Will The University Endowment Tax Slow Scientific Progress? Evidence from Elite Economics PhD Programs*

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Abstract

The 2025 university endowment tax hike and other sources of financial pressure may lead the schools that train the most prolific economics researchers to reduce graduate enrollment. Will this affect long-run research output? We use a novel sample of MIT Economics PhD program applicants to estimate the research value-added of eight elite schools. Our estimates mitigate selection bias by controlling for MIT admissions committee rankings—a remarkably strong predictor of long-run research success—and for applicant aspirations as revealed by their application portfolios. While rank controls substantially reduce estimated gaps between elite and non-elite graduates, large differences in value-added remain. Graduates of high-tax and other top-eight schools produce 60-75% more impact-adjusted publications than do comparable graduates from non-top-eight US schools. The elite-school advantage is especially pronounced for top five journal publications. Differences in research success within the elite tier, however, are relatively modest. The out-performance of elite-school PhDs does not appear to be explained by editorial connections or peer effects in elite programs.

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1 Introduction

The 2025 One Big Beautiful Bill Act raises the federal tax on private university endowment income sharply. The top tax rate of 8% is likely to hit Harvard, MIT, Princeton, Stanford, and Yale, leading to steady-state annual tax bills that may exceed current tuition revenue for some of these schools. Estimated tax liabilities exceed $15,000 per student at four of the five high-tax schools.[1] These institutions play an outsized role in training economics faculty: their graduates account for over half of the full-time faculty employed by highly-rated economics departments (Amir and Knauff, 2008). Other schools with elite economics PhD programs also face substantial financial challenges arising from federal civil rights litigation and cuts to research funding. In response to the endowment tax and other fiscal pressures, Harvard has considered plans to cut PhD admissions in many programs by half (Mao and Paulus, 2025). MIT's institute-wide graduate admissions for Fall 2026 were down by 20% (Ryan, 2026). Admissions at fifty-five top research universities declined by 15% (Patel, 2026).

This paper uses novel data to gauge the extent to which admissions cuts by elite US PhD programs might harm the economics research enterprise. Specifically, we ask whether doctoral programs at high-tax and other elite schools produce more prolific scholars as measured by publications in widely-cited journals. As in the literature on K-12 school value-added (e.g., Angrist et al. (2024)), the parameters of interest are causal effects of attendance at one school or type of school versus another. In the spirit of the landmark Dale and Krueger (2002) and related studies of undergraduate college value-added, our identification strategy for causal value-added by graduate schools mitigates selection bias with controls for applicant ability and ambition.

Our data set contains information on the roughly 200 highest-ranked applicants to the MIT Economics PhD program in select application years from 1996-2019. We focus on research value-added as seen in the publication records of MIT applicants earning PhDs from eight elite US schools: Berkeley, Chicago, Harvard, MIT, Northwestern, Princeton, Stanford, and Yale. This includes the five most highly-taxed schools, plus three facing strong financial pressure.[2] These "elite eight" schools have the strongest placement records in most of the rankings considered in Amir and Knauff (2008). Together, they enroll roughly 75% of the MIT applicants in our data who earn PhDs. Our analysis compares the research output of elite-eight graduates to the output of graduates of other US and foreign schools.

The data analyzed here include MIT admissions committee ratings of top applicants, MIT admis-

[1] Schneider and Robinson (2025) estimates annual endowment tax liabilities ranging from $174 million at MIT to $368 million at Harvard. Princeton's 2025 net tuition revenue was $142 million; Harvard's tuition revenue has been reported at around $360 million (Shaw and Rosenberg, 2024). Princeton is hoping to avoid the endowment tax by increasing financial aid to bring the number of tuition-paying students below 3000 (David-Fox and Collins, 2026). Even if successful, this tax avoidance strategy entails a substantial reduction in revenue.

[2] Responding to large deficits exacerbated by research cuts, Chicago has moved to trim many graduate programs (Glick and Eisenstadt, 2025). Northwestern and the University of California face possible financial penalties imposed on anti-discrimination grounds. (U.S. Department of Education, 2025).

sions decisions, and information on whether candidates won NSF fellowships. Admissions data are matched to dissertation and research records drawn from the data analyzed in Angrist and Diederichs (2024). Our focal measure of research output is a full-career index heavily weighting top-journal publications. The MIT admissions committee's rating of applicant finalists affords fine-grained control for applicant preparation and aptitude for doctoral work. Dummies for MIT admits and NSF awardees augment our set of ability controls. Admissions ratings, MIT admission, and NSF award status—our "key controls"—are in the spirit of admission controls used to identify US college value-added in Dale and Krueger (2002, 2014), Chetty, Deming and Friedman (2023), and Mountjoy and Hickman (2021). We also observe the set of doctoral programs to which applicants applied for a subsample. As in Dale and Krueger (2002, 2014) and Mountjoy and Hickman (2021), this information offers a gauge of applicants' self-assessed commitment to and prospects for a research career.

Following this introduction, Section 2 highlights a number of novel descriptive statistics. First, within the set of highly ranked MIT applicants, uncontrolled comparisons show graduates' research output differs markedly across schools. For instance, MIT graduates publish roughly twice as much as Berkeley graduates, who publish about twice as much as non-elite graduates. Second, admissions committee ratings are a strong, though far from deterministic, predictor of research output. Applicants ranked in the top five in their application cohort publish roughly four times as much as applicants ranked 26-50, who publish roughly twice as much as applicants ranked 101-150. These findings set the stage for an analysis that controls for key applicant characteristics. Importantly, there is sufficient variation in doctoral program attended conditional on rank and other key variables for well-controlled estimates of value-added to be informative.

Motivated by the endowment tax, the analysis of value-added in Section 3 begins with comparisons of research value-added across groups of schools. Exponential regressions omitting admissions-rank controls show that high-tax schools graduate much more prolific scholars. Inclusion of controls for applicant ability reduces this estimated gap considerably, but still leaves sizable differences. High-tax schools boost their graduates' top-journal publications by about 75% relative to non-elite US schools. The high-tax advantage over foreign graduates is even larger. Differences in research output between high-tax and other elite graduates, however, are narrowed substantially by the inclusion of admissions-rank controls. Well-controlled estimates suggest that other elite eight schools have value-added similar to that of schools in the high-tax group. Roughly 40% of the elite school value-added premium is due to increased numbers of publications, with the rest due to a shift towards publications in more widely-cited journals. Estimates that control for possible omitted variables bias due to self-assessed prospects are similar to estimates that omit these controls.

Section 4 turns to possible explanations for cross-school differences in estimated value-added. First, we gauge the importance of peer effects. The opportunity to study with talented classmates is often

said to be a key feature of elite education at all levels.[3] If elite-school value-added stems entirely from the assembly of talented cohorts, and if the best students migrate to untaxed schools following tax-induced enrollment cuts, aggregate research output need not be reduced by an endowment tax. Our investigation of peer effects exploits idiosyncratic year-to-year within-school variation in peer composition. This analysis shows no relationship between peer quality (measured by the share of highly-ranked MIT applicants among an applicant's classmates) and research productivity. Value-added estimates are consequently unchanged when measures of peer composition are added to value-added models.

Section 4 also assesses the importance of home-journals as a contributor to elite graduates' publication success. Connections to local authors might lead the *Quarterly Journal of Economics* (*QJE*) to favor Harvard and MIT graduates, the *Journal of Political Economy* (*JPE*) to favor Chicago graduates, and the *American Economic Association Papers and Proceedings* (*P&P*) to favor graduates of elite PhD programs in general. Value-added estimates allowing for individual school effects are loosely consistent with home-journal advantage. For instance, Harvard and MIT graduates do especially well at the *QJE*. Value-added estimates omitting home journals nevertheless show a strong elite-school advantage. Some estimates of individual school value-added are sensitive to admissions-rank controls. In particular, gaps between Harvard and MIT (the two most selective schools) and other elite schools shrink considerably when estimated with controls. The overall elite advantage notwithstanding, differences in estimated value-added across the elite eight are not statistically significant. The paper concludes with a rough estimate of the elasticity of research output with respect to PhD enrollment at high-tax schools.

Our analysis contributes to a literature examining the determinants of PhD student success in economics. This includes Laband (1985) and Conley and Önder (2014), both of which compare research success across PhD graduates from a wide range of schools. Athey et al. (2007) explores the predictive power of in-school performance measures for post-PhD outcomes. Hilmer and Hilmer (2009, 2011) attempts to distinguish the effects of advisor scholarship from program prestige effects in a sample of economics PhDs. García-Suaza, Otero and Winkelmann (2020) does something similar using recently-available data from the Repec economics research paper repository. Angrist and Diederichs (2024) reports that only 60% of recent graduates of elite PhD programs succeed in publishing at least one paper in a set of 137 core journals, and that graduates of the elite eight are all about equally likely to have at least one publication. Our results align with these studies in showing that research success has a large unpredictable component, although it contrasts with most in finding that applicant ability is also important. Unlike earlier work on research productivity in economics, we aim to mitigate

[3]See, for instance, Abdulkadiroğlu, Angrist and Pathak (2014), Dobbie and Fryer (2014), and Ellison and Pathak (2025) for discussions of peer effects at elite high schools.

selection bias with well-controlled estimates of elite school value-added.

Much related research is concerned with selection bias in measures of institutional and firm quality in research and patenting. Studies tackling the problem of selection bias in research comparisons include Waldinger (2010, 2012), which use the expulsion of mathematicians and scientists from Nazi Germany to examine the effects of star researchers on PhD students and peers. In the same spirit, Azoulay, Graff Zivin and Wang (2010) examines the productivity consequences of unexpected deaths of research collaborators. Other work along these lines includes the Borjas and Doran (2012) analysis of the post-1992 migration of Russian mathematicians to the US; the Chandra and Xu (2025) analysis of the research productivity of medical researchers who move between institutions, and studies of patenting and firm productivity such as Azoulay, Ding and Stuart (2007) and Baviskar et al. (2025).

Finally, we contribute to the literature on the economics of talent in a range of fields. This includes work examining early-life predictors of exceptional long-run achievement, such as Lubinski and Benbow (2006), Agarwal and Gaule (2020), and Danieli et al. (2025). Our finding that the probability of research success is much larger for the very highest-ranked applicants has a similar flavor. Moreover, paralleling our results for young economists, Bell et al. (2019) shows that typically-unmeasured characteristics of inventors predict later-life patenting.

2 Data and Descriptive Statistics

2.1 Sample Construction and Variable Definitions

Our sample includes the highest-rated applicants in the 1996-1997, 2005-2010, and 2012-2019 MIT economics PhD application cohorts. At MIT, economics PhD applications are initially rated by one or two admissions committee members, with up to 200 finalists rated by additional faculty readers. Data for most years include ratings for 190-200 finalists, although finalist ratings for a few years are available for only the top 90-150 applicants. The applicants in our sample are very strong prospects for graduate school: they have outstanding grades in difficult college courses and many have perfect GRE scores. Some have extensive experience as research assistants and pre-doctoral fellows.

For purposes of statistical analysis, we average ratings across faculty readers, and convert this to within-cohort rank.[4] Applicant rank is the outcome of multiple independent evaluations made in an initial stage of MIT's admissions process. Ranks are not deterministically related to admission decisions, which are made through a process of committee discussion and review. Admissions committees generally aim to balance incoming classes across applicants' likely fields of interest, among other considerations. Admission decisions, which we observe, also reflect applicants' prospects for outside funding and department financial constraints.

[4] Tied applicants are assigned the midpoint of the rank range for their common score.

Data on applicants' PhD alma mater and graduation year are drawn primarily from EconLit dissertation records for American and Canadian PhDs, supplemented for elite eight graduates with the data collected for Angrist and Diederichs (2024). These data augment EconLit with information from ProQuest and economics department administrative records. For remaining applicants, school information comes from department websites and post-application CVs. We add publication data (through 2024) by matching applicants to EconLit using student names. We also matched NSF fellowship award data. Further details regarding data processing, such as solutions for common or ambiguous names, appear in the Data Appendix.

Our primary measure of research output is a weighted publication count that applies citation-based influence weights to publications in 137 prominent "deep impact" (DI) journals, giving each author $1/n$ credit for an n-authored publication. These influence weights, which are similar to those used in Angrist et al. (2020), are proportional to the frequency with which each journal is cited by top five journals.[5] The DI journal list is also based on Angrist et al. (2020). Journal influence weights are normalized so that the average article in a top five journal gets a weight of one. In practice, many citations in top five journals are to other top five outlets, so even prominent field and second-tier general-interest journals get substantially lower weights. The five most highly weighted non-top five journals are *AEJ: Applied* (0.44), *Journal of Economic Perspectives* (0.30), *Journal of Economic Theory* (0.28), *RAND Journal of Economics* (0.28), and *AEJ: Policy* (0.24).[6] The influence weighting scheme generates our *weighted DI* measure of research output.

We also estimate value-added for other measures of research output. These are a count of top five publications, an unweighted DI publication count, a count of papers published in any journal covered by EconLit, and a dummy indicating at least one publication in a journal indexed by EconLit. In view of the importance often placed on the top five, counts of articles placed in these journals are of special interest (Heckman and Moktan, 2020). *P&P* publications are not counted as *American Economic Review* publications. Unweighted productivity measures make no adjustment for joint authorship; all coauthors of a paper in the *AER*, for instance, get credit for one top five publication.

Most analyses use the full set of 1996-1997, 2005-2010, and 2012-2019 application cohorts. In view of the differing horizons for publication available for different cohorts, value-added estimates control for cohort effects. Descriptive Figures 1, 2, and A1 plot data for a 1996–2010 subsample in which all applicants who graduate have roughly eight post-PhD years to accumulate publications as of our 2024 EconLit snapshot.

[5] More precisely, influence weights are a weighted average of the ratio of the fraction of each top five journal's recent citations that go to each journal, divided by the number of recent articles in the cited journal. Weights for each top five journal come from a page-rank style formula. Top five journals are the *AER, Econometrica*, the *JPE, Review of Economic Studies*, and the *QJE*. A few analyses omit two top five journals and the *American Economic Association Papers and Proceedings*.

[6] A few new journals with short track records are assigned the average AEJ journal weight.

2.2 The Graduates of Top Graduate Programs

The training of top economists has long been highly concentrated. Amir and Knauff (2008) reports that, in 2006, 75% of tenure-track faculty in top ten US economics departments earned PhDs from one of our eight elite schools. Amir and Knauff (2008) also notes substantial differences in prestige placements within the elite eight. Harvard placed 78 PhD graduates in a top ten school for every 100 placed by MIT. Excluding Harvard, other elite eight schools place around 38 students in a top ten school for every 100 placed by MIT.[7] Differences in research success parallel these differences in placement rates. Within our sample of MIT finalists, elite-eight graduates achieve 2.3 times more weighted DI publications on average than do graduates of other schools.[8] Harvard graduates have 28% fewer weighted DI publications on average than MIT graduates. Graduates of the other six elites are, on average, 54% behind MIT.

The relative research success of elite-school PhDs is evident in data on the distribution of publications as well as in average publications. Figure 1 plots the distribution of weighted DI and top five publications for 1996-2010 MIT applicants who graduated from high-tax schools, other elite eight schools, other US schools, foreign schools, and for applicants who never graduated. PhD graduates of high-tax schools are more prolific than graduates of other schools on both the intensive and extensive margins. Differences in the fraction of US PhDs with at least two weighted DI publications are especially striking. About 20% of high-tax graduates reach this level, while only about 8% of those attending non-elite US schools do so. On the extensive margin, a little over 20% of high-tax graduates never publish in a deep impact journal, whereas almost 30% of non-elite-eight US graduates and nearly 40% of foreign graduates have no DI publications.

The relative publication performance of elite-eight graduates is even stronger when gauged by publications in the top five. The right panel of Figure 1 shows the distribution of top five articles placed by graduates in the same school groups used for the left side of the figure. A little over 40% of high-tax graduates publish at least once in the top five, while roughly 25% publish three or more articles in these highly selective journals. On upper-tail margins, the top five gap between high-tax graduates and other elite graduates is more pronounced than the gap for weighted DI publications. And graduates of all elites see top-five success much more often than do graduates of other US and foreign schools.

[7] These statistics are derived from the last column in Amir and Knauff (2008) Table 2. Raw Amir and Knauff (2008) placement counts in top ten US economics departments are 74 for MIT, 58 for Harvard, 170 for the other elite eight, and 101 for the forty-seven non-elite eight schools covered by Amir and Knauff (2008) rankings.

[8] Non-elite schools with the largest number of graduates in our data are Columbia, University of Pennsylvania, NYU, and Michigan. Roughly 25% of foreign graduates went to LSE.

2.3 Admissions Ratings and Research Outcomes

MIT admissions ratings strongly predict research productivity. This is documented in Table 1, which reports publication statistics for 1996-2019 applicants grouped by rank. Differences in research success are most pronounced for the highest-rank groups. Measured by weighted DI publications, applicants ranked 1-5 publish almost three times as much as applicants ranked 6-25. The output gradient flattens at lower ranks: applicants ranked 6-25 publish one-third more than applicants ranked 26-50, while applicants ranked 100-150 publish around 25% more than applicants ranked 151-200. The skewness of the overall publication distribution is reflected in the difference between median and mean publication variables shown in the last two columns of Table 1.

Two broader measures of research output characterized in the table—unweighted DI and all Econlit counts—are also strongly correlated with applicant rank. Statistics for these measures appear in the bottom two rows of Table 1. Importantly, however, *within-cohort* research success is an especially-steeply increasing function of rank. This is highlighted by the second row of Table 1, which reports the probability that an applicant's weighted DI publications are in the top 10 ranked by weighted DI success in their cohort (in our data). Over 40% of rank 1 to 5 applicants end up in the ex-post top 10. This falls to 11% of applicants in the next rank group, and to only 2% for the lowest-ranked group. The rank gradient in top five publications (shown in the third row of the table) roughly mirrors this pattern: the top five publication count for rank 1-5 applicants is roughly triple the top five count for applicants ranked 6-25, falling more gradually with rank thereafter.

The research-rank relationship results from both extensive and intensive margin correlations. The left panel of Figure 2 shows the fraction of 1996-2010 applicants within each rank bin with weighted DI publications in various ranges. Almost all applicants ranked 1-5 publish at least one article while a little over half produce two or more weighted DI publications. Among applicants in the next group down, 25% publish nothing while only 20% publish two or more. At the lower end of the applicant list, roughly half of applicants ranked 151-200 publish nothing in a DI journal. For applicants ranked 51-200, the share with two or more weighted DI publications is around 10%. Appendix Figure A1 extends this picture by plotting the complementary cumulative weighted DI distribution for applicants with ranks in various ranges. The distribution of weighted DI publications for rank 1-5 applicants has a very thick upper tail. The fraction of rank 1-5 applicants reaching 5+ weighted publications is five times the fraction of rank 6-25 applicants who do so.

The right side of Figure 2 documents especially pronounced differences in extensive and intensive margin success for top five publications. Nearly two-thirds of the highest-rank group place at least one article in a top five journal, and a little over half publish three or more in the top five. These

applicants are likely strong candidates for tenure in elite US departments and schools.[9] At the other end of the rank distribution, roughly three-fourths of applicants ranked 51 and higher fail to place a paper in the top five.

The relationship between applicant rank and research success notwithstanding, many lower-ranked applicants see considerable success. The roughly flat 10% probability of placing two or more weighted DI publications implies that the majority of those who go on to have at least two weighted DI publications were ranked 51-200. Similarly, statistics in the second row of Table 1 imply that the ex post top 10 will, on average, consist of about two rank 1-5 students, two rank 6-25 applicants, two rank 26-50 applicants, two rank 51-100 applicants, and two rank 101-200 applicants.[10]

Our investigation of research productivity uses MIT admissions rank to control for differences in applicant ability and preparedness in cross-school comparisons. The type of school attended is highly correlated with MIT admissions rank, though rank and school are far from deterministically linked. Figure 3, which presents the distribution of school types by admissions rank (breaking out MIT), bears this out. A little over half of the highest-rank group studied at MIT, with most of the rest enrolling at another high-tax school. The share attending high-tax schools declines at lower ranks, but remains non-negligible even for applicants ranked below 150. The shares graduating from other US schools and from foreign schools, along with the no-degree share, rise as rank declines. Importantly for purposes of value-added estimation, Figure 3 shows substantial overlap in the distribution of school types conditional on rank.

3 Value-Added Models

Endowment taxes and other financial pressures may lead affected schools to reduce PhD student enrollment. The research impact of such cutbacks depends in large part on whether high-tax and other elite schools have a causal effect on their PhD graduates' research success. Perhaps the relative research success of elite graduates reflects graduate student selection and sorting, rather than value-added. If so, enrollment declines at the elites can be offset by funneling top applicants to unaffected schools. The University of Michigan Economics Department, for instance, would likely welcome applicants displaced from Harvard to Ann Arbor.

We cast causal effects in an exponential value-added framework analogous to the linear value-added models that Angrist et al. (2017, 2024) and others use to assess the quality of K-12 schools. In our

[9] The median 1996-2010 applicant with at least two weighted DI publications has 20 total publications and 5 in top five journals.

[10] This calculation comes from the fact that, since the five applicants ranked 1-5 have a .43 probability of landing in the ex post top ten, the expected number of rank 1-5 applicants in the top ten is about 2. Similarly, the expected number of rank 6-25 applicants in the top ten is $.11 \times 20 \approx 2$, and so on.

version, a constant-effects exponential specification describes applicant i's potential research outcomes indexed by $k \in \{1, \ldots, K\}$ when they attend school $j \in \{1, \ldots, J\}$. An exponential specification is well-suited to non-negative outcome variables like publication counts. This model can be written:

$$Y_{ij}^k = e^{\beta_j^k + \varepsilon_{ij}^k}, \tag{1}$$

where the average of research outcome k for school j is defined as $e^{\beta_j^k} \equiv E[Y_{ij}^k]$ and proportional value-added relative to a reference group or school is $e^{\beta_j^k} - 1$. Error term ε_{ij}^k captures unobserved applicant characteristics that contribute to research success at school j. By definition of β_j, $e^{\varepsilon_{ij}^k}$ has expectation one.

Publications observed for applicant i equal the potential outcome for i at the school attended. Let D_{ij} denote an indicator equal to one if i attends school j. Observed outcomes are then linked to potentials by:

$$Y_i^k = \sum_j D_{ij} Y_{ij}^k = \left[e^{\sum_j D_{ij} \beta_j^k} \right] \nu_i^k, \tag{2}$$

where $\nu_i^k \equiv e^{\sum_j D_{ij} \varepsilon_{ij}^k}$. Because graduate school enrollment is not randomly assigned, uncontrolled comparisons of mean outcomes across schools are likely to reflect selection bias. This is manifest in mean-dependence of unobservables parameterized by ν_i^k on school attended. In other words, $E[\nu_i^k | D_i] \neq 1$, where vector $D_i = (D_{i1}, ..., D_{iJ})'$ identifies the school attended by applicant i.

We mitigate selection bias by conditioning on covariate vector, X_i, that includes measures of applicant aptitude, preparation, motivation, and ambition. Specifically, the set of controls includes the MIT admissions rating and admit status, NSF award status, and, for some cohorts, the schools to which each MIT applicant applied. Given this information, the key conditional independence assumption identifying causal value-added is:

$$E[\nu_i^k | X_i, D_i] = E[\nu_i^k | X_i] = e^{X_i' \gamma^k}, \tag{3}$$

where γ^k is a vector of coefficients conformable with X_i. This assumption identifies a causal conditional expectation function that can be written:

$$E[Y_i^k | X_i, D_i] = e^{\sum_j D_{ij} \beta_j^k + X_i' \gamma^k}, \tag{4}$$

in which the set of $\beta_j^k; j = 1, \ldots, J; k = 1, \ldots, K$ parameterizes proportional value-added. Although not reflected in the notation, we can allow value-added to depend on applicant characteristics, X_i, as well as on j and k.

Much of the analysis described below focuses on models of research value-added for graduates from

four groups of schools: high-tax, other elites, foreign schools, and other US schools, with the first three groups indicated by dummies T_{1i}, T_{2i}, and F_i, respectively. This specification replaces value-added for individual schools, $\sum_j D_{ij}\beta_j^k$, with

$$\alpha + \beta_1 T_{1i} + \beta_2 T_{2i} + \beta_f F_i,$$

where $\beta_1, \beta_2, \beta_f$ denote group average value-added and other-US schools are the omitted reference group. The high-tax dummy, T_{1i}, sums school dummies for MIT, Harvard, Stanford, Princeton, and Yale; high-tax value-added, β_{1i}, is the coefficient associated with this dummy. The other-elite dummy, T_{2i}, sums school dummies for Berkeley, Chicago, and Northwestern, with β_2 defined similarly. All specifications, whether grouped or for individual school value-added, control for graduation status, US citizenship or permanent residence, gender, and application-year dummies.

3.1 Estimates for High-Tax and Other Elite Eight Schools

Graduates of high-tax schools publish substantially more in higher-tier journals than do graduates of other groups of schools. The precisely-estimated high-tax effect of 0.88 on weighted DI publications, reported in the first column of Table 2, implies a 141% publication gap relative to non-elite US schools.[11] This is estimated in value-added models that include dummy variables indicating non-graduates, US citizenship or permanent residence, gender, and application year. We refer to this specification as "uncontrolled" because it omits measures of applicant aptitude and ambition. For top five publications, the estimated uncontrolled research gap of 1.12, shown in column 3 of Table 2, is even larger than that for weighted DI. Graduates of untaxed elite eight schools also produce more weighted DI and top five publications than do graduates of other US schools, with estimated exponential coefficients of 0.49 and 0.62, respectively.

The addition of controls for linear, quadratic, and reciprocal functions of rank, as well as for NSF awards and MIT admission status, knocks these effects down considerably. But value-added estimates for high-tax and other elite graduates nevertheless remain substantial for both weighted DI and top five output. Controlled weighted DI estimates imply 75% and 60% higher research output for high-tax and other elite PhDs, respectively. On the other hand, the corresponding estimates of elite value-added for unweighted DI counts, all EconLit counts, and an any-publication dummy, reported in columns 5-7 of Table 2, are markedly smaller than estimates for upper-tier publication measures.

Increased numbers of publications generate roughly 40% of estimated elite-school value-added, with the rest due to a shift towards publications in more widely-cited journals. This can be seen in

[11] This calculation uses the fact that exponential coefficient β_j^k implies a percentage increase in output relative to an omitted reference group equal to $e^{\beta_j^k} - 1$.

a comparison of coefficients on the high-tax and other-elite dummies in columns 2 and 6 of Table 2. Specifically, the estimates in column 6 are about 40% the size of those in column 2 (recall that our weighted DI index credits only DI journals, implicitly giving zero weight to other journals). The estimates in column 7, by contrast, suggest that elite schools add little value on the extensive publication margin. The coefficient of 0.12 for other-elite value-added is the only significant estimated effect on a dummy indicating any publication in EconLit.[12]

Differences in estimated value-added between the two subsets of elite schools are much reduced when estimated with controls for admissions rank, NSF awards, and MIT admit status. Column 2 of Table 2, for instance, shows high-tax and other-elite effects on weighted DI publications of 0.56 and 0.47, respectively. The corresponding comparison for top five output, reported in column 4, is 0.72 and 0.59. With one exception, p-values testing the null hypothesis that high-tax and other-elite value-added is the same, reported at the bottom of Table 2, are comfortably inframarginal when estimated with controls. The exception is for the estimated impact on a dummy indicating any publication in column 7, where the other-elite estimate is larger. The fact that tests of within-elite differences reject the null hypothesis of equality for models without controls but not for models with controls suggests that the raw publication gap between graduates of high-tax and other elite schools is due mainly to selection rather than causal effects.

The estimates in columns 2 and 4 of Table 2 strongly suggest that reciprocal rank is the most important mitigator of omitted variables bias (OVB) in estimates of high-tax value-added. Estimates of coefficients on linear and quadratic functions of admissions rank, as well as estimated coefficients on NSF and MIT admit dummies, are small and statistically insignificant in these and all other specifications reported in the table. Perhaps surprisingly, estimated value-added for other elites is much less sensitive to inclusion of rank controls than is the estimated high-tax effect. This likely reflects the fact that applicants attending Berkeley, Chicago, and Northwestern were ranked similarly to many attending well-regarded non-elite-eight schools such as Columbia, Penn, NYU, and Michigan.

3.2 Estimates with Alternative Controls

Well-controlled value-added estimates imply that high-tax and other elite school graduates produce more high-impact scholarship than do graduates of other schools. At the same time, the estimates with rank controls in Table 2 suggest there is relatively little to choose between the elites. With controls for MIT admissions rank and other indicators of applicant aptitude, estimates for high-tax and other elite schools are mostly similar, with only one marginally significant estimated difference in value-added between the two groups.

[12] Appendix Table A1 reports results from value-added models allowing school-group effects to vary by gender and US citizenship. These models generate no statistically significant interactions.

Estimates of high-tax value-added that control for a measure of applicants' self-assessed prospects and ambition provide a partial check on a causal interpretation of these findings. In the spirit of Dale and Krueger (2002), which controls for undergraduate application targets in estimates of effects of undergraduate college selectivity, we estimate high-tax and other-elite value-added in models that control for the list of schools to which MIT applicants applied (in addition to the controls used for the estimates in Table 2). The 85% of MIT applicants who also applied to an economics PhD program at Harvard may be particularly ambitious or confident, while the 41% who also applied to Northwestern may be strongly committed to an economics PhD, come what may. A list of target schools is available for most applicants in the 2013-2019 application cohorts. Applicants for whom this information is available applied to a total of 69 schools besides MIT.

Estimates controlling for application targets, reported in Table 3, are similar to estimates from specifications omitting these additional controls. The models generating these include the functions of rank and the other controls used for Table 2, though the table shows only estimated coefficients on high-tax, other-elite, and foreign school dummies. As in Dale and Krueger (2002), Table 3 labels dummies for target schools "self-revelation controls." Elite value-added estimates for weighted DI output with self-revelation controls, reported in column 2, are 0.68 for high-tax schools and 0.54 for other elites. These results can be compared with the corresponding estimates of 0.61 and 0.44 generated by models that omit self-revelation controls (reported in the first column of the table). Estimates of value-added for other research outcomes are even less sensitive to inclusion of self-revelation controls.

Reciprocal rank appears to be the linchpin of the conditional independence assumption that justifies a causal interpretation of the estimates in Tables 2 and 3. In support of this claim, Appendix Table A3 reports estimates of elite value-added from models with and without NSF and MIT admission dummies, computed using data for all available cohorts. The importance of these variables to applicants notwithstanding, they have little explanatory power; the associated coefficient estimates are small and not significantly different from zero, while estimated value-added is insensitive to their inclusion. This finding reinforces the conclusion that MIT admissions rank captures the most important confounding applicant characteristics related to research success. [13]

Finally, Appendix Table A3 reports results from an Oster (2019) coefficient stability analysis of elite value-added estimates for weighted DI publications. This analysis suggests bias from unobserved confounding must be from about 1.7 to as much as 4.3 times larger than the consequences of omitting observed controls for unobserved confounding to explain estimated elite school effects. The largest of these stability estimates comes from a model that maintains rank controls.

[13] Estimates omitting non-graduates (not reported) are also similar to those reported in in Tables 2 and 3.

4 Sources of Value-Added

Why do graduates of elite PhD programs outperform graduates from programs at other schools? This section considers two explanations of the elite advantage, the first driven by peer effects, the second a consequence of home-journal favoritism. Our analysis of peer effects is motivated by the fact that, in the face of reduced elite enrollment, research quality due to peer effects might be maintained by reallocating students to other schools. The home-journal investigation builds on a model of individual school value-added rather than school-group effects. Single-school value-added is also of interest in its own right.

4.1 Probing Possible Peer Effects

Idiosyncratic year-to-year variation in matriculation rates identifies the importance of peer composition for research success. In 2010, for instance, MIT attracted only two rank 1-10 applicants, whereas, in 2016, MIT attracted all of these.[14] Harvard went from enrolling four of MIT's top 25 applicants in 1996 to enrolling 10 in 1997, while Yale jumped from enrolling three rank 1-150 applicants in 2016 to enrolling 16 in 2017. This variation is encoded in variables measuring peer strength for an applicant in cohort c who graduated from school j. These measures are the fraction of rank 1-10 applicants in c enrolling at j; the fraction of rank 1-25 applicants in c enrolling at j; and a weighted share of (up to) the top 200 applicants in c enrolling at j, using inverse ranks as weights. The latter is prompted by our finding that reciprocal rank is highly predictive of eventual research success.[15]

In models omitting school dummies, peer quality is positively correlated with weighted DI and top five output, though, for the most part, not by much. For instance, estimates in the first and fourth columns of Panel A in Table 4 imply that a 0.10 increase in the top-ten share among classmates increases weighted DI by about 5% and top five journal output by 7%. The residual standard deviation of the peer quality variable here (from a regression on cohort and school effects) is 0.094, so 10 percentage points is a large increase in peer quality.

Because peer composition varies much more within schools than within school groups, peer effects are estimated including and omitting individual school dummies rather than dummies for groups of schools. When computed without school dummies, the estimated effects of other peer-quality variables on weighted DI and top five publications in Panels B and C exceed the estimates in Panel A and are at least marginally significantly different from zero. The estimates in Panel C are the largest. The weighted DI estimate in the first column of Panel C, for example, indicates that an increase in peer

[14] Due to ties, 11 applicants are coded as ranking 1-10 in 2016.

[15] The weighted strength-of-peers measure for student i is $p_i \equiv \frac{\sum_{i' \neq i, c(i')=c(i), j(i')=j(i)} 1/Rank(i')}{\sum_{i' \neq i, c(i')=c(i)} 1/Rank(i')}$. With fewer than 200 in a cohort, this measure weights the number of applicants available.

quality equal to the average difference between Harvard and Stanford (about 0.114) is associated with a roughly 11% increase in weighted DI publications. As can be seen in column 7 of Table 4, however, peer composition is essentially unrelated to a count of all EconLit publications.

The addition of school dummies to value-added models with peer quality variables largely eliminates any evidence of positive peer effects. Comparing, for example, peer effects on weighted DI publications in columns 1 and 3 of Table 4, the estimated coefficient on the share of top-ten classmates falls from marginally significant 0.52 to statistically insignificant 0.09. Estimated peer effects on top five output, reported in columns 4 and 6 of Table 4, likewise fall from marginally significant 0.65 to statistically insignificant 0.07. In Panel C of the table, declines in estimated effects of a weighted measure of peer quality are also precipitous.

Estimated coefficients on peer variables are markedly less precise when estimated in specifications with school dummies. Importantly, however, estimated elite-school value-added is insensitive to the inclusion of any of the three peer variables. For comparability with the school-group results shown in Table 2, effects of high-tax and other elite school attendance in Table 4 are enrollment-weighted averages of individual school coefficients. Estimated high-tax and other elite coefficients in columns 2, 5, and 8, from models omitting peer variables, differ little from the corresponding coefficient estimates in columns 2, 4, and 6 of Table 2. The estimated average high-tax and other-elite effects in column 3 and 6 of Table 4, from models including peer variables, are almost identical to the corresponding estimates in columns 2 and 5 from models without peer effects. These findings suggest features other than peer quality explain elite-school graduates' research success.

4.2 School-Specific Estimates and Journal Home Teams

The *Quarterly Journal of Economics* (*QJE*) home page describes the journal as "Edited at Harvard University's Department of Economics" and the *QJE* board of editors consists mostly of Harvard faculty, a connection that may benefit Harvard affiliates. Because MIT graduates are over-represented on the Harvard faculty and vice versa, MIT-affiliated authors might benefit from the Harvard-QJE connection as well (for many years, the *QJE* was published by MIT Press, though it's now published by Oxford University Press). Similarly, the *Journal of Political Economy* (*JPE*), edited by Chicago economics faculty and published by the University of Chicago Press, may favor PhD graduates with Chicago connections. Finally, the fact that elite faculty often serve as officers of the American Economic Association (AEA) and organize AEA annual meetings may help elite graduates score invitations to publish in the (non-peer-reviewed) *AEA Papers and Proceedings* (*P&P*).

Our estimates show that elite PhD graduates are especially likely to succeed at top journals. But elite success at the QJE, JPE, and P&P might be an inside job. The view that house journals afford elite scholars a home-team advantage is buttressed by analyses of editorial connections such

as Colussi (2018) and Fourcade, Ollion and Algan (2015), which argue that authors connected to journal editors as colleagues or PhD students benefit from these connections in the form of editorial favoritism. Carrell, Figlio and Lusher (2024) provide evidence of referee-connection benefits: *Journal of Human Resources* referees who are more connected to authors (including those with PhDs from the same school) give more favorable reviews. Using panel variation from editor rotations across 28 leading economics journals, Diederichs (2026) finds that connected-author publication rates rise during a connected editor's tenure, though the productivity bump spills over to similar journals—a pattern more consistent with editorial knowledge spillovers than with relaxed quality standards. Still, if connections rather than research quality drive elite graduates' relative publication success, the social costs of cutbacks at the elite eight are likely to be modest.

School-Specific Value-Added

As a foundation for an exploration of home journals, Table 5 reports estimates of school-specific value-added, reported in a format paralleling that of Table 2. These are from specifications analogous to those used to construct the estimates of value-added by tax group in Table 2, replacing school-group dummies with dummies for each of the elite eight schools. All models again include a foreign school dummy, while the reference group remains US schools other than the elite eight.

Without controls for admissions rank, NSF awards, and MIT admit status, weighted DI value-added estimates (reported in the first column of Table 5) are large and at least marginally significant for most elites; the exceptions are Yale and Chicago. In this uncontrolled specification, MIT, Harvard, and Princeton graduates are estimated to produce more weighted DI publications than other elite graduates. The pattern for uncontrolled top five value-added estimates (shown in column 3) is similar, though here the estimate for Chicago matches that for Berkeley and is significantly different from zero.

When estimated with admissions rank and other key controls, the MIT and Harvard advantage shrinks considerably. Specifically, the MIT estimate for top five publications falls from 1.44 in column 3 of Table 5 to 0.89 in column 4, while the Harvard estimate falls from 1.23 to 0.94. Interestingly, value-added estimates for schools other than MIT, Harvard, and Stanford are diminished little when estimated with rank controls. Figure 4 highlights this by plotting school-level value-added estimates against school selectivity as measured by the average MIT admissions rank assigned to each school's graduates. Without key controls, estimated school effects (plotted with blue dots) are highly correlated with selectivity, reflecting, in large part, high output and extreme selectivity at MIT and Harvard. In estimates with controls, the relationship between value-added and selectivity (plotted with red diamonds) is much flatter within the elite eight.

Estimated school-specific value-added for all-EconLit and any-publication outcome variables are mostly small and not significantly different from zero. These estimates, computed with key controls,

appear in columns 6-7 of Table 5. Estimated all-EconLit value-added is largest for Berkeley and Princeton, while Berkeley graduates are most likely to publish at least one paper. Reflecting the importance of journal impact, estimates for unweighted DI counts in column 5 are much smaller than the corresponding weighted DI estimates in the first column of the table. In impact on unweighted DI, Princeton again stands out.

With key controls, three of five highly-taxed schools are estimated to have the highest weighted DI and top five value-added. But weighted DI and top five value-added estimates are imperfectly aligned with endowment tax exposure. Discordant estimates include Yale, which is in the highest tax bracket but has the smallest estimated value-added, and Berkeley (untaxed) and Northwestern (taxed at a lower rate), with estimated weighted DI value-added a little above that for Stanford. Chicago grads are estimated to be a little ahead of Stanford grads when measured by papers in the top five. This might be taken to suggest that a large per-student endowment is insufficient to ensure high research value-added in economics PhD programs. It's worth noting, however, that chi-square tests for equality of well-controlled value-added for all elite eight fail to reject the hypothesis of equality (p-values appear at the bottom of the table). A marginal rejection for equality of top five value-added within the high-tax group (this p-value appears at the bottom of column 4) is driven by Yale. These results contrast with clear rejections of equality for uncontrolled estimates at the bottom of columns 1 and 3.

Local Favorites

Arguments suggesting elite scholars have a home-team advantage get some support from Table 6, which reports well-controlled value-added estimates for individual journals in columns 1-3. The estimates in column 1 show that Harvard and MIT graduates indeed do exceptionally well at the *QJE*. On the other hand, estimated effects on *JPE* publications, reported in column 2 of the table, are largest for Northwestern, Berkeley, and Chicago. This seems like less of a local success story than the *QJE*. Estimated effects on *P&P* publications, reported in column 3 of Table 6, are largest for MIT, Harvard, Berkeley, and Stanford. The *P&P* advantage appears to be correlated with school selectivity and is not uniform across the elite eight.

How much does this mixed evidence of home-team advantage matter for estimates of research value-added? This is assessed in column 5 of Table 6, which reports well-controlled estimates of value-added for a weighted DI outcome that excludes the *QJE*, *JPE*, and *P&P*. Exclusion of these three journals moderates MIT and Harvard estimates and, to a lesser extent, the estimate for Berkeley, relative to the corresponding estimated effects on all weighted DI journals in column 4 (the latter are copied from Table 5). In the same spirit, column 7 reports estimated value-added for a top three dependent variable, defined as the top five without the *QJE* and *JPE*. For comparison, estimated

effects on the full top five are repeated in column 6. The most notable change here is a fall in the estimate for Harvard, with smaller declines for MIT and Berkeley, and an increase for Stanford.

Overall, the estimates in columns 5 and 7 of Table 6 are consistent with the estimates in Tables 2 and 3 in showing higher value-added for graduates of elite eight schools, and for high-tax schools in particular. When tested jointly, estimated effects for high-tax schools are jointly significantly different from zero, while joint test results for all elite eight are at least marginally significant. These results are broadly consistent with Diederichs (2026), which attributes connected-author productivity gains primarily to editorial knowledge spillovers rather than to relaxed editorial standards, although there may be other favoritism channels not excluded by Table 6.

5 Summary and Conclusions

A handful of elite schools train most of the faculty hired by top economics departments, while elite-school graduates account for an outsized share of academic economics research. University endowment taxes and other financial pressures affecting the schools that train the most prolific and influential scholars therefore threaten to handicap American economic research. This concern motivates our analysis of publication patterns and causal value-added in economics graduate training.

Graduates of the elite eight publish more than do graduates of other US and foreign schools. Elite graduates also publish more in high-impact journals. While selection bias explains some of this gap, value-added estimates with controls for applicant ability, preparation, and ambition suggest much of the elite advantage is a causal effect. Much of the research gain associated with elite graduate training is inframarginal, that is, due to greater success in high-impact outlets. The case for a causal interpretation of these findings is bolstered by the fact that, conditional on functions of the rating assigned to top applicants by MIT admissions committees, the addition of other controls—including a list of target schools, MIT admit status, and NSF award status—changes estimated value-added little.

As a descriptive matter, the distribution of research success is highly skewed. Applicants ranked 1-5 in their entering cohort are much more likely than other applicants to be highly successful. The relationship between admissions ratings and research success flattens considerably for applicants ranked below 25. It's worth emphasizing, however, that many lower-ranked applicants publish well. This reflects the fact that, although admissions ratings predict research success with large, precisely estimated coefficients, much of the variance in research outcomes remains unexplained. Conditional on school, cohort, functions of admissions rank, MIT admissions status, and NSF award status, the pseudo R^2 for our value-added models (reported in Table 5) is at most 0.35 across outcomes.

The impact of PhD program cutbacks in response to higher taxes depends on the mechanisms that underlie differences in school value-added. Gains due to peer effects might be offset by shifting top

students to untaxed schools. Our analysis of peer effects, however, suggests peer composition matters little for graduates' research output. Endowment taxes likewise matter little for research productivity when elite publication success is due to home-journal favoritism. Yet we see substantial elite-school value-added in top journals that are not typically associated with particular departments. Our estimates also suggest, however, that large per-student endowments are not the only way to promote PhD graduates' research success. High-tax schools might benefit by emulating features of successful graduate programs at relatively low-endowment elite competitors like Berkeley and Northwestern.

A permanent 20% reduction in admissions at elite eight schools might decrease production of top-five-quality papers by about 5%. A simple back-of-the-envelope calculation producing this estimate considers a counterfactual where 20% of the students at each elite school are shifted to a non-top-eight US school. The estimates in Table 2 suggests this will reduce affected students' publications by about 50%.[16] Elite eight graduates contributed about half of the papers in top five journals in the period studied here.[17] This leads to a forecast reduction of about $0.2 \times 0.5 \times 0.5 = 5\%$. A slightly more sophisticated calibration incorporates two effects omitted from this counterfactual–that the most-highly-rated applicants are less likely to be displaced and that some displaced will not get a PhD. Our estimates suggest these effects are roughly offsetting.

The relationships documented here seem unlikely to be unique to economics. Schools targeted by the endowment tax host elite science and engineering departments; some of these, including engineering and science departments at MIT, have reduced PhD admissions sharply. If elite PhD programs in disciplines other than economics also have substantial value-added, then the endowment tax, along with cuts to the NSF and other sources of federal research funding, is likely to reduce long-run research output in these fields. In answer to the question, "Will the university endowment tax (along with other research funding cuts) slow scientific progress?", the estimates reported here suggest there is good reason to think so.

[16] An estimate obtained as a weighted average of high-tax and other-elite treatment effects: $\frac{5}{8}e^{-0.72} + \frac{3}{8}e^{-0.59} \approx 0.51$.

[17] Students in our 1996 and 1997 top 200 applicant cohorts who attended elite eight programs published a total of 99 and 146 (coauthorship adjusted) top five publications by 2024, respectively. Estimates in Hamermesh (2025) suggest that this group will eventually published about 25% more, bringing the per-cohort average to about 154. Over the past 25 years, top five journals have published an average of 328 papers per year. This leads to a top five share of $154/328 = 0.47$.

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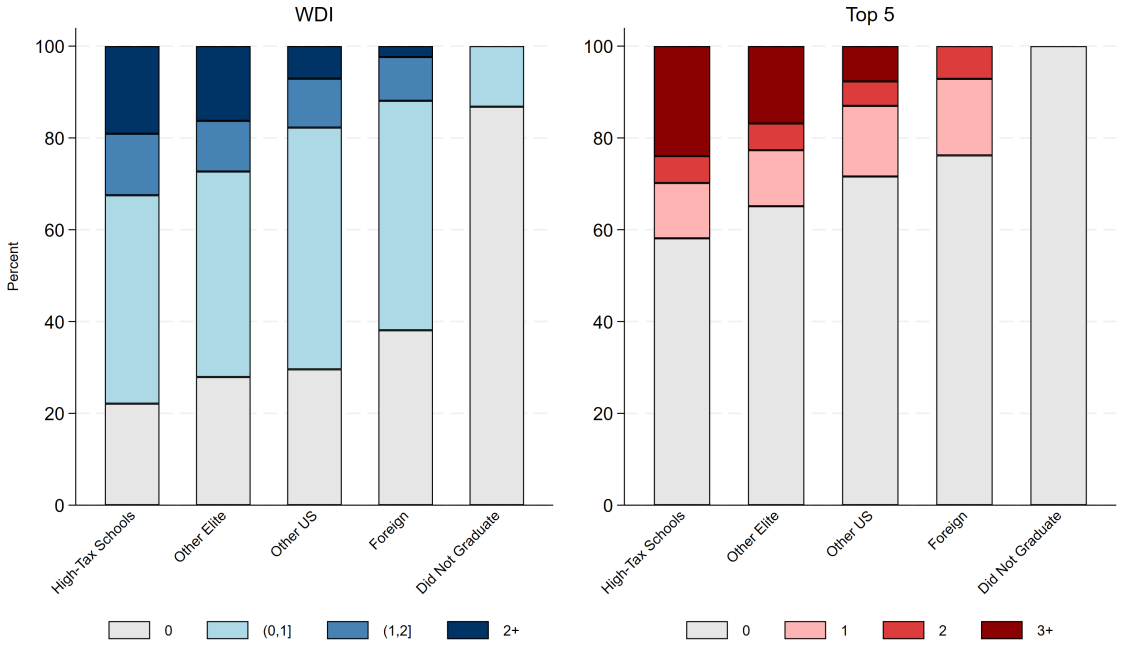
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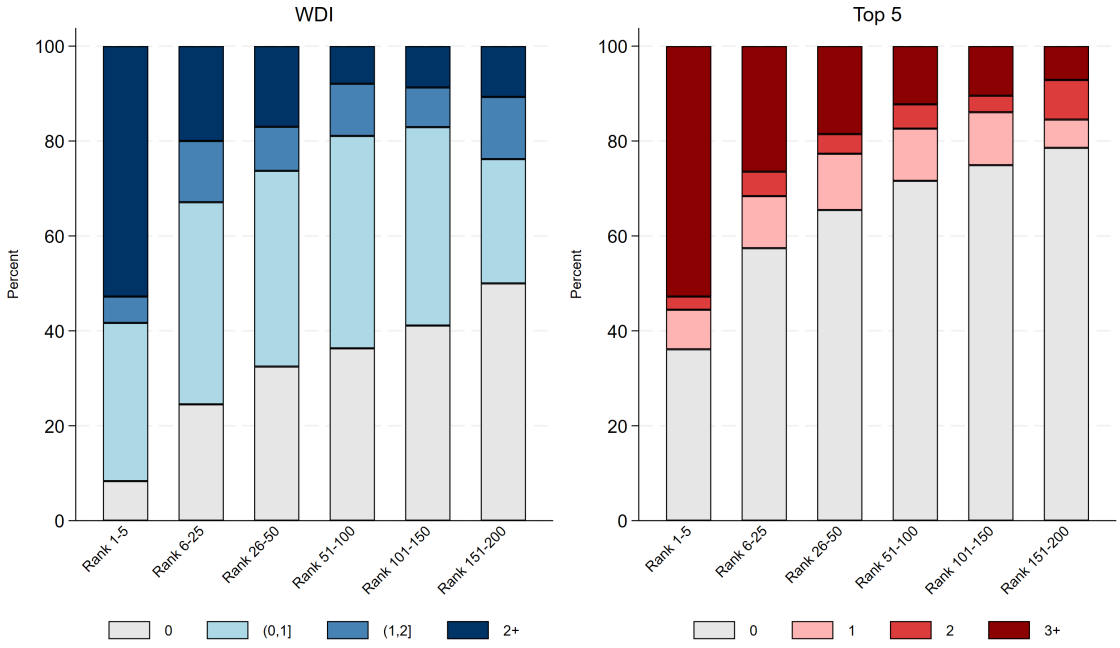
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Figure 1: Publication Distribution by School Type: Weighted DI and Top 5



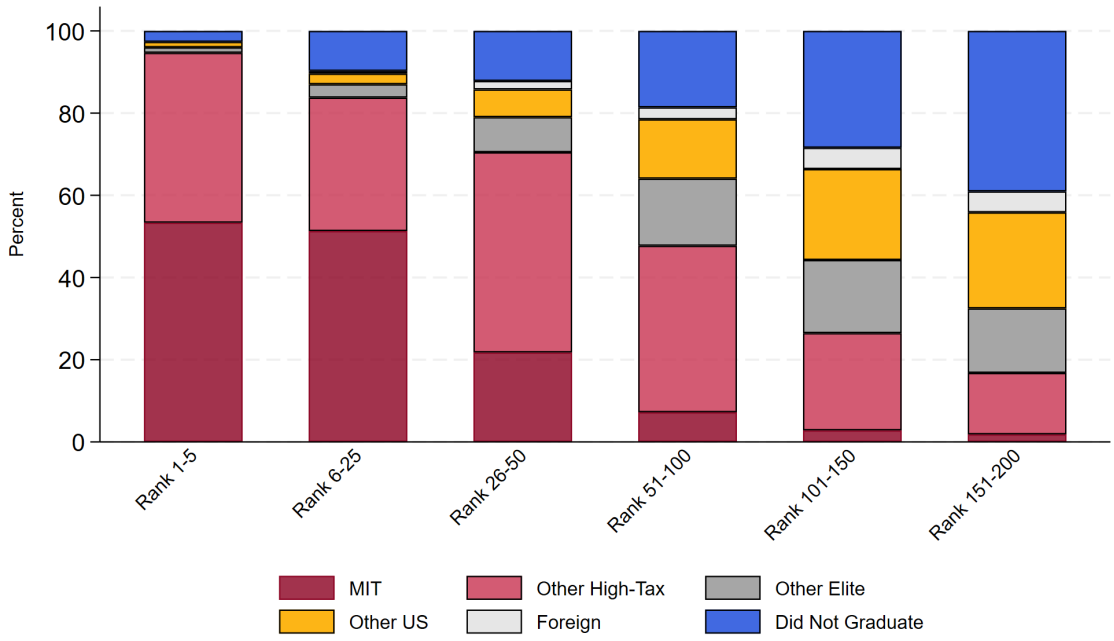
Notes: The bars in this figure show the distribution of publications for high-tax graduates (MIT, Harvard, Stanford, Princeton, and Yale), other elite graduates (Chicago, Northwestern, and Berkeley), other US graduates, foreign graduates, and applicants who did not graduate. The left panel displays coauthorship- and journal-influence-weighted publications (called weighted DI in the text); the right panel displays distributions of top five journal publications. The left panel groups weighted DI publications (a continuous variable) into bins 0, $(0, 1]$, $(1, 2]$, and 2+; the right panel groups top five counts into 0, 1, 2, and 3+. The sample is limited to 1996–2010 application cohorts.

Figure 2: Publication Distribution by Evaluation Rank: Weighted DI and Top 5



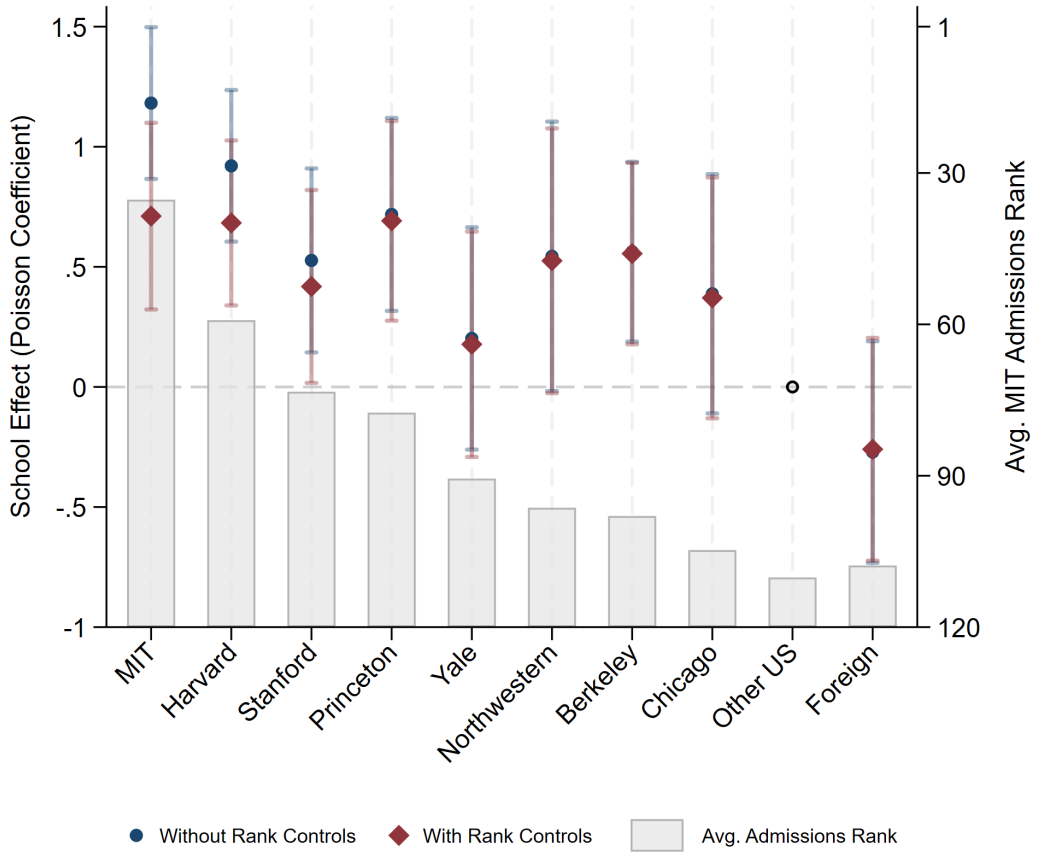
Notes: The bars in this figure show the distribution of publications across six evaluation rank groups (1–5, 6–25, 26–50, 51–100, 101–150, 151–200). The left panel displays coauthorship- and journal-influence-adjusted publications (called weighted DI in the text); the right panel displays distributions of top five journal publications. The left panel groups weighted DI publications into bins 0, $(0, 1]$, $(1, 2]$, and 2+; the right panel groups top five counts into 0, 1, 2, and 3+. The sample includes 1996–2010 application cohorts.

Figure 3: Graduate School Attended by Admissions Rank



Notes: This figure charts the distribution of graduate school attendance across six admissions rank groups (1–5, 6–25, 26–50, 51–100, 101–150, 151–200). MIT is separated from other high-tax schools (Harvard, Stanford, Princeton, and Yale). Other elite eight schools are Chicago, Northwestern, and Berkeley. Remaining school groups are: other US, foreign, and did not graduate. The sample includes application cohorts through 2019.

Figure 4: School Value-Added by Selectivity



Notes: This figure plots exponential regression estimates of individual school weighted DI value-added from Table 5. The reference category is other US. Schools are ordered from left to right by the average MIT admissions rank of their graduates (most selective first); gray bars mark average rank (on the right axis, reversed so that lower ranks appear as taller bars). Vertical lines show 95% confidence intervals based on robust standard errors. The sample includes application cohorts through 2019. $N = 2{,}582$.

Table 1: Publications by MIT Admissions Rank

	Mean Publications by Rank							
	1-5	6-25	26-50	51-100	101-150	151-200	Mean	Median
Coauthorship- and impact-adjusted pubs (WDI)	2.03	0.72	0.54	0.36	0.28	0.22	0.45	0.01
Fraction in top 10 of cohort (ranked by WDI)	0.43	0.11	0.08	0.04	0.04	0.02	0.06	0.00
Top 5 Count	3.44	1.10	0.82	0.53	0.38	0.22	0.66	0.00
DI Count	9.31	3.95	3.47	2.56	2.25	2.34	2.99	1.00
All Econlit	11.87	5.08	4.84	3.89	3.59	4.43	4.43	1.00

Notes: This table shows publication statistics for application cohorts through 2019 and publications through 2024. The first six columns contain means by rank bin; the last two columns give overall sample means and medians. The first row reports weighted DI (coauthorship- and journal-influence-adjusted) publications. The second row shows the fraction of applicants in each bin in the top 10 in their application-year cohort when ranked by weighted DI. Remaining rows report publication counts in top five journals, DI journals, and all EconLit. $N = 2{,}582$.

Table 2: High-Tax and Elite Value-Added

	WDI		Top 5		DI	All Econlit	Any Pub
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
High Tax School	0.88***	0.56***	1.12***	0.72***	0.34***	0.22**	0.04
	(0.14)	(0.15)	(0.18)	(0.20)	(0.11)	(0.10)	(0.04)
Other Elite	0.49***	0.47***	0.62***	0.59***	0.26**	0.20*	0.12**
	(0.17)	(0.17)	(0.23)	(0.23)	(0.12)	(0.11)	(0.05)
Foreign School	-0.27	-0.26	-0.36	-0.35	0.08	0.31	-0.02
	(0.24)	(0.24)	(0.33)	(0.33)	(0.23)	(0.29)	(0.09)
Admitted to MIT		0.16		0.22	0.08	0.04	0.05
		(0.12)		(0.14)	(0.10)	(0.09)	(0.04)
NSF Winner		0.13		0.10	0.10	0.07	0.05
		(0.12)		(0.14)	(0.10)	(0.09)	(0.04)
1/Rank		1.70***		1.78***	1.23***	1.23***	0.25***
		(0.24)		(0.26)	(0.24)	(0.23)	(0.09)
Rank/100		-0.53		-0.39	-0.43	-0.33	-0.10
		(0.35)		(0.43)	(0.29)	(0.28)	(0.13)
$(\text{Rank}/100)^2$		0.19		0.06	0.24	0.23	0.06
		(0.18)		(0.23)	(0.15)	(0.15)	(0.07)
Dep. var. mean	0.45	0.45	0.66	0.66	2.99	4.43	0.63
p-value High Tax = Other Elite	0.003	0.528	0.002	0.434	0.372	0.799	0.060

Notes: This table reports estimated exponential regression coefficients for regressions of publication outcomes on indicators for high-tax, other elite eight, and foreign school graduates, with other US schools as the omitted category. All specifications control for graduation status, US citizenship or permanent residence, gender, and application-year dummies. Estimates in columns 1-2 are effects on weighted DI publications; estimates in columns 3-4 are effects on top five publications. Estimates in columns 2, 4, and 5-7 are from models that include MIT admissions rank controls, MIT admission status, and a dummy indicating NSF fellowship receipt. The sample includes application cohorts through 2019. p-values at the bottom of the table test whether the high-tax and other elite-eight coefficients are equal. Robust standard errors appear in parentheses. $N = 2{,}582$.

Table 3: High-Tax Value-Added with Self-Revelation Controls (2013-2019 applicants)

	WDI		Top 5		DI		All Econlit	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
High Tax School	0.61***	0.68***	0.92***	0.99***	0.26*	0.29*	0.13	0.12
	(0.23)	(0.22)	(0.33)	(0.35)	(0.16)	(0.16)	(0.14)	(0.14)
Other Elite	0.44	0.54**	0.81**	0.82**	0.14	0.16	-0.01	-0.04
	(0.27)	(0.26)	(0.37)	(0.37)	(0.16)	(0.16)	(0.14)	(0.15)
Foreign School	0.31	0.43	0.81	0.93	0.82*	0.85*	1.04*	0.99**
	(0.37)	(0.35)	(0.61)	(0.62)	(0.50)	(0.46)	(0.62)	(0.46)
Self-Revelation Controls		✓		✓		✓		✓
Dep. var. mean	0.12	0.12	0.18	0.18	0.95	0.95	1.49	1.49
p-value High Tax = Other Elite	0.491	0.583	0.681	0.533	0.356	0.339	0.320	0.277

Notes: This table examines the sensitivity of value-added estimates to inclusion of self-revelation controls. The sample is limited to the 2013–2019 application cohorts. All specifications control for graduation status, US citizenship or permanent residence, gender, and application-year dummies as well as the admissions rank and related controls used for columns 2, 4, and 5-7 in Table 2. Results from models omitting self-revelation controls appear in odd-numbered columns; results in even-numbered columns are from models adding adding indicating non-MIT programs to which applicants applied, along with a dummy for applicants who did not report other program applications. Robust standard errors appear in parentheses. $N = 1{,}262$.

Table 4: Peers or Schools?

	WDI			Top 5			All EconLit		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Top 10 Peers									
Share Top 10 peers	0.52**		0.09	0.65**		0.07	-0.12		-0.28
	(0.25)		(0.40)	(0.28)		(0.44)	(0.18)		(0.28)
High Tax School		0.59***	0.56***		0.75***	0.73***		0.19*	0.26**
		(0.16)	(0.18)		(0.20)	(0.23)		(0.11)	(0.13)
Other Elite		0.48***	0.48***		0.61***	0.61***		0.20*	0.20*
		(0.17)	(0.17)		(0.23)	(0.23)		(0.11)	(0.11)
p-value High Tax = Other Elite	.	0.458	0.627	.	0.402	0.542	.	0.960	0.563
Panel B: Top 25 Peers									
Share Top 25 peers	0.60*		0.00	0.67*		-0.26	-0.17		-0.70
	(0.31)		(0.63)	(0.36)		(0.72)	(0.22)		(0.46)
High Tax School		0.59***	0.59***		0.75***	0.81***		0.19*	0.35**
		(0.16)	(0.20)		(0.20)	(0.25)		(0.11)	(0.15)
Other Elite		0.48***	0.48***		0.61***	0.61***		0.20*	0.20*
		(0.17)	(0.17)		(0.23)	(0.23)		(0.11)	(0.11)
p-value High Tax = Other Elite	.	0.458	0.578	.	0.402	0.362	.	0.960	0.272
Panel C: Top 200 Peers (Reciprocal Weighted)									
Share Wgt. Top 200 peers	0.95**		0.42	1.12**		0.22	-0.10		-0.38
	(0.40)		(0.71)	(0.47)		(0.80)	(0.29)		(0.53)
High Tax School		0.59***	0.51**		0.75***	0.71***		0.19*	0.26*
		(0.16)	(0.20)		(0.20)	(0.25)		(0.11)	(0.15)
Other Elite		0.48***	0.48***		0.61***	0.60***		0.20*	0.20*
		(0.17)	(0.17)		(0.23)	(0.23)		(0.11)	(0.11)
p-value High Tax = Other Elite	.	0.458	0.865	.	0.402	0.625	.	0.960	0.636
Dep. var. mean	0.45	0.45	0.45	0.66	0.66	0.66	4.43	4.43	4.43

Notes: This table gauges peer effects on graduate research productivity. The peer-share for student i equals the number of top-n MIT applicants at the same school and cohort (excluding i), divided by n if i is outside the top n or by $n-1$ if i is inside. Estimates in Panels A and B use top-10 and top-25 peers, respectively. Estimates in Panel C use a weighted share of the top 200 applicants in c enrolling at j, using inverse ranks as weights. Higher-ranked peers receive greater weight in Panel C. All specifications control for graduation status, US citizenship or permanent residence, gender, and application-year dummies as well as the admissions ratings and other controls used for columns 2, 4, and 5-7 in Table 2. Reported "High Tax School" and "Other Elite" estimates in columns 2-3, 5-6, and 8-9 are enrollment-weighted averages of individual school coefficients, where the weights are within-group enrollment shares; standard errors for these estimates are computed using *lincom*. p-values at the bottom of the table test whether the high-tax and other elite-eight coefficients are equal. The sample includes application cohorts through 2019. Robust standard errors appear in parentheses. $N = 2{,}582$.

Table 5: Value-Added by School

	High Tax	WDI		Top 5		DI	All Econlit	Any Pub
		(1)	(2)	(3)	(4)	(5)	(6)	(7)
MIT	✓	1.18***	0.71***	1.44***	0.89***	0.28*	0.14	0.05
		(0.16)	(0.20)	(0.20)	(0.25)	(0.15)	(0.15)	(0.06)
Harvard	✓	0.92***	0.68***	1.23***	0.94***	0.35***	0.20	0.02
		(0.16)	(0.18)	(0.21)	(0.22)	(0.13)	(0.12)	(0.05)
Stanford	✓	0.53***	0.42**	0.77***	0.62**	0.28*	0.14	0.03
		(0.20)	(0.20)	(0.25)	(0.26)	(0.15)	(0.14)	(0.06)
Princeton	✓	0.72***	0.69***	0.80***	0.74***	0.53***	0.41***	0.07
		(0.20)	(0.21)	(0.26)	(0.27)	(0.14)	(0.13)	(0.06)
Berkeley		0.56***	0.56***	0.67***	0.66***	0.36**	0.34**	0.18***
		(0.19)	(0.19)	(0.24)	(0.24)	(0.15)	(0.14)	(0.06)
Yale	✓	0.20	0.18	0.14	0.10	0.20	0.17	0.08
		(0.24)	(0.24)	(0.29)	(0.30)	(0.17)	(0.15)	(0.07)
Northwestern		0.54*	0.53*	0.45	0.42	0.23	0.07	0.06
		(0.29)	(0.28)	(0.38)	(0.38)	(0.19)	(0.16)	(0.08)
Chicago		0.39	0.37	0.68**	0.66**	0.15	0.11	0.07
		(0.25)	(0.26)	(0.32)	(0.32)	(0.16)	(0.14)	(0.07)
Foreign School		-0.27	-0.26	-0.37	-0.35	0.08	0.31	-0.02
		(0.24)	(0.24)	(0.33)	(0.33)	(0.23)	(0.29)	(0.09)
Rank/Admission Controls			✓		✓	✓	✓	✓
Dep. var. mean		0.45	0.45	0.66	0.66	2.99	4.43	0.63
Pseudo R^2		0.265	0.292	0.277	0.309	0.340	0.345	0.046
p-value all 8 equal		0.000	0.330	0.000	0.116	0.505	0.410	0.187
p-value high-tax 5 equal		0.000	0.122	0.000	0.032	0.383	0.378	0.759
p-value high-tax ex Yale 4 equal		0.001	0.443	0.002	0.489	0.406	0.266	0.727

Notes: Estimates in this table are from models paralleling the exponential regression models used for Table 2, replacing school-group indicators with individual school dummies. Other US schools remains the omitted category. Controls and dependent variables are as for Table 2. The sample includes application cohorts through 2019. Robust standard errors appear in parentheses. $N = 2{,}582$.

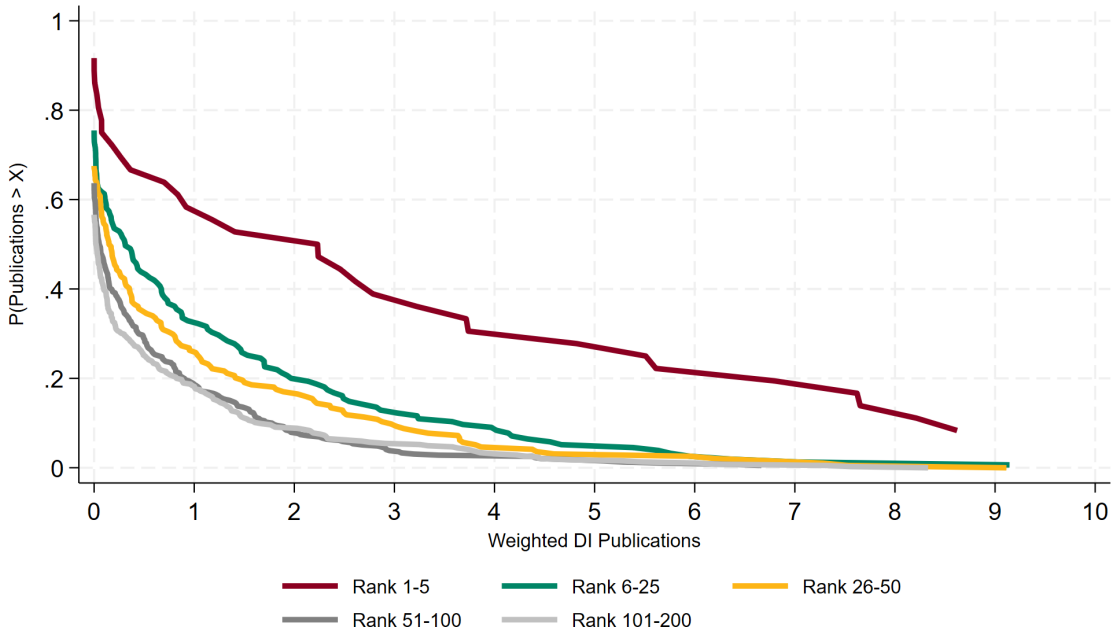
Table 6: Value-Added by School—Home Journal Outcomes

					With and Without Insider Journals			
	High Tax	QJE	JPE	AEA P&P	WDI	WDI ex 3	Top 5	Top 3
		(1)	(2)	(3)	(4)	(5)	(6)	(7)
MIT	✓	1.52***	0.25	0.95***	0.71***	0.53***	0.89***	0.81***
		(0.37)	(0.38)	(0.27)	(0.20)	(0.19)	(0.25)	(0.26)
Harvard	✓	1.62***	0.64**	0.90***	0.68***	0.46***	0.94***	0.77***
		(0.33)	(0.32)	(0.24)	(0.18)	(0.17)	(0.22)	(0.23)
Stanford	✓	-0.12	0.36	0.64**	0.42**	0.47**	0.62**	0.77***
		(0.49)	(0.36)	(0.27)	(0.20)	(0.21)	(0.26)	(0.28)
Princeton	✓	1.00**	0.60	0.51	0.69***	0.65***	0.74***	0.71***
		(0.42)	(0.41)	(0.32)	(0.21)	(0.20)	(0.27)	(0.27)
Berkeley		0.82*	0.82**	0.72**	0.56***	0.42**	0.66***	0.58**
		(0.43)	(0.40)	(0.32)	(0.19)	(0.19)	(0.24)	(0.26)
Yale	✓	-0.39	0.69	-0.24	0.18	0.16	0.10	-0.04
		(0.66)	(0.44)	(0.37)	(0.24)	(0.26)	(0.30)	(0.35)
Northwestern		-0.26	1.06**	0.09	0.53*	0.47*	0.42	0.31
		(0.81)	(0.49)	(0.45)	(0.28)	(0.24)	(0.38)	(0.37)
Chicago		0.83*	0.81*	0.40	0.37	0.25	0.66**	0.57*
		(0.43)	(0.42)	(0.33)	(0.26)	(0.25)	(0.32)	(0.34)
Foreign School		-1.49	-0.39	-2.06**	-0.26	-0.17	-0.35	-0.19
		(1.03)	(0.65)	(1.02)	(0.24)	(0.24)	(0.33)	(0.37)
Dep. var. mean		0.14	0.09	0.18	0.45	0.32	0.66	0.43
p-value all 8 = 0					0.005	0.053	0.002	0.022
p-value high-tax 5 = 0					0.001	0.014	0.000	0.004

Notes: This table reports estimated exponential regression coefficients for regressions of publication outcomes on individual school dummies. Controls are as for columns 2, 4, and 5-7 in Table 2. Columns 4 and 6 repeat columns 2 and 4 in Table 2. Columns 1-3 report estimated individual school value-added for publication counts at the *QJE*, *JPE*, and *AEA Papers & Proceedings*, respectively (publications in the *AEA P&P* are not counted as top five). Column 5 reports estimates for a weighted DI dependent variable that excludes *QJE*, *JPE*, and *AEA P&P* publications. Column 7 reports estimates for a top three dependent variable that omits the *QJE* and *JPE* from the top five. The sample includes application cohorts through 2019. Robust standard errors appear in parentheses. $N = 2{,}582$.

Appendix

Figure A1: Complementary Cumulative CDF for Weighted DI by Admissions Rank



Notes: This figure shows the complementary cumulative distribution function (1-CDF) of weighted DI publications for five admissions rank groups. For each rank group g and each weighted DI publication count x, the y-axis gives the fraction of applicants in group g with more than x weighted DI publications. For example, the rank 1–5 curve at $x = 2$ is about 0.5, meaning that roughly half of rank 1–5 applicants produce more than 2 weighted DI publications. The figure shows line segments connecting consecutive fractions at every observed weighted DI value x in group g, without smoothing. The sample includes 1996–2010 application cohorts.

Table A1: High-Tax Value-Added: Heterogeneity by Gender and Citizenship

	WDI		Top 5		DI	All Econlit	Any Pub
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Interactions with Female							
High Tax School	0.97***	0.64***	1.19***	0.79***	0.35***	0.23**	0.05
	(0.16)	(0.17)	(0.21)	(0.23)	(0.12)	(0.11)	(0.05)
Other Elite	0.58***	0.56***	0.70***	0.67***	0.27**	0.21*	0.13**
	(0.19)	(0.19)	(0.26)	(0.26)	(0.13)	(0.12)	(0.05)
Female	0.03	0.05	-0.09	-0.07	-0.18	-0.12	0.01
	(0.25)	(0.25)	(0.32)	(0.33)	(0.16)	(0.18)	(0.07)
High Tax × Female	-0.36	-0.34	-0.32	-0.30	-0.01	-0.04	-0.01
	(0.29)	(0.28)	(0.36)	(0.36)	(0.19)	(0.19)	(0.08)
Other Elite × Female	-0.43	-0.46	-0.40	-0.44	-0.09	-0.08	-0.07
	(0.36)	(0.36)	(0.44)	(0.44)	(0.25)	(0.26)	(0.11)
Panel B: Interactions with US Citizen/Perm. Resident							
High Tax School	0.88***	0.56***	1.11***	0.72***	0.34***	0.22**	0.05
	(0.14)	(0.16)	(0.19)	(0.20)	(0.11)	(0.11)	(0.04)
Other Elite	0.49***	0.47***	0.64***	0.60**	0.26**	0.21*	0.13***
	(0.18)	(0.18)	(0.23)	(0.24)	(0.12)	(0.11)	(0.05)
US Citizen/Perm. Resident	0.31	0.26	0.26	0.23	0.13	-0.16	0.21*
	(0.37)	(0.37)	(0.49)	(0.50)	(0.25)	(0.27)	(0.12)
High Tax × US Cit./Perm. Res.	0.14	0.05	0.18	0.05	-0.03	-0.08	-0.09
	(0.39)	(0.38)	(0.51)	(0.51)	(0.24)	(0.21)	(0.13)
Other Elite × US Cit./Perm. Res.	-0.01	-0.01	-0.47	-0.47	-0.02	-0.20	-0.07
	(0.58)	(0.58)	(0.72)	(0.72)	(0.29)	(0.23)	(0.16)
Rank/Admissions Controls		✓		✓	✓	✓	✓

Notes: This table reports results from models that add interaction terms to the models used for Table 2. Panel A shows results interacting school-group dummies with a female dummy; Panel B shows results interacting school-group dummies with a dummy indicating US citizens or permanent residents. Models parallel those used for Table 2. Dependent variables are also as for Table 2. The sample includes application cohorts through 2019. Robust standard errors appear in parentheses. $N = 2{,}582$.

Table A2: Sensitivity to MIT Admission and NSF Controls

	WDI		Top 5		DI		All Econlit	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
High Tax School	0.56***	0.60***	0.72***	0.78***	0.34***	0.36***	0.22**	0.23**
	(0.15)	(0.15)	(0.20)	(0.20)	(0.11)	(0.10)	(0.10)	(0.10)
Other Elite	0.47***	0.49***	0.59***	0.61***	0.26**	0.27**	0.20*	0.20*
	(0.17)	(0.17)	(0.23)	(0.23)	(0.12)	(0.12)	(0.11)	(0.11)
Foreign School	-0.26	-0.26	-0.35	-0.35	0.08	0.08	0.31	0.31
	(0.24)	(0.24)	(0.33)	(0.33)	(0.23)	(0.23)	(0.29)	(0.29)
Admitted to MIT	0.16		0.22		0.08		0.04	
	(0.12)		(0.14)		(0.10)		(0.09)	
NSF Winner	0.13		0.10		0.10		0.07	
	(0.12)		(0.14)		(0.10)		(0.09)	
Dep. var. mean	0.45	0.45	0.66	0.66	2.99	2.99	4.43	4.43
p-value High Tax = Other Elite	0.528	0.412	0.434	0.314	0.372	0.303	0.799	0.738

Notes: This table examines sensitivity of tax-group value-added estimates to the inclusion of dummies for MIT admission and NSF awards as controls. All models include MIT admissions rank and other controls as for Table 2. Dependent variables are also as for Table 2. The sample includes application cohorts through 2019. Robust standard errors appear in parentheses. $N = 2{,}582$.

Oster 2019 Coefficient Stability Analysis

Following Oster (2019), Appendix Table A3 explores sensitivity of elite value-added estimates for weighted DI publications to unobserved confounding by benchmarking OVB against the sensitivity of value-add estimates to inclusion of observed controls. The Oster (2019) coefficient stability analysis exploits linear-model algebra. Our implementation therefore uses OLS estimates of elite value-added in a regression of WDI on an any-elite indicator plus the controls used to compute the value-added estimates in column 2 of Table 2. OLS value-added estimates are close to Poisson average marginal effects evaluated at the mean. We add GRE scores (quantitative, verbal, and analytical) to the set of holdout observable covariates; these are available for 63% of the applicant sample. Missing scores are coded as zero and a dummy added for missing.

Our stability analysis compares baseline estimates of any-elite value-added computed with a set of maintained controls, W_0, to estimates obtained with a set of holdout controls, W_1. Changes to the estimated treatment effect ("observed OVB") and in the R^2 obtained when including W_1 generate stability parameter δ. Given a bound on the R^2 for the long regression that includes unobservables (denoted R_{max}), this parameter measures the ratio of OVB from omitted unobserved confounders relative to the OVB from omitting observed controls that is required to drive the elite coefficient to zero. As suggested by Oster (2019), we set $R_{max} = \min\{1.3\tilde{R}^2, 1\}$. Estimates are typically deemed robust to unobserved confounding when $\delta > 1$.

Table A3 shows stability estimates for the following three allocations of variables to W_0 and W_1:

Assignment of controls	A	B	C
NSF winner; MIT admission; GRE percentiles (Quant., Verbal, Analytic)	W_1	W_1	W_1
Admissions rank (1/Rank, Rank/100, (Rank/100)^2)	W_1	W_1	W_0
US citizen/perm. resident; Female; Foreign school	W_1	W_0	W_0
Graduated; application-cohort dummies	W_0	W_0	W_0

The first of these maintains only cohort and graduation dummies in W_0. The resulting baseline R^2 is 0.139. Since the set of holdout controls includes reciprocal rank, the estimated elite effect is sensitive to their inclusion, falling from 0.34 to 0.23. The associated stability parameter exceeds one even so; in this case, $\delta = 1.58$. Moving dummies for citizenship, female, and foreign school attendance into the set of maintained controls moderates observed OVB and boosts baseline R^2 slightly, generating a somewhat higher stability parameter value of 2. The third and most telling specification adds rank variables to W_0, while keeping NSF and MIT admit status and GRE scores in W_1. This boosts baseline R^2 to 0.2. Although R_2 increases to 0.267 when holdouts are included, observed OVB is negligible at $0.24 - 0.22 = 0.02$. The upshot is a stability parameter value of 4.32.

Table A3: Oster (2019) Stability of Elite Value-Added Estimate for Weighted DI

	WDI; treatment X: Any Elite					
Specification	$\hat{\beta}$ (W_0)	$\hat{\beta}$ $(W_0{+}W_1)$	R^2 (W_0)	R^2 $(W_0{+}W_1)$	R_{max}	δ for $\beta = 0$
A	0.34***	0.22***	0.139	0.267	0.347	1.58
	(0.04)	(0.04)				
B	0.33***	0.22***	0.141	0.267	0.347	2.00
	(0.04)	(0.04)				
C	0.24***	0.22***	0.200	0.267	0.347	4.32
	(0.04)	(0.04)				

Notes: OLS estimates of regressions of weighted DI publications on various sets of controls. The treatment is a dummy indicating PhDs earned at a high-tax or other elite program; the omitted group is all other US programs. Each specification compares a baseline regression with maintained controls W_0 to a regression that adds holdout controls W_1; the allocation of controls to W_0 and W_1 appears in the text above. Stability parameter δ estimates the degree of selection on unobservables relative to selection on W_1, conditional on W_0. R_{max} is set at $\min\{1.3\tilde{R}^2, 1\}$. Robust standard errors appear in parentheses; $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. $N = 2{,}582$.