whose minimum entry requirement includes employ-

[12]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.

[13]While the lottery registration information identifies which registered petitions are on behalf of an individual claiming a US master's or above, there is no way to see whether such a registered petition does contain a flag for whether the petitioner was selected in the master's ("M") or basic ("B") lottery, but 1) all registered petitions for a beneficiary indicating a US Masters or above not selected in the "B" lottery enter the "M" lottery, and 2) entry to the "M" lottery does not distinguish workers with master's degrees from those with professional or doctoral degrees. One sign that education is not missing-at-random with respect to wages is that education is more likely to be missing for workers changing status from an F-1 student visa.

ment in a specialty-occupation position typically requiring a four-year degree. Each US native's locality is identified by the commuting zone within which he or she resides (Tolbert and Sizer 1996). Commuting zones were created using data on workers' movements as they commute from home to work, often crossing administrative boundaries like county and city limits, and have become the standard definition of the spatial extent of local labor markets in economic research (e.g. Autor et al. 2019; Burstein et al. 2020). As in the H-1B base salary data, total wage income from the ACS is deflated to 2023 dollars with the Consumer Price Index for urban consumers (research series, CPI-U-US). I consider comparable *native* workers following Borjas.[14]

2.2.3 *Current Population Survey Job Tenure Supplement*

One potentially important dimension of worker heterogeneity that is absent from both the ACS and the H-1B petition records is employer tenure (or "job tenure"). Employer tenure is the "number of years that wage and salary workers had been with their current employer" (BLS).[15] New H-1B petition beneficiaries are, almost by definition, at the start of a job spell with their sponsoring employer, whereas comparable US natives observed in the ACS have a distribution of tenure that includes many years of accumulated firm-specific experience. It is well-established that wages rise with tenure, through returns to firm-specific human capital, deferred compensation, or match quality (Altonji and Shakotko 1987; Topel 1991; Altonji and Williams 2005). Thus a raw comparison between wages for H-1B and US natives in the ACS can confound the effect of visa status with the effect of tenure.

To quantify the tenure–wage relationship among observably similar US natives, I supplement the ACS with data from the Current Population Survey (CPS) Job Tenure Supplement, administered in January, biennially in even-numbered years (Flood et al. 2025). The supplement asks

[14] I define American 'natives' to be people who are natural-born US citizens, which includes not only those born in the 50 states or District of Columbia, plus US territories like Puerto Rico and Guam, but Americans born to US citizen parents overseas. The results are robust to restricting the definition of 'native' to exclusively people born in the 50 states and District of Columbia (see Appendix). Looking at US natives is different than looking at all US workers, which the Department of Labor describes for immigration purposes as all US citizens, US nationals, lawful permanent residents, refugees, and asylee recipients (20 CFR 656.3). Following Borjas (2026) as closely as possible, I exclude workers at the extremes of the wage distribution: a floor of "about $30,000" is applied to both the ACS and H-1B samples (I use $28,800), ACS observations with top-coded earnings are dropped, and the top 0.05 percent of the H-1B nominal wage distribution (roughly corresponding to salaries above $1.2 million) is trimmed.

[15] Employer tenure is most commonly called "job tenure" in CPS and BLS data, but both terms refer not to the number of years in a "job", such as with the same title, but the total number of continuous years at a single employer, even if holding different jobs there.

respondents how long they have worked continuously for their current employer. I apply sample restrictions that parallel the ACS as closely as possible—salaried natural-born US citizens age 21–50, employed full-time, with at least a college degree—and use weekly earnings from the supplement, annualized by multiplying by 52 and deflated to 2023 dollars.[16] Because the CPS lacks PUMA-level geography, I define geographic fixed effects at the level of Core-Based Statistical Areas (metropolitan areas), which are perfect supersets of counties. Similarly to commuting zones, CBSAs are designed to map counties into functional economic regions based on commuting patterns, and for cities of substantial size CBSAs are functionally similar to commuting zones.

3 The wages of H-1B workers and comparable US natives

The H-1B datasets from administrative records released to Bloomberg (2025) by US Citizenship and Immigration Services through the Freedom of Information Act allow direct comparison of H-1B workers' wage compensation with that of US natives, controlling for key wage determinants, at the individual level. This section first presents core estimates of the H-1B wage gap by comparing H-1B workers' base salary in the USCIS administrative data to US natives' total wage income in the ACS survey data, a comparison that has been influential in the literature. It then discusses challenges in interpreting those gaps: First, employer tenure differs between the groups, in a way that systematically reduces the observed relative wages of H-1B workers. Second, the measured wage concept differs between the groups, further reducing the observed relative wages of H-1B workers. Accounting for these differences implies bounds on the gap between comparable wages for comparable workers.

3.1 Comparing H-1B workers' base salary to US natives' total wage income

I estimate various versions of the standard Mincer (1958) regression,

$$\ln w_i \;=\; \beta \cdot \mathbb{1}\left(\text{H-1B}_i\right) \;+\; X_i'\gamma \;+\; \alpha_{g(i)} \;+\; \alpha_{o(i)} \;+\; \alpha_{t(i)} \;+\; \varepsilon_i, \tag{1}$$

[16]The \$28,800 annual wage floor used in the ACS is applied (equivalent to approximately \$554 per week).

where $\mathbb{1}(\text{H-1B}_i)$ is an indicator for whether worker i is an H-1B worker, with US natives as the base group; X_i includes demographic controls: single-year age, gender, and four levels of education (bachelor's, master's, professional degree, or doctorate); $\alpha_{g(i)}$, $\alpha_{o(i)}$, and $\alpha_{t(i)}$ denote fixed effects for geography, occupation, and year, respectively. In most specifications, the error term $\hat{\varepsilon}_i$ is allowed to be arbitrarily correlated within local labor markets, occupations, or both. Due to constraints of the underlying data, w_i represents two different wage concepts for the different groups of workers: guaranteed base salary for H-1B workers, and total wage income for US natives. The coefficient of interest is β, the wage gap between H-1B workers and US natives.

These wage gaps are presented in Table 1. Panel (a) reports estimates of the linear regression specification (1). H-1B workers' base salary is 17.6 percent higher than the total wage income of the average college-educated, prime-age, full-time US native, without controlling for any individual worker traits (column 1). When H-1B workers are compared to US natives in the same commuting zone, but no other traits, H-1B workers earn 5.7 percent more (column 2), which rises to 10.9 percent when demographic controls are added. In other words, when comparing workers only within the same local labor markets but across all occupations, the base salary of H-1B workers exceeds total wage income of US natives.

This changes sharply when comparing workers within the same occupation (Table 1, column 4). H-1B workers' base salary is 7.5 percent less than the total wage income of US natives within the same four-digit census occupation code—in the same commuting zone, and with the same demographic traits. This gap does not account for worker traits that are unobserved in the American Community Survey data, especially employer tenure, which will be examined further below.

Because wage income is top-coded in the ACS data (at a level far above median income), panel (b) of the table presents estimation of the corresponding quantile (median) regressions, whose results are unaffected by top-coding. The results are similar. In particular, the median base salary of H-1B workers is 6.8 percent less than the median total wage income of US natives in the same four-digit census occupation code, in same commuting zone, and with the same demographic traits—without considering differences in employer tenure.

Table 1: Initial Exploration of Wage Gaps: Comparing H-1B and US native compensation, without considering unobserved differences in wage concept or employer tenure

Dep. var.: ln real wage income	(1)	(2)	(3)	(4)
(a) *Linear*				
H-1B worker	0.1764	0.0567	0.1093	−0.0748
	(0.0013)	(0.0143)	(0.0174)	(0.0352)
Observations	645,264	640,656	640,656	640,636
(b) *Quantile (median)*				
H-1B worker	0.2072	0.0939	0.1250	−0.0682
	(0.0063)	(0.0144)	(0.0172)	(0.0348)
Observations	645,264	640,656	640,656	640,641
Fixed effects:				
Year	Yes	Yes	Yes	Yes
Local labor market (geography)	—	Yes	Yes	Yes
Age, education, gender	—	—	Yes	Yes
Occupation (4 digit)	—	—	—	Yes
Standard errors:				
Robust	Yes	—	—	—
Clustered: geography	—	Yes	Yes	Yes
Clustered: occupation	—	—	—	Yes

Sample includes only workers employed, full-time and year-round, age 21–50 with a college degree or above. Includes years when education levels are observed for both H-1B workers and US natives, that is, 2021–2022. Local labor markets are defined as commuting zones (Autor et al. 2019). Age fixed effects are by individual year; education fixed effects are for the four categories of bachelor's, master's, professional degree, or doctorate. Standard errors in parentheses; two-way clustering for quantile regressions uses the method of Cameron et al. (2011). US natives weighted with the census sampling weight, H-1B workers weighted by unity (full universe). All regressions include suppressed constant term.

It is worth emphasizing why the standard errors must be clustered in columns 2–4 of Table 1. Although immigration status varies across individuals—unlike a state-level policy variable that is constant within a jurisdiction—one cannot conclude that the "Moulton problem" is absent.[17] H-1B visa holders sort nonrandomly into a narrow set of occupations and metropolitan areas: conditional on belonging to a 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. The resulting intra-

[17] Moulton (1986) derives a variance inflation factor of approximately $1 + \rho_x \rho_\varepsilon (\bar{n}_g - 1)$, where ρ_x denotes the intracluster correlation of the regressor and ρ_ε that of the error. Abadie et al. (2023) formalize a related result showing that the ratio of cluster-robust to conventional variance is governed by the product of these intracluster correlations.

cluster correlation of the H-1B indicator is far from negligible, and the conditions under which Abadie et al. (2023) counsel against clustering are not satisfied. In this setting, the problem identified by Moulton (1986, 1990) and discussed by Cameron and Miller (2015) applies: conventional standard errors can overreject the null, perhaps severely.

The regressions in Table 1 do not control for employer tenure, which is unobserved in the ACS; subsection 3.2 below examines the role of tenure gaps between H-1B and native workers. The sample in Table 1 is restricted to 2021–2022, the years in which education is observed for essentially all H-1B workers (and natives). In 2023–2024, education is missing for roughly a third of H-1B records, and because missingness may correlate with both education level and wages, a regression that included the two thirds of workers with *observed* education in those years could introduce bias of unknown sign and magnitude. Appendix A3 nevertheless reports, for completeness, the results when workers with observed education levels are included, from all years 2021–2024, but not workers with unobserved education. The coefficients of interest are more negative by two percentage points, with a standard error of four percentage points, making them statistically indistinguishable from the results in Table 1. It is difficult to interpret those findings given that they include large numbers of workers observed selectively by criteria that are unknown, and which may correlate with determinants of wages; the results in Table 1 are not vulnerable to that bias.

Figure 1 presents a visualization of the key results in Table 1. Figure 1a compares the unconditional distribution of annual base salary for H-1B workers (blue) to total wage income for US natives (orange), in constant 2023 dollars (corresponding to Table 1, column 1). Figure 1b then shows the visual analog of the key regression (Table 1, column 4): It presents the wage for each worker residualized on all fixed effects, shifted between groups by the coefficient estimate on the H-1B dummy. Again, these comparisons do not adjust for differences in wage concept or worker employer tenure between H-1B workers and natives.[18]

The distribution of these wage measures within geographic, occupation, and demographic cells

[18] That is, I construct $\tilde{y}_i = \mu_0 + \hat{\varepsilon}_i$ for U.S. workers and $\tilde{y}_i = (\mu_0 + \hat{\beta}) + \hat{\varepsilon}_i$ for H-1B workers, where μ_0 is the estimated conditional mean log wage for US natives under a common covariate distribution. The figure plots kernel density estimates of $\tilde{y}_i$ separately by group, so that the horizontal shift between the two distributions equals $\hat{\beta}$, the regression coefficient on H1B_i, while the spread of each distribution reflects the group's residual wage variation around its conditional mean.

Figure 1: Visualizing the Gap between H-1B and US natives: Without accounting for differences in wage concept or employer tenure

(a) *Unconditional on worker fixed effects* (b) *Residualized on worker fixed effects*

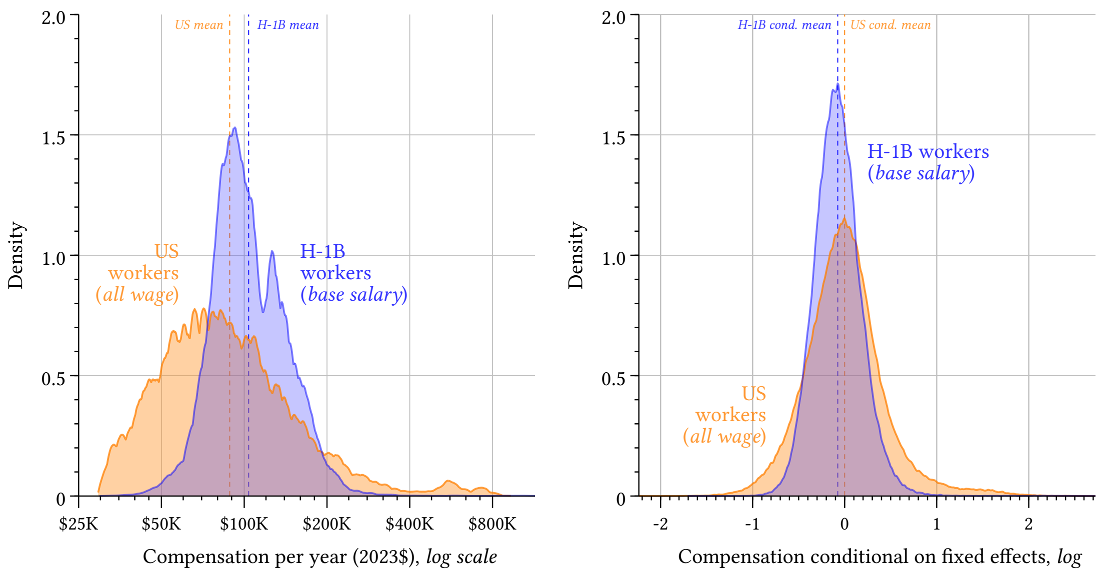


Figure 1a corresponds to Table 1, column 1; Figure 1b corresponds to Table 1, column 4. The sample includes only prime age (21–50), full-time employed workers with a college education or higher, in the years when education is observed for both H-1B and US natives (2021–22). The H-1B wage concept is base salary only; the native wage concept is total wage income, including base salary plus variable pay, including bonuses.

is much wider for US natives than for H-1B workers, in Figure 1b. H-1B workers' base salary is well above what the lowest earning US natives earn in total wage income, consistent with the fact that H-1B workers' salaries are regulated by a wage floor but US natives' total wage income is not. The total wage income of the highest-paid US natives is far above the base salary of relatively well-paid H-1B workers, which I will return to below.

3.2 Accounting for differences in employer tenure

A crucial limitation of the wage comparisons above is that they do not consider differences in employer tenure ("job tenure") between H-1B workers and US natives, the number of years that a worker has been continuously employed at the same firm—not necessarily performing the same job. The average US native worker has been at their employer for several years; initial-employment H-1B workers—those contained in the Bloomberg (2025) sample used here—are by definition low-tenure workers.

It is well established that workers' wages rise with employer tenure, even controlling for age (Altonji and Shakotko 1987; Topel 1991). Wages rise by roughly one percent per year of tenure for workers in general (Altonji and Williams 2005), and by more for the college-educated workers studied here (Deming 2026). The tenure effect arises from a mix of factors, including the accumulation of firm-specific human capital and rising bargaining power (Bagger et al. 2014). Omitting employer tenure from the analysis means comparing natives with years of tenure on average, and commensurately higher wages, to H-1B workers with very low tenure. This tends to artifactually reduce the wages of H-1B workers relative to natives.[19]

3.2.1 *Unobserved gaps in employer tenure between H-1B and native workers*

Employer tenure among H-1B workers: Average employer tenure is very low among the initial-employment H-1B workers studied here: 0.6 years or less on average. I estimate this as follows. 42 percent of the H-1B workers in the sample are observed to have zero tenure at the sponsoring firm because they are arriving in the United States for the first time.[20] The vast majority of the remaining 58 percent are changing status from an F-1 student visa.[21] Of those, 29 percent do so without utilizing Optional Practical Training (OPT), that is, without working in between graduation and starting employment at the H-1B sponsor firm (Clemens et al. 2025, Fig. 3). If workers on OPT never changed employers, this would imply an average tenure of about 0.6 years, an estimate that is conservatively high because changing employers is common for workers on OPT.[22] It would imply median tenure of close to zero when looking at all employers and all OPT

[19]The literature has extensively debated whether the returns to tenure reflect firm-specific human capital accumulation, or other factors such as the quality of the initial firm-worker match (Bagger et al. 2014; Jarosch 2023). That debate is not relevant to the present study. Whether tenure *per se* causes wages to rise or proxies for some other omitted determinant of native workers' employment conditions, it biases in equal measure the estimates that omit tenure.

[20]These have no US visa at the time they petition for an H-1B visa, and their visa undergoes overseas consular processing.

[21]These are typically F-1/OPT workers at the same firm, or a small number of L-1 intracompany transferees (~3 percent). A trivial number hold O-1, TN, or J-1. A very low number could change status from an H-4 spousal visa at the same firm, but most initial-employment H-1B visas for spouses previously on H-4 are for people changing from dependent status or from another employer.

[22]That is, $(1-(0.42+(0.29\times(1-0.42))))\times 1.5 = 0.62$, assuming that OPT workers' tenure is uniformly distributed between 0 and 3 years, which requires that OPT workers never change employers while they are on OPT. Although workers on Optional Practical Training can in principle have a hypothetical maximum of three years of tenure at the

participants.

Employer tenure among US natives: Employer tenure is unobserved in the sample of US native workers used so far, full-time year-round employees in the American Community Survey (ACS). But two separate factors imply that employer tenure in the native worker sample is higher than in the H-1B worker sample. The first is that true tenure in the underlying subpopulation of skilled native workers is several years on average (Deming 2026). Second, and less obviously, restricting the native sample to full-time, year-round employees has the side effect of truncating the large majority of workers with tenure less than one year. The native sample includes only people who worked full-time for at least 50 of the previous 52 weeks.[23] This omits almost all new employees who are labor-force entrants, such as new graduates from full-time degree programs or people returning to work after parenting. It furthermore omits all new employees who experienced a period of unemployment, or simply non-employment, of two weeks or more since their previous job.

Overall, this restriction truncates about 73 percent of the US worker sample with employer tenure less than one year. This is the fraction of all college-educated workers who began as employees at a private-sector firm during 2018–2024 who had either had never worked before, or experienced more than two weeks of nonemployment since their most recent job, disqualifying them as full-time, year-round employees.[24] In other words, a sample of full-time, year-round

sponsoring firm before they begin work, H-1B wage offers are typically set on I-129 forms early in the calendar year for an October start date, that is, roughly 7–9 months before the H-1B start date and thus before the end of OPT.

[23]The sample includes only people who worked full-time for the entire previous year, without interruption. The ACS questionnaire to identify year-round employees reads, "*During the PAST 12 MONTHS (52 weeks), how many WEEKS did this person work?*'; the sample includes only people who answered with 50 weeks or more. The ACS question to identify those who worked full-time for the entire past year reads, "*During the PAST 12 MONTHS (52 weeks), in the WEEKS WORKED, how many hours did this person usually work each WEEK?*"; the sample includes only those who answered with 35 hours or more.

[24]Details of this novel calculation are reported in the Appendix. I estimate the distribution of pre-start nonemployment duration using job-spell data from the Survey of Income and Program Participation (SIPP), pooling the 2018 through 2024 reference years across all three SIPP panels fielded in this period. The annual longitudinal panels of the SIPP began in 2018, before which the survey was quadrennial. For each new employer job start observed among college-educated workers aged 21–50, I measure the gap in weeks between the start and the person's most recent prior employment, linking job spells across calendar years so that, for example, a job ending in December 2021 and a new job beginning in February 2022 yields a measured gap of roughly eight weeks rather than being treated as two unrelated within-year events. Job starts where the person was simultaneously employed at another job are excluded, so that the sample captures genuine transitions into employment rather than the addition of a secondary job. The design accounts for left-censoring (never employed before). I validate the analysis by comparison to summary statistics of the same estimand from full-universe data on formal-sector workers from the Longitudinal Employer-Household Dynamics (LEHD) database.

workers from the ACS is highly unrepresentative of workers with less than one year of tenure, but representative of workers with more than one year of tenure.

3.2.2 *The explanatory power of differences in employer tenure*

Although employer tenure is unobserved for native workers in the ACS data used so far, tenure is observed for a nationally-representative sample of workers in a supplement to the Current Population Survey (CPS). Its semiannual Job Tenure Supplement asks respondents how many years they have worked continuously at their current employer. This allows estimation of what gap in wages for average workers would arise by construction from comparing workers with very low tenure to workers with average tenure, controlling for education, demographic traits including age, geography, and occupation.

In other words, it allows estimation of the bias in the estimates of Table 1 that arises from omitting consideration of the gap in tenure between H-1B and native workers. Table 2 reports this exercise. It uses CPS samples closely paralleling the ACS sample: full-time employed US natives with a college degree or higher, age 21 to 50. Because CPS samples are much smaller than in the ACS, it pools the Job Tenure Supplements from 2018, 2020, 2022, and 2024.[25]

The first column of Table 2 shows a regression of log total wage income on employer tenure, with no other controls but year fixed effects. Wages rise with tenure by 2.2 percent per year, on average across all workers. Column 2 adds dummy variables for single-year age, as well as education and gender: wages still rise just 0.8 percent per year of tenure, implying that the returns to tenure capture wage determinants beyond those determined by overall years of work experience. Column 3 adds controls for metropolitan area and occupation: Within urban labor markets and within occupations, wages rise 1.0 percent per year of tenure.[26] Column 4 restricts the sample to the localities and occupations where H-1B workers concentrate: the 39 out of 680 metropolitan areas containing more than 90 percent of all H-1B workers, and the 83 out of 548

[25]The Job Tenure Supplement is administered in January, thus the 2020 wave was not affected by the outbreak of Covid-19, which began to affect labor markets in March.

[26]Because the CPS sample is much smaller than the ACS, the geography reported in public-use microdata is coarser. While the CPS does not allow mapping of individual responses to PUMAs and thus Commuting Zones, it does indicate the metropolitan area of each respondent, which in practice overlaps heavily with Commuting Zones for the cities where H-1B workers concentrate.

Table 2: Bias from Omitting US natives' More Advanced Tenure at Firms: Native workers in the CPS Job Tenure Supplement

Dep. var.: ln real wage income	(1)	(2)	(3)	(4)
(a) *Linear*				
Tenure at firm	0.0222	0.0081	0.0101	0.0109
	(0.0032)	(0.0022)	(0.0026)	(0.0048)
(b) *Quantile (median)*				
Tenure at firm	0.0224	0.0083	0.0103	0.0108
	(0.0026)	(0.0020)	(0.0017)	(0.0036)
Observations	6,964	6,964	6,964	1,551
(c) *Implied wage gaps explained by employer tenure gap*				
Mean tenure, US (*ACS comparable*)	6.96	6.96	6.96	6.42
Mean tenure, H-1B initial employment	0.6	0.6	0.6	0.6
Wage gap explained by tenure gap	0.1416	0.0517	0.0644	0.0633
	(0.0206)	(0.0141)	(0.0165)	(0.0281)
Restrict sample?				
H-1B intensive localities/occupations	—	—	—	Yes
Fixed effects:				
Year	Yes	Yes	Yes	Yes
Age, education, gender	—	Yes	Yes	Yes
Geography (metropolitan area)	—	—	Yes	Yes
Occupation (4 digit)	—	—	Yes	Yes
Standard errors:				
Clustered: geography (metro)	Yes	Yes	Yes	Yes
Clustered: occupation	Yes	Yes	Yes	Yes

US natives in the Current Population Survey, January Job Tenure Supplement, weighted with the census sampling weight specific to job-tenure respondents, years 2018, 2020 (pre-*Covid*), 2022, 2024. 'H-1B localities' are the 39 out of 680 metropolitan areas comprising the worksites of ⩾90 percent of all H-1B workers; 'H-1B occupations' are the 83 out of 548 four-digit census occupation codes comprising ⩾90 percent of all H-1B workers. Sample includes only workers employed, full-time and year-round, age 21–50 with a college degree or above. Coefficient estimates consider workers with less than 15 years of employer tenure. For comparability with the American Community Survey data used earlier, where workers with tenure < 1 year are omitted by the requirement that respondents be employed for the entire previous year, the average tenure reported here is conditional on tenure $\geqslant 1$. Local labor markets are defined as metropolitan areas. Age fixed effects are by individual year; education fixed effects are for the four categories of bachelor's, master's, professional degree, or doctorate. Standard errors in parentheses; standard errors for the quantile regression robustness check (not used in part (c)) are clustered by occupation only. Annual wage is estimated as 52 times weekly wage earnings in the previous week. All regressions include suppressed constant term.

occupations containing more than 90 percent of all H-1B workers (2021–2024). The coefficient of interest rises further to 1.1 percent per year of tenure, implying that the relevant localities and occupations exhibit tenure-wage dynamics that are at least as important as those in the broader US labor market. In all columns, results from the linear specification (panel a) closely match those in the quantile (median) regression (panel b), indicating that topcoding of wages does not meaningfully affect the results.

Panel (c) of Table 2 then estimates the magnitude of wage gaps that could be explained by given gaps in employer tenure. The first row reports average tenure in the CPS sample comparably to the ACS sample used earlier. Because the ACS sample truncates the large majority of workers with tenure under one year—as discussed above—inclusion of workers with tenure under one year would bias average tenure in the CPS sample downward relative to the workers in the ACS sample. This row therefore reports average tenure of workers conditional on having one or more years of tenure. The next row contains the above estimate of average tenure among the initial-employment H-1B workers studied here, primarily accounting for the fact that a limited fraction of them have some employer tenure at the same employer as Optional Practical Training workers. The next row reports the product of the regression coefficient of interest, the partial tenure-wage relationship, and the difference between average ACS-compatible employer tenure among US workers and average tenure among these H-1B workers. This product is the percent difference in wages that would arise mechanically from comparing US native workers of average tenure with other workers of much lower tenure (such as H-1B workers), controlling for education, demographics, geography, and occupation.

The results reveal an important bias in the estimates of Table 1. That prior analysis compares native workers with about six and a half years of tenure, on average, to H-1B workers with very low tenure. Confounding by unobserved tenure differences explains almost the entire wage gap in the last column of Table 1. In the linear specification, Table 1 reports that H-1B workers earn 7.48 percent lower wages than comparable natives, without adjusting for the tenure gap. Table 2 indicates a wage gap of 6.33 percent could arise simply by comparing US workers who have the same low tenure as H-1B workers to US workers in the ACS sample with average tenure.

Accounting for differences in employer tenure, then, the gap between the wages of H-1B workers and comparable natives is just −1.2 percent. The standard errors in Table 1, column 4 (0.0352) would not allow such a gap to be reliably distinguished from zero ($p = 0.74$), even in that sample of over 600,000 workers.[27]

3.2.3 *Comparing directly to native workers in the CPS*

It is not feasible to exactly replicate the comparison of H-1B workers to natives in the ACS (with tenure unobserved), the exercise in Table 1, as a comparison to natives in the CPS (with tenure observed). This is because the the ACS and CPS differ in other relevant aspects: 1) the CPS microdata do not identify respondents' PUMA (nor, thus, commuting zone), because 2) the January CPS samples are about one sixtieth the size of the annual ACS samples, and 3) the Job Tenure Supplement is semiannual and thus not available in 2021 or 2023. 4) The sample size is not large enough to include occupation fixed effects at the four-digit census code level. Finally, 5) the January CPS reports weekly wage income only, which does not permit restricting the sample to year-round workers—though this last point has the offsetting advantage of avoiding the problem of truncating low-tenure workers in the ACS, discussed above.

It is nonetheless informative to carry out the exercise of comparing H-1B workers directly to natives in the CPS, bearing in mind those important caveats, as a check on the result in Table 2. That test can control for respondents' metropolitan area, which for the cities where most H-1B workers congregate, strongly overlaps with commuting zones. It can control for occupation fixed effects at the three digit level, which still sharply identify the occupations where H-1B workers concentrate.[28] The problems of sample size and of nonoverlapping years can be ameliorated by comparing the last three pooled Job Tenure Supplements (2020, 2022, and 2024) to H-1B workers from the years 2022 and 2024 only.[29] Only H-1B workers with observed education values are

[27] That is, $-0.0748 - (-0.0633) = -0.0115$. This calculation conservatively uses the linear regression specification. In the quantile specification, Table 1 reports that the median wage of H-1B workers is 6.8 percent below that of comparable natives, without adjusting for the tenure gap. But Table 2 indicates that US workers with the same low tenure as H-1B workers would have median wages 6.33 percentage points below the wages of workers with average tenure. Because the linear and quantile regression results are so similar, the *wage gap explained* estimates are identical to three significant figures whether the coefficient from the linear or quantile regression is used. In other words, correcting for unobserved tenure gaps using the quantile specification would imply that H-1B workers' wages are just 0.1 percent below the wages of comparable natives' wages, again to a degree that cannot be reliably distinguished from zero.

[28] 66.0 percent of H-1B worker observations fall into the *two*-digit occupation category 10 or 11, which contains: Computer and information research scientists; Computer systems analysts; Information security analysts; Computer programmers; Software developers; Software quality assurance analysts and testers; Web developers; Web and digital interface designers; Computer support specialists; Database administrators and architects; Network and computer systems administrators; Computer network architects; Computer occupations, all other.

[29] Note that the 2020 CPS Job Tenure Supplement was fielded in January 2020, before the Covid-19 pandemic began

used for 2024, without imputation of missing values.

Table 3 presents the results, with the specifications corresponding as closely as possible to earlier the ACS-based regressions (Table 1, column 4). In column 1, without adjusting for differences in employer tenure, H-1B workers earn 3.8 percent less than native workers controlling for single-year age, gender, education, metropolitan area (CBSA), and three-digit occupation fixed effects. This finding of a statistically significant wage gap of about −4 percent broadly but not precisely replicates the analogous ACS-based finding of −7 percent (from Table 1, column 4).[30]

The second column of Table 3 presents the exercise of directly controlling for employer tenure in the closest analog to the ACS-based specifications that is feasible within the limitations of the CPS data. This column presents results imputing to each H-1B worker the true median tenure of zero. Controlling for employer tenure, the wage gap of −3.8 percent from column 1 falls to −0.9 percent in column 2, and is no longer statistically distinguishable from zero ($p = 0.64$). Column 3 more conservatively imputes to each H-1B worker the tenure of 0.6 years, the mean estimated earlier. The wage gap for H-1B workers unexplained by tenure rises by only 0.4 percentage points in absolute value, and remains indistinguishable from zero. These patterns in the linear specification (panel (a)) hold *a fortiori* in the quantile specification (panel (b), median): In column 2, with H-1B tenure conservatively imputed at the true median of zero, the conditional median wage of H-1B workers is just 0.8 percent below the median wage of natives, a difference that cannot be statistically distinguished from zero ($p = 0.68$). These point estimates closely match the finding from subsubsection 3.2.2 that controlling for unobserved employer tenure would reduce the H-1B wage gap in Table 1, column 4 to just −1.2 percent and statistically indistinguishable from zero.

Together, Table 2 and Table 3 imply that estimates of wage gaps between H-1B workers and natives are severely biased when they do not account for differences in employer tenure between the two groups, even when controlling for age, education, geography, and occupation. Both

to shock the labor market.

[30]One reason for the imperfect replication could be that, as mentioned above, the CPS sample does not truncate low-tenure native workers as does the ACS subsample of full-time, year-round native workers. This would tend to lower the average native wage in the CPS relative to the ACS subsample, all else equal, reducing the absolute value of the wage gap between H-1B workers and natives. The remainder of the gap could arise from slightly different years, geographies, and occupation codes, and sampling error.

Table 3: Suggestive Replication of H-1B Wage Gap Analysis Controlling for Tenure at Firm

Dep. var.: ln real wage income	(1)	(2)	(3)
	Omit tenure	Include tenure	
Sample and assumptions:			
US native data	Jan. CPS	Jan. CPS	Jan. CPS
H-1B worker tenure	—	≡0 years	≡0.6 years
(a) *Linear*			
H-1B worker	−0.0378	−0.0094	−0.0138
	(0.0189)	(0.0198)	(0.0195)
Tenure at firm		0.0073	0.0073
		(0.0013)	(0.0013)
Observations	143,356	143,356	143,356
US natives (CPS)	5,818	5,818	5,818
H-1B workers	137,538	137,538	137,538
(b) *Quantile (median)*			
H-1B worker	−0.0372	−0.0083	−0.0127
	(0.0189)	(0.0197)	(0.0195)
Tenure at firm		0.0073	0.0073
		(0.0012)	(0.0012)
Observations	143,495	143,495	143,495
US natives (CPS)	5,878	5,878	5,878
H-1B workers	137,617	137,617	137,617
Fixed effects:			
Year	Yes	Yes	Yes
Geography: metropolitan area	Yes	Yes	Yes
Age, education, gender	Yes	Yes	Yes
Occupation (3 digit)	Yes	Yes	Yes

In this table, US natives' traits are estimated not from the annual American Community Survey, but from the Current Population Survey's semiannual January Job Tenure Supplement. To maximize temporal overlap with the H-1B sample, only years 2020 (pre-Covid), 2022, and 2024 are used. Sample includes only workers employed full-time, age 21–50 with a college degree or above. Local labor markets are defined as metropolitan areas (CBSA). Age fixed effects are by individual year; education fixed effects are for the four categories of bachelor's, master's, professional degree, or doctorate. US natives weighted with the census sampling weight specific to the Job Tenure Supplement, H-1B workers weighted by unity (full universe). US workers' annual wages are estimated by multiplying their average weekly wage by 52. All regressions include suppressed constant term. Robust standard errors in parentheses: The reduced sample size does not allow clustering of standard errors by geography and occupation, so standard errors in this table should be regarded as conservatively low.

tables imply that controlling for the unobserved tenure gap in Table 1 would yield estimates of the gap between H-1B workers' base salary and comparable natives' total wage compensation that are statistically indistinguishable from zero. This is consistent with earlier estimates by Mithas and Lucas (2010), using data on H-1B workers from the early 2000s, that differences in employer tenure are large enough to explain the wage gap between H-1B workers and US workers in information technology.[31]

3.3 Accounting for differences in wage concept: base pay vs. variable pay

A further, separate challenge complicates interpretation of the wage gaps in Table 1. The wage concept differs for H-1B and US natives, in the underlying data. The wage concept for native workers includes types of wage income that are not counted for H-1B workers. In other words, the measure of the 'wage' for H-1B workers is systematically lower than the 'wage' for the native workers they are compared to, by construction.

As discussed above, the contractual wage that employers report for H-1B workers is guaranteed cash income delivered each pay period: the base salary. By law, H-1B workers may not hold any employment other than their principal job. US native wage income, in contrast, comes from the American Community Survey (2021–2024), which asks respondents to report "*wages, salary, commissions, bonuses, or tips from all jobs*" (the variable `incwage` in Ruggles et al. 2025). The US-worker wage concept in the census data thus includes 1) bonuses, and 2) income from secondary jobs, while H-1B workers' wage income does not.

These additional elements of total wage income are typically, accurately reported by workers responding to census surveys. This has been verified by audit studies that link census income responses to administrative data on W-2 tax records, which include bonuses and include all jobs held. Bee and Mitchell (2017a, 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

[31] Mithas and Lucas (2010, 756) find that the returns to firm-specific experience for IT professionals is 2.4 percent per year, controlling for demographic traits, education levels, and firm traits such as sector and size that would tend to vary across geographic areas. This estimate does not vary depending on the inclusion of a dummy variable identifying H-1B workers. In other words, their finding of a conditional H-1B wage premium of 6.6 percent within occupational categories (IT professionals) and 3.7 percent within narrow job titles controls for employer tenure. It implies that comparing US natives with even a few years of employer tenure to new-hire H-1B workers would misleadingly result in an estimate of a conditional wage penalty for H-1B workers.

survey forms. Important discrepancies exist for retirees and for non-wage income, especially transfers and pensions, that are not relevant to the analysis here.

No information in the ACS allows these incommensurable elements to be stripped away, isolating the base salary of native workers in their principal job, for proper comparison with the base salary of H-1B workers. But it is possible to approximate the magnitude of the discrepancy as a fraction of native workers' base salary.

3.3.1 *Bonus payments*

The information and financial services sectors, where H-1B workers concentrate, pay the highest shares of total wage income as bonuses among all sectors in private industry (BLS 2020, 2024). In nationally representative data from the National Compensation Survey, the Bureau of Labor Statistics estimates average bonus payments relative to base pay for native workers in these sectors. For non-managerial professional workers in the information and financial activities sectors, bonuses are 8–9 percent of the magnitude of base salary.[32] This represents average bonuses actually paid, rather than targets. But it is corroborated by industry surveys on target bonuses. The median annual *target* cash performance bonus for low-level professionals in major US private sector firms is ten percent of base pay (WorldatWork 2025, 13, 35).[33] Both of these estimates rightly exclude high-level officers as well as low-level office and administrative support staff.

It is nevertheless possible that these broad estimates of bonus payments are weighted toward more client-facing roles, while the roles filled by native workers comparable to H-1B workers

[32] The Bureau of Labor Statistics publishes a decomposition, by industry and worker type, of Employer Costs for Employee Compensation (ECOC). Bonus payments as a share of total compensation, and base pay as a share of total compensation, are published separately so that irrelevant components of total compensation (especially benefits) may be excluded. This allows estimation of the ratio of bonus income to base salary for the relevant industries, while appropriately omitting high-level managers/officers as well as omitting office and administrative support staff. For non-management professionals in the information industry, bonus income is 7.96 percent of base salary (= 5.2/65.2). For non-management professionals in the financial activities industry, this ratio is 8.84 percent (= 5.7/64.5). BLS, "Employer Costs of Employee Compensation", Table 4: Employer Costs for Employee Compensation for private industry workers by occupational and industry group September 2025, accessed February 20, 2026.

[33] The WorldatWork figure is reported for a sample of 151 firms by a network of human resources professionals at large publicly-traded and private companies, many of them multinational firms in the tech, finance, and professional services industries. This figure appropriately excludes upper-level professionals such as managers, supervisors, officers, and executives, and excludes long-term incentive payments like Restricted Stock Units.

are more computer-intensive and subject to different incentive pay in practice. For this reason I assemble data on typical bonuses actually paid to native workers in a single occupation—software engineer—at 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.[34]

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. The dataset has been the basis of peer-reviewed studies in *Econometrica* and the *Review of Economic Studies* (Cullen and Pakzad-Hurson 2023; Cullen et al. 2026). Each compensation data submission records base salary, equity value, bonuses, job title, career level, and employer.

Table 4 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—somewhat below the nationally representative 8–9 percent from BLS, because 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.

3.3.2 *Payments in stock*

The ACS wage income question asks respondents to report all income received from an employer, including tips, commissions, and bonuses, with no exclusion for equity-sourced pay. When payments-in-stock are received, and restricted stock units vest or nonqualified stock options are exercised, the resulting gain appears on the employee's W-2 in Box 1 and is subject to ordinary income tax and FICA withholding—the same channels through which base salary and

[34]Because I am interested in bonuses paid to comparable US native workers reporting total wage income on the ACS, I exclude the firms whose employees are foreign nationals in their vast majority. These include Infosys, Tata Consultancy Services, and HCS 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 natural-born US citizens, so including them in the analysis here would be incorrect.

Table 4: 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.

cash bonuses reach the employee (Bureau of Economic Analysis 2023). Vested stock compensation should therefore be captured by the standard survey instrument, under accurate reporting.

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 excluded 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 repeatedly verified (Bollinger et al. 2019, 2025; Rothbaum 2015), 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 2017b; Bee et al. 2023).

A conservative conclusion from this literature is that the ACS-reported total wage income for US workers in Table 4 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 seven percent. Assuming that just one third of that W-2 reported wage income is reported on census survey forms (2.3 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 average firm in the table by an amount exceeding 8.3 percent, 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 six percent higher than their base salary.

3.3.3 *Wage income from second jobs*

The ACS reports wage income from “all jobs,” while H-1B workers are legally restricted to a single employer. Bailey and Spletzer (2021) find that 2.16 percent of the average American worker’s quarterly wage income arises from second jobs.[35] And the tendency to receive income from second jobs is higher among workers with a college education than among non-college workers: 51 percent of workers who hold more than one job are workers with a college education, even though workers with college only comprise 45 percent of employed workers overall.[36]

[35]That is, they estimate that 7.8 percent of wage-earners get income from second jobs, and for those workers, the second job provides 27.8 percent of total wage income. For prime-age workers, the secondary job typically supplements a full-time job rather than another part-time job; the samples considered in this paper include only prime-age workers.

[36]The fraction of multiple job holders who are college graduates is reported by Birinci and Garriga (2025) as of December 2024. The fraction of employed US workers who are college graduates is reported by the Bureau of Labor Statistics, “Employment Situation News Release USDL-26-0169”, Table A-4, as of January 2025.

3.3.4 *The implied bound on the wage gap between H-1B and native workers*

In sum, H-1B workers' base salary is lower than the total wage income of comparable US workers in the ACS data by construction: because the ACS wage income concept includes variable pay and second-job income that is not included in the base salary. This creates an important bias in any estimates of wage gaps between H-1B workers and native workers using these data sources. The exact magnitude of this bias cannot be estimated, but it can be conservatively bounded. Total wage income reported on the ACS, for native workers in jobs comparable to those of H-1B workers, includes cash bonus income that exceeds base salary by about six percent on average, and includes second-job income that conservatively exceeds base salary in the principal job by over 1 percent. To yield an extremely conservative estimate, I assume that *none* of the W-2 payments-in-equity received by comparable native workers are included when they report their annual income to the census, even though the research literature finds that a substantial portion of equity income is reported.

These conservative assumptions imply that comparable native workers' total wage income in the ACS exceeds H-1B workers' base salary by an amount greater than 7 percent, simply because the wage concepts in the two data sources measure different quantities. Failing to account for this difference in wage concepts would tend to create the appearance of a wage gap between H-1B workers and comparable native workers that is substantially more negative than a comparison of commensurable wages would imply. It would be large enough to make H-1B wages appear several percentage points lower than those of comparable native workers even when they are higher. Borjas (2026) does not mention or account for these differences.

This analysis does not allow a precise estimate of the wage gap between H-1B workers and comparable native workers. But it does allow a confident lower bound. Without adjusting for differences in employer tenure or wage concept, the average base salary of H-1B workers is 7.48 percent less than the average total wage income of US workers of the same education, age, and gender, working in the same occupation in the same local labor market or city (Table 1, col. 4). 6.33 percentage points of that gap is accounted for simply by the relatively low tenure of H-1B workers relative to US native workers, even controlling for age (Table 2). This implies that H-1B workers' average base salary is just 1.15 percent lower than the total wage income of native workers with the same education, age, and gender, performing the same occupation in the same local labor market (city), *with the same employer tenure.* This wage gap is well below the gap that can be statistically distinguished from zero.

But even this wage gap compares two fundamentally different wage concepts: the guaranteed base salary for the sole job of H-1B workers, and the total wage income for natives, including base salary, bonuses, payments-in-equity, and income from second jobs. The gap between base salary and total wage income rises above 7 percent of base salary for comparable native workers. That is the size of the wage gap between H-1B and native workers that would arise artifactually from comparing workers of *identical* compensation simply by omitting the components of total wage income beyond base salary for H-1B workers, but including them for native workers. While it is not possible to precisely estimate the gap between commensurable wages for H-1B workers and comparable native workers using these data sources, it is possible to reliably rule out an average base salary for H-1B workers that is lower than the commensurable base salary for comparable native workers. An analysis finding that the wages of average H-1B workers fall below those of average native workers is inconsistent with the evidence when using the same wage concept for both groups, and controlling for differences in education, age, gender, occupation, geography, and employer tenure. The evidence is exclusively consistent with a small wage premium for H-1B workers on average. H-1B workers' wage premium is likely to be around 6 percent or more.

This is consistent with the finding of Mithas and Lucas (2010, 756–760), using data from the years 2000–2005, that H-1B workers' compensation exceeds that of comparable US citizens 1) using the same wage concept (base plus bonus) and 2) controlling for employer tenure. They find that H-1B wages exceed comparable US workers' wages by 6.6 percent with the same education, gender, and experience, within the same occupation category (IT professionals). This conditional H-1B premium for total wage income is 3.7 percent within narrow job titles.[37] Their finding that H-1B wages are higher than those of comparable natives moreover implies that they would have spuriously concluded the opposite, if their analysis had counted bonus income for US workers

[37]The partial coefficient estimate on the H-1B dummy in Mithas and Lucas (2010, Table 8, col. 3) is 0.026 while the coefficient on the interaction term with the *capreached* dummy (whose mean is 0.27 for H-1B workers in Tab. 2) is 0.039, thus $0.026 + (0.039 \times 0.27) = 0.037$. The analysis by Mithas and Lucas does not control for geographic location of the worksite, but does control for a variety of firm characteristics (size, sector, and financing) that would serve to control for some important differences in employers across space.

but not H-1B workers, and had ignored the large gap in employer tenure between the two worker types.

The overall finding of a modest wage premium for H-1B workers, approximately six percent, is subject to uncertainty. It arises from combining estimates from three different data sources (the original H-1B and ACS data; the CPS Job Tenure Supplement; and separate data on variable pay). Each introduces uncertainty, but each is conservative. It implies that for any H-1B wage penalty to exist, either the regression estimate of Table 1 would need to be off by six percentage points (which falls outside the confidence interval); or the wage premium for average-tenure natives relative to low-tenture H-1B workers would need to be off by six percentage points (the entire value of the correction, tantamount to assuming that the well-documented tenure premium for natives does not exist); or that ACS total wage income exclude all variable pay such as bonuses (which is contradicted by audit studies). The overall finding of a modest wage premium in the neighborhood of six percent should be taken as a point estimate with substantial uncertainty, but the evidence does not admit a wage penalty of any magnitude.

4 Reconciling Differences with Prior Estimates

Borjas (2026) finds, "The average H-1B worker earns about 16 percent less than an American worker with the same education, age, gender, occupation, and who works in the same locality," which he defines as "comparable natives". This finding is striking in light of the markedly different results above, because it uses the same original dataset on H-1B workers (Bloomberg 2025), and the same ACS survey source for native workers (Ruggles et al. 2025). Below I reconcile the two estimates, first by successfully replicating the Borjas result and then by tracing the causes of the discordant results.

4.1 Replication

I begin by building a dataset that stacks individual H-1B workers (2021–2024) with individual native workers from the ACS (2021–2024). Summary statistics for this dataset are in Table 5, with native workers on the left, H-1B workers on the right.

Table 5: Summary Statistics: Means in analysis sample and replication dataset, using original wage concepts

	US natives			H-1B Workers		
	(1) All years	(2) 2023 only	(3) Borjas	(4) Complete records only	(5) Education imputed	(6) Borjas
Sample size	980,261	252,686	252,664	284,704	343,252	343,608
Annual wage (000s)	\$100.9	\$103.0	\$103.1	\$101.3	\$102.0	\$101.1
Log annual salary	11.32	11.34	11.34	11.47	11.48	11.47
Age	36.3	36.2	36.2	32.0	31.9	31.9
Male (%)	48.8	48.5	48.6	67.9	67.2	67.2
Degree (%):						
Bachelor's	64.2	64.3	64.3	42.4	52.2	52.3
Master's	26.9	26.8	26.8	50.4	41.8	41.8
Professional	5.2	5.3	5.3	1.1	0.9	0.9
Doctorate	3.7	3.6	3.6	6.1	5.0	5.0
Percent of workers with imputed education level:						
2021	0.0	—	*	0.0	0.0	*
2022	0.0	—	*	0.0	0.0	*
2023	0.0	0.0	*	0.0	38.2	*
2024	0.0	—	*	0.0	31.9	*

* = Unreported by source. In column 5, all workers with unobserved education receive the imputed education level of "bachelor's". Sample includes only workers employed, full-time and year-round, age 21–50 with a college degree or above. US natives weighted with the census sampling weight, H-1B workers weighted by unity (full universe). Data on US natives from single-year American Community Survey public-use microdata files (Ruggles et al. 2025); data on H-1B workers from USCIS via (Bloomberg 2025).

There are 980,261 native workers in this ACS subsample of college-educated, full-time, year-round employed workers age 21–50, with extreme wage values trimmed (col. 1). When the full sample 2021-2024 is truncated to include only the year 2023, as Borjas (2026) does, the sample falls to 252,686 native workers (col. 2). My replicated sample exhibits a near-perfect match with the summary statistics from Borjas (2026) in col. 3: the sample sizes differ by only 0.009 percent. Average wages match to three significant figures. The demographic and education composition of the replication sample differ from the original by 0.1 or less. These are substantively identical samples.

The remaining columns of Table 5 show samples of H-1B workers. Col. 4 shows the data on 284,704 H-1B workers in its original form, without imputation of education levels for the large numbers of workers in 2023 and 2024 for whom education is unobserved. The next column shows the same summary statistics when the sample is greatly expanded by imputing the education level of "bachelor's" to all H-1B workers with a missing education value.

This mass-imputation of missing education values gives a near-perfect match to the summary statistics in the Borjas (2026) sample (col. 6). The sample sizes differ between columns 5 and 6 by just 0.1 percent. The education composition of the sample resulting from this mass imputation matches the education composition of the Borjas dataset exactly, to all the decimal places reported by Borjas (2026). This indicates the Borjas analyzed the H-1B data after assuming that 38 percent of the H-1B workers in 2023 and 32 percent in 2024, whose level of education is unobserved in the raw data, had the highest degree of bachelor's. No other plausible treatment of the large number of observations in the original H-1B data is consistent with the educational composition of the H-1B sample reported by Borjas.

This mass-imputation stands out for three reasons. First, it is a major alteration of the underlying data that is not disclosed by Borjas (2026). Second, the altered characteristic is not ignorable with respect to wages, the outcome of central interest, but is a key determinant of wages. Third, the mass imputation results in large numbers of impossible observations. For example, 38.7 percent of the workers that Borjas imputes as having a bachelor's degree came through the "M" lottery, which requires a master's degree or above.

Unfortunately, there is no reliable way to fill in the missing values. As discussed above, no observed traits of the same workers allow confident imputation of this key unobserved trait. Workers coming via the basic ("B") lottery can have any degree level; workers coming via the master's ("M") lottery can have a master's, professional degree, or doctorate. And it is not feasible to establish bounds on wage gaps by, say, assuming all missing education levels are alternatively master's degrees, or any other level: missingness could be correlated with both education level and wages, thus any such blanket imputation would generate bias of unknown sign and magnitude. An alternative approach of simply dropping individual observations with missing education has the potential to generate important bias for the same reason. The straightforward and transparent way to address this major flaw in the H-1B data is simply to rely on the two years of data during which education is observed for essentially all workers (2021–2022), as the analysis in Table 1 did above.[38]

4.2 Reconciliation of discordant results

Having established the close correspondence between the replication dataset and the data used by Borjas (2026) in Table 5, I proceed to decompose the large gap between the results in Borjas and the results above (in subsubsection 3.3.4).

Table 6 begins, in column 1, by transcribing the core result reported by Borjas (2026, Table 2, panel A, col. 4). Column 2 reports the results of running the same regressions on the replication dataset (Table 5, cols. 2 and 5). Broadly, I successfully replicate the paper's core finding that "H-1B workers earn 16 percent less than comparable natives". More precisely, H-1B workers in the replication dataset have a base salary 0.155 log points below the total wage income of comparable natives, while Borjas found a gap of 0.161. A likely reason for the slight difference is Borjas's method of converting H-1B nominal salaries in the four years to 2023 dollars, the details of which are not given in the paper. (That method will become irrelevant in the next step.)

The next column (3) of Table 6 shows the results of an identical regression when all four years of ACS data on US workers are used, instead of Borjas's choice of using a single comparison year. This allows the inclusion of year fixed effects, to account for not only changes in the price level but any other nationwide economic shocks affecting all prime-age college educated workers. This step alone causes the wage gap to fall by 2.98 percentage points in absolute value. In other words, three percentage points of the wage gap found by Borjas arises only from the unusual

[38]The rest of the summary statistics in my replication sample with mass imputation of education match the Borjas sample almost exactly, with the exception of average annual wages, which differ modestly: by 0.9 percent between columns 5 and 6. One possible explanation for the difference in wages this is that the average H-1B wage reported by Borjas (2026) is the average in the useable sample before he imputed education levels, given that the average H-1B wage in Borjas (col. 6) is very close to the average wage in col. 4, for observations without imputed wages. Another plausible possibility is that the difference arises from how Borjas converted the raw H-1B wages from 2021–2024 into 2023 dollars. The paper does not mention which price index he used. This matters for Borjas's unique research design of comparing H-1B wages in four different years to ACS wages in a single year, but the nominal/real wage distinction does not affect my corrected research design that simply compares H-1B and native workers in all four years. The corrected design allows inclusion of year fixed effects to account for any changes in the price level over time, rendering conversion to constant dollars unnecessary.

Table 6: Reconciliation of Discordant Results: Stepwise trace from replication of Borjas (2026) to corrected wage gap, without considering unobserved differences in wage concept or employer tenure

Dep. var.: ln real wage	(1) Borjas (2026)	(2) Replication	(3) All years 2021–2024	(4) Standard geography	(5) Education observed, 2021–2022	(6) Std. geog., education observed
Sample and specification:						
Matched years?	—	—	✓	✓	✓	✓
Standard geography?	—	—	—	✓	—	✓
Education observed?	—	—	—	—	✓	✓
(a) *Linear*						
H-1B worker	−0.161	−0.1548	−0.1250	−0.0849	−0.1195	−0.0748
	(0.004)	(0.0045)	(0.0425)	(0.0424)	(0.0374)	(0.0352)
(b) *Quantile (median)*						
H-1B worker	−0.156	−0.1501	−0.1196	−0.0794	−0.1130	−0.0682
	(0.004)	(0.0065)	(0.0420)	(0.0420)	(0.0367)	(0.0348)
Observations	596,271	589,213	1,316,788	1,316,747	640,667	640,641
Fixed effects:						
Year	—	—	Yes	Yes	Yes	Yes
Neighborhood/PUMA	Yes	Yes	Yes	—	—	—
Commuting zone	—	—	—	Yes	Yes	Yes
Age, education, gender	Yes	Yes	Yes	Yes	Yes	Yes
Occupation (4 digit)	Yes	Yes	Yes	Yes	Yes	Yes
Standard errors:						
Robust	Yes	Yes	—	—	—	—
Clustered: geography	—	—	Yes	Yes	Yes	Yes
Clustered: occupation	—	—	Yes	Yes	Yes	Yes

'Standard geography' means that local labor markets are defined as commuting zones (Autor et al. 2019); otherwise defined as neighborhoods or Public-Use Microdata Areas (PUMA), geographic areas defined by the US census to contain no more than 100,000 residents. Education is unobserved for large numbers of H-1B workers in 2023 and 2024 (Table 5), thus 'education observed' implies a sample restricted to the complete-data years of 2021 and 2022. Sample includes only workers employed, full-time and year-round, age 21–50 with a college degree or above. Age fixed effects are by individual year; education fixed effects are for the four categories of bachelor's, master's, professional degree, or doctorate. Standard error two-way clustering for quantile regressions uses the method of Cameron et al. (2011). US natives weighted with the census sampling weight; H-1B workers weighted by unity (full universe).

choice of comparing H-1B workers in four different years with US workers in a single, selected year. Columns (3) and onward furthermore report standard errors clustered by geography and occupation, which is necessary in this setting, as discussed above in subsection 3.1.

The next column changes the measure of geography. Borjas defines local labor markets as the Census Bureau's Public Use Microdata Areas (PUMAs), small divisions of cities that are similar to neighborhoods. In the two most important locations of H-1B workers, California's Bay Area and the New York City metropolitan area, these are small areas indeed. The Bay Area is divided into 40 PUMAs; the New York metro area is divided into 55 PUMAs. This is not an innocuous choice. Column (4) replaces PUMAs with commuting zones, the standard geographic definition of local labor markets in labor economics. The H-1B wage gap falls by an additional 4.01 percentage points in absolute value.

The final two columns of Table 6 restrict the analysis to the years in which H-1B workers' education levels are observed. This shows the effects of Borjas's undisclosed mass-imputation of missing education levels. When local labor markets are defined as PUMAs (columns 3 and 5), analyzing the years with nonmissing education levels reduces the H-1B wage gap by 0.6 percentage points. When local labor markets are defined as commuting zones (columns 4 and 6), analyzing the years with nonmissing education levels reduces the H-1B wage gap by 1.0 percentage points.

In sum, almost the entire difference between the 16.1 percent wage gap in Borjas (2026) and the 7.48 percent gap in Table 1 arises from three sources: 1) Borjas imputes the education level of "bachelor's" to about one third of the H-1B workers in 2023 and 2024 whose education is missing, without disclosing or justifying this, without testing for sensitivity to that assumption, and without comment on the fact that a large share of the imputed education levels are impossible given other traits of the same workers. 2) Borjas does not compare H-1B workers to US workers in the same years, but instead compares H-1B workers in four different years to US workers in a single selected year, without explanation. 3) Borjas defines local labor markets as PUMAs—essentially neighborhoods—rather than commuting zones.

The first two of these choices are difficult to justify on methodological grounds. There is no reason to compare H-1B workers in four different years to US workers in a single year. And mass imputation of missing data—especially when a large share of the imputed values are impossible, and when the imputation materially affects the results—would typically require disclosure and sensitivity analysis.

The last of these three choices, however, might seem less obviously erroneous: Is there a clear reason to define local labor markets as commuting zones rather than narrow neighborhoods or PUMAs?

4.3 Defining local labor markets

The choice to define local labor markets as narrow PUMAs is highly unusual. I am not aware of, and could not identify, any other research paper in the field of labor economics other than Borjas (2026) that defines local labor markets as PUMAs, or any other similarly small geographic segment. The research literature consistently posits much broader labor markets within which immigrants and natives compete: cities (commuting zones or metropolitan areas/CBSAs), states, or nationwide skill cells (education × experience groups). US law is also inconsistent with Borjas's unique definition of labor markets: the law explicitly requires H-1B workers' wages to be compared to those of US workers living in much larger commuting zones that include the worksite, not the narrow neighborhoods immediately surrounding those worksites.[39]

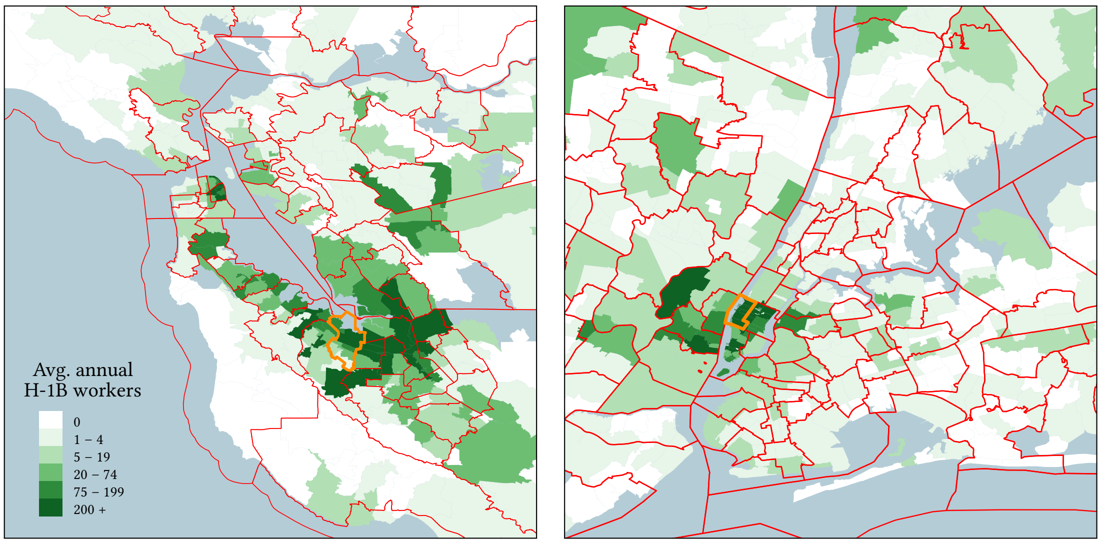
The reason for this research convention and legal requirement is that PUMAs are not plausibly separate labor markets, and that assuming they are separate labor markets can create important bias. This is apparent in Figure 2. Panel (a) shows the San Francisco Bay Area, the metropolitan area with the largest share of H-1B worksites. The boundaries of PUMAs are shown with red lines; many are 1–3 miles across.[40] The green areas are ZIP Code Tabulation Areas, shaded in proportion to the number of H-1B employees whose worksite lies in that area. The Bay Area

[39]The law requires employers to offer "H-1B nonimmigrant wages that are at least (i) the actual wage level paid by the employer to all other individuals with similar experience and qualifications for the specific employment in question, or (ii) the prevailing wage level for the occupational classification in the area of employment, whichever is greater," and defines the area of employment as "the area within normal commuting distance of the worksite or physical location where the work of the H-1B nonimmigrant is or will be performed" (Sections 212(n)(1) and (n)(4) of the Immigration and Nationality Act).

[40]The resident population of a PUMA is around 150,000 by design, and never less than 100,000. The divisions are set to provide users of public-use microdata with some measure of geographic granularity while preventing the use of census microdata to identify individuals.

Figure 2: Public Use Microdata Areas (red) vs. H-1B Worksites (green): Comparing workers only within very small divisions of metropolitan labor markets introduces bias

(a) *San Francisco Bay Area* (b) *New York City metropolitan area*



Red lines show Public Use Microdata Areas (PUMA) boundaries defined by the US Census in the latest vintage (2020). Green areas show the worksites of H-1B workers. They are ZIP Code Tabulation Areas (ZCTA), shaded in proportion to the average number of H-1B workers per year during FY2021–2024, from Bloomberg (2025). Corporate ZIP codes not corresponding to a residence have been converted to the ZCTA that contains them. In the Bay Area, the PUMA highlighted in orange is Mountain View & Los Altos (PUMA 08522), in Silicon Valley; in New York it is Chelsea & Hell's Kitchen (PUMA 04104), in Manhattan.

(nine counties, including Santa Clara) includes 56 separate PUMAs.

Including PUMA fixed effects in Table 6 implies comparing H-1B workers whose worksite is in a given PUMA exclusively to natives whose residence is in the same PUMA. Consider Mountain View, California (highlighted in orange in Figure 2a), the worksite for H-1B workers at firms like Google and Intuit. PUMA fixed effects assume that these H-1B workers compete only with natives who *live* within that narrow strip of land—not the many natives who commute from other PUMAs. But housing in Mountain View is among the most expensive in the country, so natives who choose to live there may have systematically higher incomes than those who commute from cheaper areas. Eliminating all natives who live outside the worksite PUMA artifactually inflates native wages in the comparison.

The same logic applies to Chelsea neighborhood of Manhattan (Figure 2b), comprising an entire

PUMA. Living in Chelsea is far more expensive than living in outlying areas of metropolitan New York and commuting to work in Chelsea. Thus workers who choose to rent an apartment within Chelsea for $5,800 per month could have systematically different incomes from those who choose to rent in Bay Ridge, Brooklyn for $2,300 and commute into Manhattan. Comparing H-1B workers who work in Chelsea (but often live outside it) to the highly selected group of natives who *live* in Chelsea serves to artificially inflate natives' relative wages. The New York metro area contains 153 separate PUMAs; these are not separate labor markets.

This intuition is tested systematically by Mansfield (2026, Table 4). He studies the effects on workers across all the PUMAs in a metropolitan labor market from a shock to labor demand in any given PUMA. Only about nine percent of employment gains within the average PUMA go to workers within the same PUMA. This fraction is even lower for the regions (urban), sectors (information, financial activities), and workers (with college education and thus higher incomes) who are relevant to the present analysis. In other words, PUMAs are neighborhoods inside much broader, integrated labor markets within which the vast majority of workers commute *between* PUMAs in response to labor demand at worksites. Comparing exclusively the native workers who live adjacent to H-1B worksites in the Presidio (San Francisco) or Tribeca (New York) to H-1B workers who commute to those neighborhoods for work is highly problematic. It creates the clear danger of biased income comparisons, by ignoring large shares of participants in the same metropolitan labor market who chose where to live due to their incomes, and thereby selecting the sample on income itself.

This is a point widely accepted in mainstream labor economics. It is the fundamental reason why peer-reviewed research on labor market shocks overwhelmingly—if not exclusively—studies the effects of those shocks within commuting zones or metropolitan areas, not within PUMAs.[41] Incomes vary systematically across urban neighborhoods, and most of the people who work in the average urban PUMA do not reside in that neighborhood. Thus comparing their incomes to people who reside within the worksite PUMA can typically and substantially bias attempts to compare incomes between groups of workers, or measure the response of workers in a broadly

[41] Canonical studies define local labor markets as commuting zones or metropolitan areas: Moretti (2011) in his *Handbook* chapter on local labor markets, Bartik (1991) in the framework giving rise to the "Bartik instrument," Card (2001, 2009) on immigrant-native competition, Autor et al. (2013) on Chinese import competition, Acemoğlu and Restrepo (2020) on robots, and Ashenfelter et al. (2022) and others on employer monopsony. No influential study in the literature defines local labor markets as PUMAs.

integrated labor market to shocks elsewhere in that market. It is also the reason that, as mentioned above, US law requires H-1B workers' wages to be compared to US workers in the broader commuting zone, not the narrow neighborhood of the worksite.

This problem points to a reinterpretation of the several weighted regression estimates presented by Borjas (2026, e.g. Table 2, panel B). The stated purpose of this weighting is "the construction of 'better' native baseline groups" (Borjas 2026, 11). When the native worker sample is reweighted to have the same PUMA-level distribution as H-1B *worksites*, H-1B workers appear to have even lower wages relative to comparable natives. The problem with this unusual decision is apparent in Figure 2. Reweighting the native sample to overweight natives who *reside* in the PUMAs where H-1B workers' worksites are located (in dark green) overweights the natives who live within some of the most expensive neighborhoods of the metropolitan labor market, such as Chelsea, Tribeca, and the Financial District. Far from resulting in a more meaningful comparison of workers, this further aggravates the bias created by truncating from the sample all natives who live outside the narrow PUMA where an H-1B worksites is located.

The first two of these choices—undisclosed mass-imputation and mismatched years—constitute methodological errors. The third is an assumption ruled out as implausible by all influential research on the spatial extent of local labor markets, as well as by US law. I therefore interpret the rightmost column of Table 1 and Table 6 as a "corrected" estimate of the gap between H-1B workers' base salary and native workers' total wage compensation, without adjusting for differences in employer tenure or wage concept, the quantity estimated by Borjas and the basis for his calculation of the $100,000 tax.

5 Fundamental limitations of the case for an H-1B tax

The preceding empirical analysis set aside a more fundamental question of theory: whether it is optimal to impose immigration tariffs in response to an immigrant-native wage gap in general. If there were a substantial empirical wage penalty for H-1B workers, would this justify an immigration tariff in principle? The claim that H-1B workers reduce native wages is not a market failure (the labor market may clear) but a distributional concern: the policymaker may prefer to transfer surplus from firms and foreign workers to the government, and thus in theory at least, to native taxpayers. The question is whether a per-worker tax achieves that redistribution effectively, or is dominated by alternatives. Sharma and Sparber (2024) explore the efficiency of an H-1B tax relative to quotas; here I highlight the comparison to other alternative policies.

The literature generally finds that the H-1B program's restrictions on worker mobility convey monopsony power on employers. H-1B workers face substantial costs of changing employers and have strong incentives to remain at a single employer during a protracted green card process, resulting in low employer-switching rates (Depew et al. 2017) and a wage jump upon obtaining a green card (Mukhopadhyay and Oxborrow 2012; Wang 2021), consistent with employer monopsony power (see also Mundra and Bagheri 2025; Amior and Manning 2025). Kline (2025, 665) cites the H-1B visa as a quintessential example of an institution giving employers additional monopsony power over workers.

Given that this monopsony power arises from policy restrictions on H-1B workers' mobility, *no* wage gap implies that an immigration tariff must raise welfare. In the most general case, a per-worker tax reduces welfare—even the welfare of native workers specifically—relative to alternative policies such as wage floors or enhanced worker mobility. This remains true even when employers' elasticity of demand to the tax is low. This section builds up the finding from a minimal, general model.

5.1 Firms' hiring decision under monopsony

Consider a firm producing output $Y = f(L)$ using labor L, where $f'(L) > 0$ denotes the marginal product of labor with diminishing returns ($f''(L) < 0$). Unlike the competitive benchmark where the firm faces a flat wage, this firm faces an *upward-sloping labor supply curve*:

$$w = w(L), \qquad w'(L) > 0. \tag{2}$$

Here $w(L)$ is the wage the firm must pay *each* of its L workers; to attract one more, it must raise the wage for all. The total expenditure on labor is $C(L) = w(L) \cdot L$, and the marginal expenditure—the additional cost of hiring one more worker—is

$$\mathrm{ME}_L \;\equiv\; C'(L) = \underbrace{w(L)}_{\substack{\text{wage paid to}\\\text{new worker}}} + \underbrace{w'(L)\cdot L}_{\substack{\text{wage increase for}\\\text{existing workers}}}, \tag{3}$$

with $\mathrm{ME}_L > w(L)$. The firm maximizes profit $\pi = f(L) - w(L)\cdot L$, with the first-order condition $f'(L^m) = w(L^m) + w'(L^m)\cdot L^m = \mathrm{ME}_L(L^m)$. In standard fashion, the firm equates the marginal revenue product of labor to marginal expenditure—not to the wage. Because $\mathrm{ME}_L > w$, the firm hires fewer workers and pays a lower wage than in the competitive outcome L^* where $f'(L^*) = w(L^*)$.

The wage markdown (μ) is the gap between the wage and the worker's marginal product:

$$\mu \;\equiv\; f'(L^m) - w(L^m) = w'(L^m)\cdot L^m > 0. \tag{4}$$

The markdown grows with a more steeply-sloped labor supply (w', reflecting worker immobility) and with the firm's scale (L^m).

5.2 A per-worker tax deepens the distortion

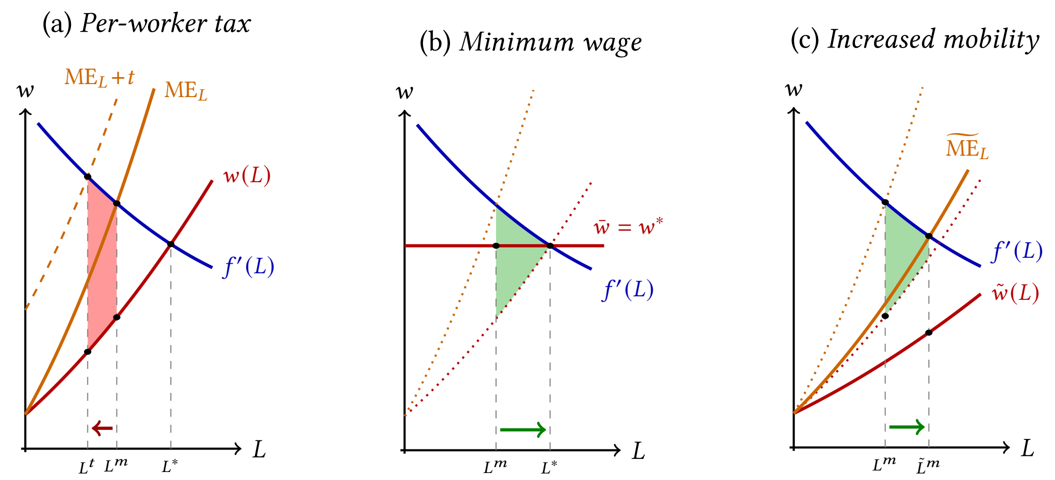
Now impose a per-worker tax $t > 0$ on the firm. Profit becomes $\pi = f(L) - w(L)\cdot L - tL$, and the first-order condition is $f'(L^t) = w(L^t) + w'(L^t)\cdot L^t + t = \mathrm{ME}_L(L^t) + t$. The only way to satisfy this, because $f''(L) < 0$ (the marginal product schedule is downward-sloping), is with lower employment:

$$L^t < L^m < L^*. \tag{5}$$

The tax reduces employment further below the already-too-low monopsony level, reduces the wage ($w(L^t) < w(L^m)$), and *widens* the markdown. In the language of standard incidence analysis, the distortion deepens: the deadweight-loss triangle between the marginal product and supply curves expands (Figure 3a).

Consider instead two alternatives to the tax. In Figure 3b, a *wage floor* $\bar{w}$ set at the competitive wage w^* makes the effective supply curve horizontal for $L \leq w^{-1}(\bar{w})$, collapsing ME_L to $\bar{w}$.

Figure 3: Welfare Effects of Three Policy Instruments under Monopsony: Immigration taxes, wage regulation, and mobility regulation



Three policy instruments under monopsony. (*a*) A per-worker tax shifts ME_L upward, reducing employment from L^m to L^t and creating additional deadweight loss (red shading). (*b*) A minimum wage at $\bar{w} = w^*$ makes $\mathrm{ME}_L = \bar{w}$, expanding employment to L^* and eliminating the deadweight loss (green shading). (*c*) Greater worker mobility flattens the supply curve, lowering ME_L and expanding employment from L^m to $\tilde{L}^m$ (green shading). Dotted lines in panels (b) and (c) show the original monopsony regime.

The firm then hires where $f'(L) = \bar{w} = w^*$, restoring competitive employment L^* (e.g. Card and Krueger 1995; Cahuc and Laroque 2014). In Figure 3c, *improved worker mobility* flattens the supply curve—lowers $w'(L)$—which reduces ME_L toward $w(L)$, expanding employment. Both instruments lower the effective cost of the marginal hire; a tax raises it. The tax has the wrong sign from an efficiency standpoint.

Beyond this, when heterogeneous firms compete for a fixed number of H-1B visa slots, a per-worker tax can induce adverse selection among employers. Firms whose wage markdown is small (those closest to paying the competitive wage) find that the tax exceeds their surplus from the hire and stop petitioning. The remaining employers are those with the *largest* markdowns. The average H-1B worker employed after the tax therefore faces a *wider* gap between wage and marginal product than before, because the composition of H-1B employers shifts toward more monopsonistic firms.

5.3 Revenue-maximizing tax and welfare

A government might set the tax to maximize revenue $G(t) = t \cdot L(t)$ rather than to correct the monopsony distortion. Borjas (2026) explicitly recommends an H-1B tax that is calibrated to "maximize government revenue". The first-order condition for revenue maximization is

$$G'(t) = L(t) + t\,\frac{dL}{dt} = 0 \quad\Longrightarrow\quad t_R^* = -\frac{L}{dL/dt} > 0, \tag{6}$$

where $dL/dt < 0$. At t_R^* the government collects the maximum feasible revenue from the per-worker tax.

In this framework, *any* positive tax—including t_R^*—reduces total welfare relative to the no-tax monopsony baseline. Define welfare as the total surplus generated in the labor market:

$$W(t) \;\equiv\; f\big(L(t)\big) - \int_0^{L(t)} w(s)\,ds. \tag{7}$$

This is the area between the marginal product curve and the supply curve up to $L(t)$. Differentiating:

$$\frac{dW}{dt} = \big[f'\big(L(t)\big) - w\big(L(t)\big)\big]\,\frac{dL}{dt} = \underbrace{\mu(t)}_{>0}\;\underbrace{\frac{dL}{dt}}_{<0} \;<\; 0. \tag{8}$$

The markdown $\mu(t) = f'(L) - w(L) > 0$ for any $t \geq 0$ (the monopsony distortion persists), and $dL/dt < 0$ (proven in the Appendix). Welfare is strictly decreasing in t for all $t \geq 0$: every dollar of tax revenue comes at a cost in total surplus. This applies equally to the revenue-maximizing tax t_R^* and to a tax set equal to the markdown $t = \mu$. Neither corrects the price distortion; both shrink the surplus pie.

5.4 The tax is not optimal even with zero wage elasticity of labor demand

The wage elasticity of labor demand, $\varepsilon_d \equiv \frac{\partial L}{\partial w}\frac{w}{L}$, measures how much the firm adjusts employment when labor costs change. Suppose, as Borjas (2026) does, that US firms' demand for H-1B workers is invariant to a \$100,000 per-worker tax. If $\varepsilon_d = 0$, a tax t does not eliminate jobs: $L^t = L^m = \bar{L}$, for any t. The tax instead extracts $t \cdot \bar{L}$ from the firm without reducing employment. But it still fails as a monopsony correction: workers continue to earn $w(\bar{L}) < f'(\bar{L})$, and the markdown $\mu = f'(\bar{L}) - w(\bar{L})$ persists. A minimum wage set at $f'(\bar{L})$, by contrast, achieves the same transfer from the firm while routing it to workers:

$$\begin{array}{lcccc} \textit{Policy}: & \Delta\Pi & \Delta W & \Delta G & \text{Markdown corrected?} \\ \hline \text{Tax } (t = \mu) & -\mu\bar{L} & 0 & +\mu\bar{L} & \text{No} \\ \text{Wage floor}(\bar{w} = f'(\bar{L})) & -\mu\bar{L} & +\mu\bar{L} & 0 & \text{Yes} \end{array} \tag{9}$$

Here $\Delta\Pi$ is the change in firm profit, ΔW the change in worker welfare, and ΔG the change in government revenue. The wage floor strictly dominates on efficiency grounds: it costs the firm the same amount but eliminates the price distortion.[42]

The strongest distributional case for the tax is visible in (9): the tax generates revenue $\Delta G > 0$ that can in principle benefit native taxpayers, while the wage floor routes the same transfer to foreign workers ($\Delta W > 0$) with nothing for the treasury. A policymaker weighting only native welfare might therefore prefer the tax. But three factors offset this advantage. First, natives predominantly own the firms paying the tax (as shareholders and through pension funds) so $\Delta\Pi$ falls largely on natives themselves, partly shuffling revenue between natives' pockets net of deadweight costs. Second, monopsony incidence of the tax depresses H-1B wages, which can spill over to native workers at the same firms through linked internal salary bands. Third, as shown next, when foreign and native workers are complements, a tax-induced reduction in foreign employment directly reduces native employment.

5.5 The tax is not optimal even if only native workers' welfare counts

In immigration settings, a policymaker might assign weight only to native welfare. But even under this restriction, a per-worker tax on foreign labor can reduce native employment. Let the firm employ L_N native workers and L_F taxed foreign workers, producing $Y = f(L_N, L_F)$, where $f_N \equiv \partial f/\partial L_N > 0$ and $f_F \equiv \partial f/\partial L_F > 0$ are marginal products, and $f_{NN} < 0$, $f_{FF} < 0$ (diminishing returns in each type). Natives are hired competitively at wage w_N; foreign workers face upward-sloping supply $w_F(L_F)$ with $w_F' > 0$ and a per-worker tax t. The firm's first-order

[42] Derived in Appendix A5.2.

conditions are $f_N(L_N, L_F) = w_N$, and $f_F(L_N, L_F) = w_F(L_F) + w'_F(L_F) \cdot L_F + t$.

This allows derivation (in the Appendix) of the effect of immigrant labor inflows on native employment,

$$\frac{dL_N}{dL_F} = -\frac{f_{NF}}{f_{NN}}. \tag{10}$$

When the two types are complementary ($f_{NF} > 0$), this ratio is positive: $dL_N/dL_F > 0$ (since $f_{NN} < 0$). A tax-induced reduction in foreign employment L_F therefore reduces native employment L_N. The qualitative result—that taxing foreign workers harms native employment—holds for any production technology with $f_{NF} > 0$, the empirically relevant case. Under CES technology specifically, $f_{NF} > 0$ for any finite elasticity of substitution $\sigma < \infty$, and $dL_N/dL_F = L_N/L_F$—a one-percent reduction in foreign employment causes a one-percent reduction in native employment.

The intuition is a homogeneity property of CES technology. Because CES is homogeneous of degree one, each factor's marginal product depends only on the *ratio* L_F/L_N, not on the levels separately. The native first-order condition $f_N = w_N$ therefore pins this ratio. If a tax reduces L_F, the only way to maintain $f_N = w_N$ is for L_N to fall in the same proportion—hence the unit elasticity, for any finite σ.

The tax thus harms the very group whose welfare the policymaker seeks to protect. Even under a welfare criterion that assigns zero weight to foreign workers, a minimum wage or visa portability reform dominates: it corrects the monopsony distortion, narrows the wage gap between foreign and native labor, and does not generate the negative spillover to native employment that a tax creates through the complementarity channel.[43]

[43] When foreign and native workers are perfect substitutes ($f = g(L_N + L_F)$ with $g'' < 0$), the cross-partial is $f_{NF} = g'' < 0$, reversing the sign: a tax on foreign workers *increases* native employment through substitution. But even in this case the tax fails to correct the monopsony distortion—it routes rents to the government rather than to workers, leaves the structural source of employer power intact, and creates deadweight loss whenever total labor demand has any elasticity. A minimum wage or mobility reform dominates under either welfare criterion. See the Appendix.

6 Conclusion

This paper has examined the empirical and theoretical foundations of the $100,000 per-worker tax imposed on H-1B high-skill immigrant visas in 2025. Using the same administrative data on individual H-1B workers and the same census data source for US native workers in the only public study to shed light on the White House's reasoning for the tax, I find that the base salary of H-1B workers is approximately 1.2 percent less than the total wage income of US natives with comparable education, age, gender, occupation, employer tenure, and geographic location—a gap that is statistically indistinguishable from zero. The two wage concepts being compared are not equivalent: base salary for H-1B workers excludes bonuses and equity compensation, while total wage income for US natives includes these components. Accounting conservatively for this difference implies that H-1B workers receive a small wage premium relative to comparable natives, not a penalty. No evidence in these data supports the claim that H-1B workers' wages are substantially below those of comparable US natives.

The 16 percent wage gap reported by Borjas (2026)—who interprets it as the quantitative foundation for the $100,000 tax—arises from four specific methodological choices, each of which inflates the estimated gap. These are: an undisclosed imputation that assigns erroneous education levels to more than a third of H-1B workers in FY2023–2024; the unexplained pooling of four years of H-1B data against a single year of US native data rather than straightforward within-year comparisons; a nonstandard definition of local labor markets using Census Public Use Microdata Areas, units so narrow that they assume well-integrated metropolitan labor markets are fragmented into scores of disconnected "markets"; and failing to consider the large gap in employer tenure between average native workers and new H-1B employees. Correcting these choices, sequentially and collectively, accounts for the entire discrepancy between the 16 percent estimate and the 1.2 percent estimate obtained here.

These empirical findings bear directly on the design and justification of immigration tariffs. Even setting aside the corrected wage gap, the theoretical case for a revenue-maximizing per-worker tax on H-1B labor is weak. Under monopsony—the labor market structure that the H-1B program's own restrictions on worker mobility tend to create—a per-worker tax deepens rather than corrects the existing distortion. It reduces employment below the already-inefficient monopsony level, widens the gap between workers' wages and their marginal product, and reduces total welfare for any positive tax rate. This remains true even when the wage elasticity of labor demand is zero and even when the policymaker assigns no weight whatsoever to immigrant welfare. The strongest distributional case for the tax—that it generates revenue benefiting natives—is offset by native ownership of the firms bearing the tax, by adverse wage spillovers to native workers, and by complementarity between foreign and native labor that transmits tax-induced reductions in foreign employment to native employment. When heterogeneous firms compete for fixed visa slots, the tax further induces adverse selection, shifting the composition of H-1B employers toward more monopsonistic firms. Alternative policies, including wage floors and enhanced portability of H-1B status across employers, dominate the tax under standard welfare criteria. The magnitude and design of the H-1B tax are not supported by the available evidence on relative wages or by the standard public economics of monopsonistic labor markets.

Finally, the methods used in the Borjas study are also important to carefully examine because those methods, and the study's results, might be relevant to other policy debates that presume a large wage gap between H-1B workers and natives. For example, the US Administration has announced its intention to revise how determinations of "prevailing wages" will be made for purposes of the H-1B visa program. Whether a significant wage gap exists would be relevant in both determining whether existing wage floors function as intended and whether or how they might be reformed. ■

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Online Appendix

"Immigrant-Native Wage Gaps and Immigration Taxes: Examining the Case for an H-1B Visa Tax "

Michael A. Clemens — March 2026

This Appendix presents details of the dataset construction, robustness checks, details of the SIPP analysis, and derivations of key equations.

Contents

A1 Dataset construction

The ten data sources are:

1. *H-1B I-129 petition microdata* (FY 2021–2024), five CSV files from USCIS via (Bloomberg 2025).
2. *DOL LCA disclosure files* (FY 2017–2025), from DOL/ETA: annual for FY 2017–2019, quarterly for FY 2020–2025.
3. *American Community Survey* 2021–2024 one-year samples (Ruggles et al. 2025).
4. *CPS Job Tenure Supplement* (January files, 2010–2025), from IPUMS CPS; provides weekly earnings (`earnweek2`) and employer tenure (`jtyears`, collected in even-year supplements) (Flood et al. 2025).
5. *CPI-U-RS* (monthly, all items), from BLS; used to build fiscal-year deflators.
6. *GEOCORR 2022* (ZCTA→PUMA and PUMA→CBSA), from Missouri Census Data Center; maps ZCTAs to 2022-vintage PUMAs and PUMAs to CBSAs, both by plurality population.
7. *2018 Census occupation crosswalk*, from Census Bureau; maps 2010- and 2018-vintage SOC codes (and Census 2010 occupation codes) to 2018 Census occupation codes.
8. *UDS Mapper ZIP-to-ZCTA crosswalk* (2022), from Chris Prener (Washington Univ.); maps non-ZCTA ZIPs to nearest ZCTA, recovering PUMAs for ~5,300 corporate/PO Box H-1B worksite ZIPs.
9. *PUMA 2010-to-2020 population crosswalk*, from IPUMS to harmonize PUMA vintages.
10. *David Dorn's PUMA-to-CZ crosswalk* (Autor et al. 2019) maps vintage-2010 PUMAs to 1990 commuting zones.

A1.1 H-1B Data Construction

The five Bloomberg CSV files are appended; 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; ~35–40% of FY 2023–2024 records have both fields blank. To replicate the summary statistics of Borjas (2026), 100 percent of the missing education values must be imputed as bachelor's, and these imputed values are flagged; ~370 records with observed sub-bachelor's education are dropped. Gender is from BEN_SEX; age is fiscal year minus birth year; worksite ZIP is the first five digits of WORKSITE_ZIP. Key LCA variables carried forward: six-digit SOC code, prevailing-wage level (1–4), and full-time indicator.

A1.2 Crosswalks and Geographic Assignment

Occupation. The Census Bureau's crosswalk workbook maps 2018- and 2010-vintage SOC codes to 2018 Census occupation codes; wildcard SOC codes (e.g. 15-113X, 13-20XX) are expanded into all possible 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.

ZIP to PUMA. The Missouri Census Data Center's GEOCORR 2022 maps each ZCTA to the PUMA with the largest population share (2022-vintage, that is, the 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 (PUMA_ZCTAFIX), recovering ~5,300. Records with no usable geography (178) are dropped.

PUMA vintage harmonization. The ACS uses vintage-2010 PUMAs for 2021 and vintage-2020 for 2022–2024; H-1B PUMAs (from GEOCORR PUMA22) are vintage-2020. The IPUMS PUMA 2010-to-2020 crosswalk is used forward to translate ACS 2021 codes to vintage-2020 (PUMA_CONSISTENT) and in reverse to translate ACS 2022–2024 and all H-1B codes back to vintage-2010 (PUMA2010); 1:many mappings are resolved by plurality population.

Commuting zones and CBSAs. David Dorn's crosswalk (Autor et al. 2019) maps vintage-2010 PUMAs to 1990 commuting zones; GEOCORR maps vintage-2020 PUMAs to CBSAs. In both, where a PUMA spans multiple targets, the largest allocation factor wins.

A1.3 Sample Restrictions, Wage Deflation, and Stacking

The initial ACS comparison sample (2021–2024) is restricted to: native-born US citizens; bachelor's or higher; salaried; employed; ages 21–50; full-time (≥35 hrs/wk); year-round (≥50 wks); positive wage income below top-code; and a $28,800 wage floor. H-1B restrictions (post-LCA-merge): full-time (LCA field); ages 21–50; no-geography records dropped; $28,800 wage floor; 99.95th-percentile wage cap (≈$1.2M); observed sub-bachelor's education dropped. CPI-U-RS fiscal-year deflators (ratio of CY 2023 average to each fiscal year's Oct–Sep average) are applied to both samples.

H-1B workers receive weight one; ACS respondents receive PERWT. After stacking, REPLICATION_SAMPLE flags all H-1B plus ACS 2023 (Borjas's baseline); other ACS years are carried for extensions.

A1.4 CPS Job Tenure Supplement

January CPS files for 2010–2025 from IPUMS CPS with sample restrictions mirroring the ACS: native-born; bachelor's or higher; salaried; employed; ages 21–50; full-time (≥35 hrs/wk); valid weekly earnings above the annualized wage floor ($28,800/52 ≈ $554/week). No year-round filter is applied (CPS monthly files lack annual weeks worked). The primary wage is EARNWEEK2 (JTS weekly earnings, present for all waves including 2024); annual wages are EARNWEEK2 × 52. Employer tenure or "job tenure" (JTYEARS) is collected in even-year January supplements only. Pre-2020 CPS occupation codes (Census 2010) are mapped to Census 2018 via the same crosswalk workbook, with unmatched codes assigned to broad-category fallbacks. CPS geography uses state × metro area (CBSA via METFIPS, ∼402 groups), coarser than the ACS state × PUMA (∼3,383 groups); H-1B records are assigned CBSAs via the GEOCORR PUMA-to-CBSA crosswalk while CPS records use METFIPS directly. CPS observations are weighted specifically to the Job Tenure Supplement by JTSUPPWT.

A2 Robustness to alternative definition of native worker

Table A1 shows how the results of Table 1 change with an alternative assumption about what constitutes a 'native'. In the main results, any natural-born US citizen is a native; that is, the definition excludes any naturalized US citizen or noncitizen. Table A1 shows how the results change when only people born within the 50 US states or the District of Columbia are counted as 'natives' (omitting US citizens born in territories like Puerto Rico and Guam, and omitting natural-born US citizens born to citizen parents outside the 50 states). The coefficients of interest in column 4 (linear and quantile specifications) shift by two tenths of a percentage point.

A3 Robustness to including H-1B workers with observed education in FY2023–2024

The analysis in Table 1 restricts the sample to the years 2021 and 2022, the only two years in which the underlying H-1B dataset is uncorrupted by widespread missing education levels. An alternative approach would be to include the years 2023 and 2024, thereby including only the H-1B workers with observed education. The clear disadvantage of this approach is that education may not be missing-at-random. Thus including H-1B workers with selectively nonmissing education levels has the potential to introduce

Appendix Table A1: ALTERNATE CRITERION FOR 'NATIVE': Replicating Table 1 without including natural-born US citizens born to citizen parents outside US territory

	(1) ln real wage	(2) ln real wage	(3) ln real wage	(4) ln real wage
Linear				
H-1B	0.1762	0.0556	0.1079	-0.0771
	(0.0066)	(0.0143)	(0.0173)	(0.0352)
Quantile				
H-1B	0.2071	0.0928	0.1237	-0.0704
	(0.0063)	(0.0143)	(0.0171)	(0.0349)
N	635,632	631,024	631,024	631,009

bias of unknown sign and magnitude.

Specifically, if the education levels are missing for H-1B workers with higher than average earnings, including H-1B workers with observed education in this way would tend to make H-1B wages appear artificially lower than native wages; and vice versa.

Nevertheless, it may be of interest to view the results when those workers are included. Table A2 reports this exercise. The regressions it reports are identical to Table 1, except that all H-1B and native workers with *observed* education levels in 2023–2024 are included. The results are broadly similar. The key point estimate in column 4 shifts by two percentage points, but with a standard error of four percentage points, the new coefficient estimate cannot be statistically distinguished from the one in Table 1. More importantly, including the years 2023–2024, in which about a third of H-1B workers' education levels are rendered missing in the original database by an unknown criterion, could bias the estimates in Table A2 to an unknown degree. For this reason the main text relies on the years 2021 and 2022, when education levels are fully reported for both H-1B workers and natives, allowing a comparison without this unknown bias.

A4 SIPP Analysis

The analysis in subsubsection 3.2.2 requires an ancillary calculation of the number of low-tenure workers dropped from the ACS sample. This is only used in order to establish that the ACS sample of workers employed for $\geqslant 50$ of the past 52 weeks makes it contain few workers with tenure less than one year, so that the average tenure of workers in the CPS Job Tenure Supplement who are comparable to workers in the ACS is estimated by dropping workers with less than one year of tenure. The SIPP longitudinal sample allows estimation of how many weeks of non-employment precede the typical job-start of a prime-age worker with a college education; all who start a new job with more than two weeks of non-employment are thereby truncated from the ACS sample by the full-time criterion.

The resulting CDF is shown in Figure A1. The estimation of this CDF proceeds as follows:

A4.1 Data and sample

The analysis uses the public-use person-month files from the Survey of Income and Program Participation (SIPP), reference years 2018–2024. Over this period the Census Bureau fielded three overlapping panels (initial interview cohorts 2018, 2020, and 2021), each following respondents for up to four consecutive

Appendix Table A2: Robustness to Including Workers from Years of Corrupted H-1B Data: Comparing H-1B and US native compensation, without considering unobserved differences in wage concept or employer tenure, across all years 2021-2024 but omitting workers with unobserved education

Dep. var.: ln real wage income	(1)	(2)	(3)	(4)
(a) *Linear*				
H-1B worker	0.1635	0.0456	0.0860	−0.0948
	(0.0009)	(0.0163)	(0.0201)	(0.0424)
Observations	1,323,513	1,316,782	1,259,050	1,259,019
(b) *Quantile (median)*				
H-1B worker	0.1928	0.0801	0.1007	−0.0891
	(0.0046)	(0.0161)	(0.0198)	(0.0420)
Observations	1,323,513	1,316,782	1,259,050	1,259,024
Fixed effects:				
Year	Yes	Yes	Yes	Yes
Local labor market (geography)	—	Yes	Yes	Yes
Age, education, gender	—	—	Yes	Yes
Occupation (4 digit)	—	—	—	Yes
Standard errors:				
Robust	Yes	—	—	—
Clustered: geography	—	Yes	Yes	Yes
Clustered: occupation	—	—	—	Yes

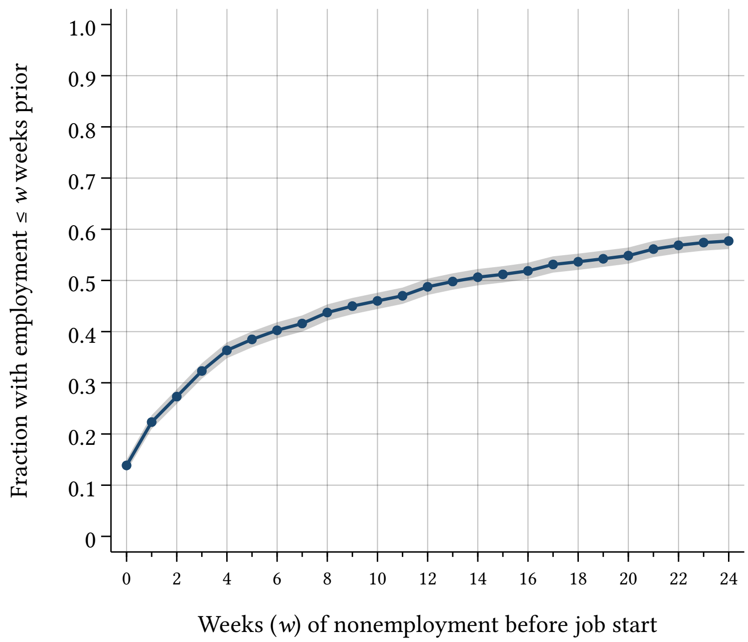
Sample includes only workers employed, full-time and year-round, age 21–50 with a college degree or above. Includes all workers with observed education levels 2021–2024. Local labor markets are defined as commuting zones (Autor et al. 2019). Age fixed effects are by individual year; education fixed effects are for the four categories of bachelor's, master's, professional degree, or doctorate. Standard errors in parentheses; two-way clustering for quantile regressions uses the method of Cameron et al. (2011). US natives weighted with the census sampling weight, H-1B workers weighted by unity (full universe). All regressions include suppressed constant term.

calendar years. Each annual file records, for every respondent, up to seven concurrent job spells per reference year, with each spell characterized by the first and last week the job was held (EJB*N*_STARTWK, EJB*N*_ENDWK; week 1 through 52) and a work-arrangement flag distinguishing employer jobs from self-employment (EJB*N*_JBORSE). I restrict the sample to persons aged 21–50 with at least a bachelor's degree (EEDUC $\geq$ 43) and consider only employer jobs (JBORSE = 1). I use the first four job slots per year; slots 5–7 are populated for fewer than three percent of sample-eligible person-years.

A4.2 Cross-year job linking

To measure nonemployment gaps that span calendar-year boundaries, all job spells from 2018 through 2024 are placed on a unified weekly timeline. Within each reference year y, the SIPP records the first

Figure A1: Average Duration of Non-Employment Before New Job Start, Including Never-Employed: Prime-age, college educated workers, 2018–2024



Nationally representative sample of all job starts by college-educated workers age 21–50, in the Survey of Income and Program Participation (SIPP 2018–2024) three pooled overlapping panel cohorts 2018, 2020, and 2021. CDF includes never-employed (left-censored). New jobs are jobs at employers (self-employment excluded). Shaded gray shows 95 percent confidence interval.

week the person held job j as $\mathrm{STARTWK}_{jy} \in \{1, \dots, 52\}$ and the last week as ENDWK_{jy}. I map these to cross-year week numbers:

$$w_{jy}^{\text{start}} = 52\,(y - 2018) + \mathrm{STARTWK}_{jy}, \qquad w_{jy}^{\text{end}} = 52\,(y - 2018) + \mathrm{ENDWK}_{jy}.$$

Each person's annual files are merged on the person identifier (SSUID × PNUM), yielding up to 4 slots × 7 years = 28 job spells per person on the common timeline.

A4.3 Identifying new job starts

A job slot is flagged as a *new employer job start* if STARTWK > 1 and the work arrangement is employer (JBORSE = 1). Jobs with STARTWK = 1 are excluded because they are ambiguous: they may be continuations of jobs that began in a prior year (whose prior-year spell is already captured elsewhere on the timeline) or genuine January starts, and these cases cannot be distinguished. I further exclude starts where another job held by the same person is still active during the start week—that is, where some other slot k satisfies $w_k^{\text{start}} \le w_j^{\text{start}}$ and $w_k^{\text{end}} \ge w_j^{\text{start}}$. These are secondary-job additions (e.g., moonlighting) rather than labor-market transitions; the person was continuously employed, so the nonemployment gap is mechanically zero. Retaining them would inflate the job-to-job fraction well above external benchmarks such as the LEHD Job-to-Job Flows Explorer.

A4.4 Gap computation and left censoring

For each new job start at cross-year week W, the nonemployment gap measures how many weeks the person held no job immediately before starting. Because individuals may hold multiple jobs across years and slots, I scan all other spells $k \neq j$ with $w_k^{\text{start}} \leq W - 1$ to find the latest week of prior employment:

$$L = \max_{k \neq j} \Big[\min\big(w_k^{\text{end}},\ W - 1\big) \Big], \qquad \text{gap} = W - L - 1.$$

A gap of zero indicates a direct job-to-job transition; negative values from within-week overlaps are recoded to zero. If no prior employment is found across all 28 slots, the gap is set to $W - 1$ (weeks from the start of the 2018 timeline) and the observation is flagged as left-censored.

Left censoring can occur for job starts in 2018 whose prior employment ended before the observation window. The left-censored group includes both first-time labor-force entrants and long-term nonemployed whose last job predates 2018. Because these measured gaps are large, left-censored observations enter the CDF denominator and pull it downward at every displayed week, correctly reflecting that a nontrivial share of new hires come from outside the ranks of the recently employed.

A4.5 CDF construction, weights, and standard errors

The CDF at integer week $w = 0, 1, \ldots, 24$ is $\hat{F}(w) = \sum_i \mathbf{1}[\text{gap}_i \leq w] \cdot \text{wt}_i \ / \ \sum_i \text{wt}_i$, where both sums run over all job starts and wt_i is the cross-sectional person weight (WPFINWGT) from the reference year, averaged across the twelve monthly observations within that year. No single set of longitudinal weights spans all seven reference years; however, because the CDF is a ratio estimator, only the *relative* magnitudes of the weights matter, and cross-sectional weights closely approximate what seven-year longitudinal weights would yield. Point estimates are monotonized (replaced with the running maximum) and confidence bands are clipped to bound the point estimate.

Confidence intervals are $\hat{F}(w) \pm 1.96\,\widehat{\text{se}}$, where $\widehat{\text{se}}$ is the Taylor-linearized standard error from Stata's SVY: MEAN estimator. Balanced repeated replication is infeasible because no single set of replicate weights spans the pooled sample; the linearized standard errors are conservative relative to BRR but yield narrow intervals given the large sample.

A4.6 Validation in full-universe data

In full-universe data on formal sector US workers, roughly half (44.3 percent) of college-educated workers starting a new job in the United States during 2021–2024 did so after a period of nonemployment lasting more than three months: US Census Bureau, "A Comparison of Hires to Jobs by Year/Quarter filtered by Education", *J2J Explorer*, accessed February 27, 2026. *J2J explorer* data are derived from the Longitudinal Employer-Household Dynamics (LEHD) database, covering almost all formal private-sector employment in the United States.

The estimates agree broadly with the literature finding that for a college-educated worker in the United States, roughly half of transitions from one job to another involved a spell of 8–9 weeks of unemployment on average, or an even longer spell of nonparticipation in the workforce (Bjelland et al. 2011; Haltiwanger et al. 2018; Cairó and Cajner 2018; Fujita et al. 2024; Bureau of Labor Statistics 2024; Cline and Kaymak 2025).

A5 Derivations

This section derives key, non-obvious results from section 5.

A5.1 Revenue-maximizing tax reduces welfare

With tax $t \geq 0$, the firm employs $L(t)$ workers and the government collects $G(t) = t \cdot L(t)$. Define total welfare as the area between the marginal product and supply curves:

$$W(t) = f\big(L(t)\big) - \int_0^{L(t)} w(s)\, ds.$$

This equals the sum of firm profit, worker surplus, and government revenue: $W = \pi + \text{WS} + G$. Then,

$$G'(t) = L(t) + t\,\frac{dL}{dt}.$$

At $t = 0$, $G'(0) = L^m > 0$, so a small tax raises revenue. As t grows, $L(t)$ shrinks, and the second term becomes increasingly negative. Setting $G'(t_R^*) = 0$ yields

$$t_R^* = -\frac{L(t_R^*)}{dL/dt\big|_{t_R^*}} > 0,$$

the revenue-maximizing tax. Differentiating $W(t)$,

$$\frac{dW}{dt} = \big[f'(L) - w(L)\big]\,\frac{dL}{dt} = \mu(t)\,\frac{dL}{dt}.$$

Under monopsony, $\mu(t) = f'(L) - w(L) > 0$ for all $t \geq 0$. The markdown not only remains but widens under the tax. Since $dL/dt < 0$, then $dW/dt < 0$ for all $t \geq 0$.

This means total surplus is maximized at $t = 0$ (no tax). Both the revenue-maximizing tax t_R^* and a tax set equal to the initial markdown $t = \mu(0)$ reduce total welfare relative to the no-tax baseline. The revenue-maximizing tax and the markdown-sized tax will generally differ in magnitude, but both share one trait: they shrink the total surplus because each additional dollar of tax pushes employment further below the efficient level.

A minimum wage $\bar{w} = w^*$ *expands* employment from L^m to L^*, so the surplus integral grows—welfare rises. The tax contracts employment from L^m toward zero, so the surplus integral shrinks—welfare falls. Revenue maximization and welfare maximization are fundamentally opposed objectives.

A5.2 Zero wage elasticity of labor demand

If labor demand is perfectly inelastic, the MRPL curve is vertical at some $\bar{L}$: the firm hires $\bar{L}$ workers regardless of cost. Formally, this means $f'(L)$ drops discontinuously (or extremely steeply) at $L = \bar{L}$, so that the first-order condition is satisfied at $\bar{L}$ for any wage or tax level.

Under monopsony, the wage is $w^m = w(\bar{L})$ and the markdown is $\mu = f'(\bar{L}) - w(\bar{L})$. A per-worker tax $t \leq \mu$ extracts $t\bar{L}$ from the firm without affecting employment: $L^t = \bar{L}$. The worker's wage remains $w(\bar{L})$.

Under a wage floor $\bar{w} = f'(\bar{L})$, the firm pays $f'(\bar{L})$ per worker. The cost to the firm is $[f'(\bar{L}) - w(\bar{L})] \cdot \bar{L} = \mu\bar{L}$, identical to the tax. But the wage floor routes the transfer to workers, closing the markdown ($w = f'(\bar{L}) = \text{MRPL} \Longrightarrow \mu = 0$). The tax leaves $\mu > 0$ and workers no better off.

A5.3 Two-worker model: perfect substitutes

Let $Y = g(L_N + L_F)$ with $g' > 0$, $g'' < 0$, so that natives and foreign workers are perfect substitutes. Natives are competitively supplied at w_N; foreign workers face monopsony supply $w_F(L_F)$ with $w_F' > 0$

and tax t. Denote total labor $L \equiv L_N + L_F$. The first-order conditions are

$$g'(L) = w_N, \tag{A.1}$$

$$g'(L) = w_F(L_F) + w_F'(L_F) \cdot L_F + t = \mathrm{ME}_F(L_F) + t. \tag{A.2}$$

From (A.1), total employment L is pinned by w_N, $L = (g')^{-1}(w_N) \equiv \bar{L}$. From (A.2), $w_N = \mathrm{ME}_F(L_F) + t$, so that L_F falls as t rises (since ME_F is increasing in L_F), and $L_N = \bar{L} - L_F$ rises by the same amount.

Native employment: $dL_N/dt = -dL_F/dt > 0$: native employment *rises* one-for-one with the decline in foreign employment. With perfect substitutes, the tax induces substitution toward natives.

Despite the favorable native-employment effect, the tax does not correct the monopsony distortion. Foreign workers are still paid $w_F(L_F) < g'(\bar{L}) = w_N$; the markdown $\mu_F = w_N - w_F(L_F)$ *widens* as L_F falls (since $w_F'(L_F) > 0$ and fewer foreign workers means a lower w_F). The tax routes $t \cdot L_F$ to the government rather than to workers. A minimum wage or portability reform that raises w_F toward w_N corrects the price distortion directly, without the deadweight loss arising from the tax.

A5.4 Two-worker model: CES technology

Let $Y = A\big[\alpha\, L_N^{\rho} + (1-\alpha)\, L_F^{\rho}\big]^{1/\rho}$, with $0 < \rho < 1$, where $A > 0$ is a productivity scalar, $\alpha \in (0,1)$ is the native-labor share, and $\sigma \equiv 1/(1-\rho) > 1$ is the elasticity of substitution. Denote $S \equiv \alpha L_N^{\rho} + (1-\alpha) L_F^{\rho}$ so that $Y \equiv A\, S^{1/\rho}$. The marginal products are

$$f_N = A\,\alpha\, L_N^{\rho-1}\, S^{(1-\rho)/\rho}, \qquad f_F = A\,(1-\alpha)\, L_F^{\rho-1}\, S^{(1-\rho)/\rho}. \tag{A.3}$$

Differentiating f_N with respect to L_F:

$$f_{NF} = A\,\alpha(1-\alpha)(1-\rho)\, L_N^{\rho-1}\, L_F^{\rho-1}\, S^{(1-2\rho)/\rho}. \tag{A.4}$$

Since $0 < \rho < 1$, every factor is strictly positive, so $f_{NF} > 0$: the two types of labor are q-complements. More foreign labor raises the marginal product of natives.

Let $s_N \equiv \alpha L_N^{\rho}/S$ denote the native share of the CES aggregate. Then

$$f_{NN} = -\,A\,\alpha(1-\rho)(1-s_N)\, L_N^{\rho-2}\, S^{(1-\rho)/\rho} \;<\; 0. \tag{A.5}$$

Native employment: Natives are competitively supplied at wage w_N, so $f_N(L_N, L_F) = w_N$. By the implicit function theorem,

$$\frac{dL_N}{dL_F} = -\frac{f_{NF}}{f_{NN}}.$$

Substituting (A.4) and (A.5):

$$\begin{aligned}\frac{f_{NF}}{f_{NN}} &= \frac{A\,\alpha(1-\alpha)(1-\rho)\, L_N^{\rho-1}\, L_F^{\rho-1}\, S^{(1-2\rho)/\rho}}{-\,A\,\alpha(1-\rho)(1-s_N)\, L_N^{\rho-2}\, S^{(1-\rho)/\rho}} \\ &= -\,\frac{(1-\alpha)\, L_N\, L_F^{\rho-1}\, S^{-1}}{1-s_N}. \end{aligned} \tag{A.6}$$

Since $1 - s_N = (1-\alpha)L_F^{\rho}/S$, the expression simplifies to $\frac{f_{NF}}{f_{NN}} = -\frac{L_N}{L_F}$. Thus

$$\frac{dL_N}{dL_F} = \frac{L_N}{L_F} > 0. \tag{A.7}$$

The elasticity of native employment with respect to foreign employment is

$$\frac{dL_N}{dL_F} \cdot \frac{L_F}{L_N} = \frac{L_N}{L_F} \cdot \frac{L_F}{L_N} = 1.$$

Under CES technology, a one-percent tax-induced reduction in foreign employment causes exactly a one-percent reduction in native employment. The complementarity between worker types transmits the cost shock from foreign workers to natives.

This unit elasticity arises because the CES production function is homogeneous of degree one. Thus marginal products depend only on the input *ratio* L_F/L_N, rather than absolute levels. The native labor-market clearing condition $f_N(L_N, L_F) = w_N$ pins L_F/L_N at a constant. If L_F falls by any amount, L_N must fall in the same proportion—yielding unit elasticity regardless of σ.

The tax raises the effective cost of foreign labor, reducing L_F. By (A.7), L_N falls proportionally. A policy-maker who cares only about native welfare therefore faces a negative spillover: the tax intended to capture foreign-labor rents reduces native employment through the production complementarity channel. ■