The Class Gap in Career Progression: Evidence from US academia

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Unlike gender or race, class is rarely a focus of research or DEI efforts in elite US occupations. Should it be? In this paper, we document a large class gap in career progression in one labor market: US tenure-track academia. Using parental education to proxy for socioeconomic background, we compare career outcomes of people who got their PhDs in the same institution and field (excluding those with PhD parents). First-generation college graduates are 13% less likely to end up tenured at an R1, and are on average tenured at institutions ranked 9% lower, than their PhD classmates with a parent with a (non-PhD) graduate degree. We explore three mechanisms: (1) productivity, (2) preferences, and (3) discrimination. Research productivity can explain at most a third of the class gap, meaning first-gen college grads are "underplaced" at lower-ranked institutions than their research record would predict. Preferences explain almost none of the class gap. Discrimination likely explains the residual. Specifically, systemic or direct discrimination may arise from a lack of social and cultural capital. We find evidence consistent with this in analyses of coauthor networks and NSF awards. Finally, examining PhDs who work in industry we find a class gap in pay and in managerial responsibilities which widens over the career. This establishes that a class gap in career progression exists in other US occupations beyond academia.

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1 Introduction

Race and gender disparities in elite career progression – including hiring, pay, and promotions – are the focus of a voluminous body of academic research, as well as major organizational and legislative efforts to tackle them. Socioeconomic background or class origin, in contrast, is rarely considered in this context. This absence of research in part reflects a lack of data. Socioeconomic background is rarely measured by large-scale government surveys, or by organizations' DEI efforts. For example, in 2020 "not one of the companies on DiversityInc's 'Top 50 Companies for Diversity' mentioned social class in their diversity, inclusion, and equity goals and programs" (Ingram, 2021). This absence of data collection, though, itself suggests that many researchers and practitioners assume class is unimportant for elite career progression – that, while socioeconomic background may matter for getting into a good college, any lingering impacts of class are washed out beyond that point. This assumption is incorrect.

In this paper, we show that socioeconomic background is an important determinant of elite career progression – using US tenure-track academia as a case study. This is an interesting setting in itself, as professors' background may impact their research and teaching. More importantly, though, tenure-track academia is a setting which is uniquely suited for detailed, quantitative study of the role of socioeconomic background in elite career progression more generally: (i) there is a relatively standardized hiring and promotion process across organizations (tenure-track job market and tenure), (ii) productivity is quantifiable (through data on research output), and (iii) the relative quality of jobs at different organizations is to some extent quantifiable (e.g. research-intensiveness or rank of academic institutions). By examining the class gap in career progression in tenure-track academia, we can shed light on mechanisms by which analogous class gaps may emerge in other elite occupations.

Indeed, if anything, tenure-track academia likely represents a lower bound: in other elite occupations where productivity is less objectively measurable, promotion is less ostensibly meritocratic, and networking with elite clients or colleagues is more openly relevant for productivity, we would expect class to matter even more.

Our main data set is the NSF Survey of Doctorate Recipients, a large representative survey of US PhD recipients. We use parental education to proxy for socioeconomic background, primarily focusing on the "class gap" in outcomes between first-generation college graduates vs. people with a parent with a *non-PhD* graduate degree (since we want to focus on the effects of generalized socioeconomic advantage, and not academia-specific advantages conferred by PhD parents).[1]

Our core finding is that PhD graduates from less advantaged backgrounds are less likely to end up tenured at a research-intensive or highly-ranked school, as compared to their more-advantaged PhD program classmates. Specifically, conditional on fixed effects for PhD institution and field, first-generation college graduates are 13% less likely to end up tenured at an R1 than people with a parent with a non-PhD graduate degree. Among those tenured at ranked institutions, first-gen college grads are tenured at institutions ranked 9% lower – again, conditional on fixed effects for PhD institution and field.[2] Note that by conditioning on fixed effects for PhD field and institution, we effectively compare people *from the same* PhD program. All our analyses also condition on gender, race/ethnicity, and birth region, isolating the effect of socioeconomic background from other correlated demographic characteristics.

We examine the junctures at which this class gap emerges. First, we find that the

[1]We refer to class and socioeconomic background interchangeably, and abbreviate socioeconomic background to "SEB", using "lower-SEB" for less advantaged backgrounds, and vice versa.

[2]The coefficient for those with a parent with a college degree but no graduate degree is between the other two groups, consistent with monotonic class advantage. Those with a PhD parent are even more likely to end up tenured at an R1 or highly-ranked institution than those with a parent with a non-PhD graduate degree – suggestive of academia-specific advantages.

class gap is not a result of lower-SEB PhD recipients disproportionately choosing to leave academia for industry: there is no class gap in whether someone leaves academia, conditional on our baseline fixed effects. That is, socioeconomic background does not affect *whether* someone stays in academia, but does affect *where* they end up. Second, we find that a large class gap in outcomes exists at both major junctures in the PhD to tenure pipeline. There is a class gap in placement in tenure-track jobs at R1s or highly-ranked schools, conditional on PhD institution and field. There is also a class gap in whether someone gets tenure, when compared to their peers who were on the tenure track at the same institution.[3] And the class gap does not just manifest in tenure institution type: we also find a class gap in earnings among tenured and tenure-track academics (of 2.7%), and a class gap in job satisfaction (of 5.2%).

Strikingly, the class gap in the likelihood of ending up tenured at an R1 is as large as or larger than the analogous race or gender gaps. This emphasizes that – just as race and gender gaps require serious scrutiny – the class gap is also worthy of research and policy attention. Note, though, that the mechanisms by which the class gap is generated may differ from the mechanisms by which gender and racial gaps are generated, in large part because class is less directly observable than race or gender.

What explains the class gap in tenure outcomes? In section 4, we explore three possible channels: research productivity, preferences, and discrimination.

We start with research productivity. Lower-SEB academics might produce less or lower quality research than their PhD classmates if, for example, they have less time for research and/or less access to the mentorship required to develop their skills. Using a new linkage between the 2015 SDR, Web of Science, and NSF award data, we

[3]Conditional on tenure-track institution, first-gen college grads are 6.3 percentage points less likely to get tenure than people with a parent with a non-PhD graduate degree. Sample limited to those at ranked institutions, and following Sarsons et al. (2021) in defining "getting tenure" as ending up with tenure at their tenure-track institution, or a similarly-ranked or higher-ranked institution.

estimate class gaps in tenure outcomes with detailed field-specific controls for research quantity and quality, including publications, citations, journal impact factor, authorship position and contribution, and NSF awards. Controlling for research explains only one third of the class gap in tenure institution rank, and less than one-tenth of the class gap in the rate of getting tenure. Thus, differential research productivity cannot explain most of the class gap: first-gen college grads are "underplaced" at lower-ranked institutions than would be predicted by their research output.

Next we examine preferences. It is possible that lower-SEB academics choose tenured jobs at lower-ranked or less research intensive institutions because of different preferences or constraints than their higher-SEB peers – perhaps trading off employer prestige in order to be closer to family or community, to prioritize higher paying jobs as a result of financial constraints, to prioritize family care needs, or to work at an institution with a stronger social mission. We explore each of these to the extent possible in our data and find no evidence consistent with any of them playing a role.

Finally, we consider discrimination. Following Bohren et al. (2023) we define discrimination as "group-based disparities among equally-qualified individuals". In our context, the large residual class gap in tenure outcomes conditional on productivity and preferences thus appears to reflect discrimination. Systemic discrimination against low-SEB academics may arise if a lack of social or cultural capital makes it difficult to form valuable professional networks, which can affect the quality of recommendation and tenure letters, mentorship, and sponsorship. Direct discrimination may arise as a result of "fit" concerns in predominantly elite environments.

Examining coauthor networks and NSF awards, we find evidence which would be consistent with systemic and/or direct discrimination. First, we find suggestive evidence of difficulties forming professional networks: (i) lower-SEB academics have fewer coauthors, (ii) there is coauthorship homophily, with the coauthors of lower-

SEB academics more likely to also be lower-SEB than their other characteristics would predict, and (iii) lower-SEB academics' coauthors are less well-published than their other characteristics (including PhD and employer institution) would predict. These suggest frictions to forming professional networks across socioeconomic background (which matters because most academics at elite institutions come from advantaged backgrounds) and with high-productivity individuals. While this evidence is from coauthors, it seems likely that similar frictions would apply in forming other valuable relationships with potential mentors or letter writers. Second, examining NSF awards, we find that lower-SEB academics receive 11% fewer NSF awards than would be predicted by their institution, seniority, research record, and prior NSF award receipt. This suggests non-productivity-related obstacles to success for lower-SEB academics.

Is the class gap unique to tenure-track academia? In section 5, we examine PhDs who work in industry. We find substantial evidence of a class gap in career progression: (i) a class pay gap, conditional on our baseline fixed effects, which widens substantially with years of experience; (ii) a class gap in job satisfaction, with particular dissatisfaction with the level of responsibility and opportunities for advancement; and (iii) class gaps in the likelihood of being a manager, and in the number of supervisees, emerging as individuals progress in their careers. This means that tenure-track academia is not unique. A class gap in career progression also exists for PhDs in private industry – and thus likely in other elite US occupations as well.

Socioeconomic background is rarely considered in DEI efforts in either academia or other elite US occupations. This paper, along with other recent work (cited below), builds a strong case for researchers to consider socioeconomic background as an additional, crucial, axis of advantage in elite career progression, and to embark on the data collection and research efforts needed to document and understand the class gap in career progression in more detail.

2 Background and Empirical Setting

2.1 Related literature

There is very little work in economics on the role of class in career outcomes like hiring, pay, or promotion - in contrast to a large and growing literature on gender and race.[4] Our work is most closely related to Zimmerman (2019) and Michelman et al. (2022), who demonstrate the importance of elite social ties for labor market outcomes for graduates of elite colleges in Chile and in the US; Shukla (2022), who finds caste-based hiring discrimination based on perceived "fit"; and Staiger (2023), who finds family ties matter for access to high-paying jobs. Our paper also builds on recent work in sociology including qualitative work on the "class ceiling" in elite UK occupations (Friedman and Laurison, 2020; Friedman, 2023) and on class-based hiring discrimination in elite US occupations (Rivera, 2012); resume audit studies finding callback discrimination by social class cues (Rivera and Tilcsik, 2016; Galos, 2024); and findings of within-occupation pay gaps by class origin conditional on education level (Laurison and Friedman, 2024; Witteveen and Attewell, 2017; Torche, 2011).[5]

We see our contribution as threefold. First, we provide detailed, large-scale, quantitative evidence of a large class gap in career progression in an elite occupation. To our knowledge, we are the first paper to do this.[6] Second, we can quantify multiple as-

[4]A large literature documents how socioeconomic background affects whether and where someone goes to college (e.g. Chetty et al., 2020), but there is little work on the role of class beyond that point – whether and how class background affects people's career outcomes once they've graduated.

[5]See also Friedman and Laurison (2020) for the UK, Falcon and Bataille (2018) for France, Hällsten (2013) for Sweden, and Núñez and Gutiérrez (2004) for Chile; as well as Engzell and Wilmers (2021) on the role of firm pay premia in mediating this.

[6]Specifically, we estimate class gaps *conditional on* very fine-grained measures of the point of entry into the occupation (specific PhD program or tenure-track job). This is crucial: data limitations mean prior studies, such as those estimating class pay gaps, have not been able to condition on fine-grained measures of educational attainment and/or first job. This means that estimated class pay gaps in earlier work may be explained by people from less advantaged backgrounds having started on a worse footing (e.g. less prestigious college, lower grades, or a worse initial employer).

pects of career progression, including not just pay but also promotion and the quality of the employer institution. Third, we can shed light in detail on the mechanisms – in particular, the role of productivity, as proxied by highly granular research measures.

In focusing specifically on class in academia, we build on Morgan et al. (2022), who show that US tenure-track faculty are highly socioeconomically elite, and that those with more advantaged backgrounds place at more prestigious institutions; Stansbury and Schultz (2023), who study the socioeconomic background of US PhD recipients; and contemporaneous work by Airoldi and Moser (2024), who find class gaps in who became a "star" in early 20th century US academia.[7]

More broadly, our work speaks to the large literature on demographic disparities in elite career progression, which is primarily focused on gender and race. In particular, in finding an important role for networks, our work relates to Cullen and Perez-Truglia (2023) who find frictions to within-workplace social interactions partially explain the gender gap in promotions; in highlighting the role of bias in subjective evaluations we relate to Benson et al. (2024), who find a large gender gap in evaluations of "potential" even conditional on objective performance measures; and in finding a role for homophily we relate to Linos et al. (2023) who find effects of the racial composition of teams on individuals' retention and performance. In academia specifically, research has found gender differences in reference letters (Eberhardt et al., 2023), recognition or evaluation of work (Sarsons et al., 2021; Card et al., 2020; Hengel, 2022), coauthorship (Ross et al., 2022; Davies, 2022), and citations (Koffi, 2021); and racial differences in funding awards (Ginther et al., 2018; 2011) and citation patterns (Koffi et al., 2024). Our work emphasizes that class should also be considered – alongside gender and race – as an important axis of potential disadvantage in elite occupations.

[7] Our work also emphasizes that, even beyond disparities in the opportunity to become a scientist at all (Bell et al., 2019), socioeconomic background affects career success once in academic science.

2.2 Data

Our main dataset is the National Science Foundation's Survey of Doctorate Recipients ("SDR"). The SDR is a biennial survey of a representative sample of people who received a PhD from a US institution in a science, social science, engineering, or health field. The SDR is matched with the NSF's Survey of Earned Doctorates ("SED"), an annual census of all individuals who receive a research doctorate from a US institution in a given year. We use the SDR to obtain information on individuals' employment, including sector, salary, and – if in academia – employer institution and the type of position, and the SED to obtain information on parental education, other demographics, and PhD field and institution. To study research output, we use new linkages between the 2015 SDR and (1) the Web of Science bibliometric database, as well as (2) data from the NSF on all NSF awards.[8]

For most of our analyses, we use the 1993-2021 SDR surveys. This represents 14 survey waves, with about 30,000 individuals per wave for 1993-2013 and 80,000 for 2015-2021. The median respondent appears in 4 survey waves. Since our analyses typically pool over multiple survey waves, we cluster standard errors at the individual level. In all our main analyses, we weight our results using the survey weights provided by the NSF.[9] We restrict the sample for all our analyses to those living in the US.

2.3 Measuring socioeconomic background

To measure socioeconomic background, we use the highest level of education attained by either parent or guardian, creating four categories: (i) less than a four-year college degree ("first-gen"), (ii) four-year college degree, (iii) non-PhD graduate de-

[8]See Chang et al. (2022) for details on the SDR 2015 - Web of Science link.

[9]These weights adjust for differential sampling and nonresponse rates in order to be representative of the SDR population – US PHD recipients in science, social science, engineering, and health fields – with respect to gender, race/ethnicity, location, PhD year, and PhD field. SDR response rates are around two thirds. Unweighted regressions for all core analyses provide similar results.

gree, and (iv) PhD. While we compare all four parental education groups, our main focus is on differences in outcomes between first-generation college graduates and people with a parent with a non-PhD graduate degree (considering these two groups to be the least and most socioeconomically advantaged groups we observe).[10] We do not focus on people with a parent with a PhD because we want to evaluate the effects of generalized socioeconomic advantage on career outcomes, and having a parent with a PhD may confer academia-specific preferences, knowledge, or resources.[11] Parental education is one of the three most commonly used indicators of socioeconomic background, alongside family incomes and parental occupations (e.g. Hauser, 1994; Sirin, 2005). These other indicators are not available in our data. We believe parental education is an effective proxy for socioeconomic background, both because it is a strong predictor of family income (Sirin, 2005), and because parental education itself can provide students with a better understanding of elite professional opportunities, and the strategies needed to access and succeed in them.

2.4 Descriptive statistics

The bulk of our analyses focus on PhD recipients who are 10-30 years post-PhD and working in the US. Among this group in the 2021 SDR (aka people who got their PhD between 1991-2011), 32% were first-gen college grads; 25% had a parent with at most a four-year college degree; 29% had a parent with at most a non-PhD graduate

[10]The four parental education groups can proxy for average household income: for example, in the 1992 Current Population Survey (when many in our sample would have been children), the average household income of households with children, by the highest level of parental education, was: $29,300 for less than a four year college degree; $52,600 for a four-year college degree; $66,200 for a non-PhD graduate degree; and $76,600 for a PhD. Note that our SDR data does not tell us what kind of non-PhD graduate degree a person's parent received; in the 1993 National Survey of College Graduates, 38% of non-PhD graduate degree recipients had MBAs, JDs, or MDs (or other business, law, or medical degrees); 32% had degrees in psychology, education, or social work; 16% had masters degrees in STEM fields; and 16% had masters or professional degrees in non-STEM fields.

[11]A large literature shows occupational inheritance even within socioeconomic groups (e.g. Weeden and Grusky, 2005; Dal Bó et al., 2009), including in academia (Morgan et al., 2022).

degree; and 14% had a parent with a PhD.

The tenure outcomes among these four groups differ starkly: among both first-gen college grads, and those with a parent with a four-year college degree only, 22% were in tenured positions. This compares to 25% among people with a parent with a non-PhD graduate degree, and 29% among people with a parent with a PhD. Tenure rates at R1 institutions differ even more: 7.6% of the first-gen college grads in our sample were tenured at R1s, as compared to 10% of people with a parent with a non-PhD graduate degree. Thus, a randomly selected PhD recipient with a parent with a non-PhD graduate degree was about 14% more likely to be a tenured professor *somewhere*, and about 32% more likely to be a tenured professor at an R1, than a randomly selected first-gen college grad from the same group.[12]

Thus, tenured professors - particularly at elite institutions - are much more socioeconomically elite than the population of PhD recipients from which they draw (as also shown by Morgan et al., 2022). This drop-off in socioeconomic diversity motivates our analysis: it could reflect a class gap in career *starting point*, since lower-SEB individuals are more likely to do their PhDs at lower-ranked programs which send fewer graduates to elite tenure-track jobs; or it could reflect a class gap in career *progression* even conditional on PhD program. Our paper examines the latter.

3 Empirical Analysis

Our core analyses document whether tenure outcomes differ by socioeconomic background conditional on PhD program. Specifically, for all SDR respondents who are employed in the US and 10-30 years post-PhD,[13] we estimate, for three different

[12] Statistics calculated as, respectively: 24.9/21.8=1.14, 10.0/7.6=1.32. People with a PhD parent are even more likely to be tenured, and tenured at R1s. See Appendix Table A7. We show parental education shares of tenured professors over time in Appendix Figure A1.

[13] Starting at 10 years so that most individuals have faced their (first) tenure decision, and ending at 30 years to exclude any differential retirement patterns by SEB.

dependent variables $TenureVar_i$:

$$TenureVar_i = \alpha + \beta_1 ParentalEducation_i + X_i\gamma + \epsilon_i,$$

Our three dependent variables are (i) *Tenure anywhere*, a binary dependent variable taking value 1 if someone is in a tenured academic job and 0 otherwise, (ii) *Tenure at an R1,* a binary dependent variable taking value 1 if someone is tenured at an R1 and 0 otherwise, and (iii) *(log) Tenure institution rank*, measured as the most recent *US News and World Report* ("USNWR") graduate program ranking for the tenure institution (in the field in which the person got their PhD).[14] For the first two dependent variables, we use the full sample of working individuals, with the "0" category including people outside tenure-track academia (in non-tenure-track academic positions, industry, or government). For the third, our sample is by definition limited to those tenured at ranked institutions.

All regressions include fixed effects for PhD field, PhD institution, survey year, years since PhD, year of PhD receipt (in 5-year buckets), birth region, gender, and race/ethnicity (X_i).[15] We refer to this set of fixed effects as our *baseline fixed effects* and use them in all analyses unless noted otherwise. Note that PhD field and institution fixed effects hold constant any differences in socioeconomic background and tenure rates by PhD institution or field, roughly comparing individuals who got their PhD in the same PhD program (and in a robustness check we explicitly control for PhD program fixed effects).

[14] "R1" denotes the institutions deemed the most research-intensive by the Carnegie Classifications.
[15] See Appendix C for more details on these variables.

3.1 Main results

Table 1 (and Figure 1) illustrate our main results. Our core comparison of interest is between first-gen college grads (labeled "Less than college") and people with a parent with a non-PhD graduate degree (the omitted category).

Conditional on our baseline fixed effects, there is no statistically significant difference in the likelihood of ending up a tenured academic between first-gen college grads and people with a parent with a non-PhD graduate degree (column 1). The point estimate is very close to zero (-0.003) and the 95% confidence interval rules out more than a one percentage point difference in either direction – a small margin when compared to the 26% of our sample who are tenured.

In contrast, we find a large class gap in the likelihood of tenure at an R1. Conditional on our baseline fixed effects, a first-gen college grad is 1.3 percentage points less likely to be tenured at an R1, as compared to someone with a parent with a non-PhD graduate degree (column 2). Since only 10% of our full sample are tenured at R1s, this means first-gen college grads are about 13% less likely to end up tenured at an R1 than their classmates from the same PhD program who had a parent with a non-PhD graduate degree. The coefficient estimates for those with a parent with a college degree only are between the two groups, suggestive of a monotonic class advantage across parental education groups.

We also find a large class gap among those tenured at ranked institutions: first-gen college grads are at institutions ranked 9.2 log points lower than their PhD classmates with a parent with a non-PhD graduate degree (column 3). Again, the estimates for those with a parent with a college degree only are between the two groups.

Our results show that, conditioning on PhD field and institution, there is a large "class gap" in career progression in US academia. Defining the extensive margin as selection into or out of tenured academia, and the intensive margin as institution type conditional on being a tenured academic, we see that *this class gap exists entirely on the intensive margin, with no class gap on the extensive margin.*[16]

While not our main focus, it is interesting to also examine the results for those with PhD parents. Compared to those with a parent with a *non-PhD* graduate degree, they are more likely to end up in a tenured position at all (by 1.2pp), more likely to be tenured at an R1 (by 1.7pp), and are tenured on average at institutions ranked 15 log points higher. This is suggestive of academia-specific advantages which matter even beyond generalized socioeconomic advantage.

Robustness. Using alternative measures for tenure institution research-intensiveness or rank, we also find large, statistically significant class gaps, suggesting that our main results are not artefacts of specific definitions of tenure institution type (Appendix Table A1).[17] We also show that our coefficients are robust to alternate regression specifications: (i) including fixed effects for field-specific PhD program rank instead of PhD institution; (ii) including fixed effects for PhD field by institution by decade (directly comparing individuals in the same PhD program); (iii) including saturated fixed effects for age and time periods, specifically age at survey (5-year group), years since PhD receipt, survey year, and year of PhD receipt (5-year group); (iv) including fixed effects for narrow PhD field instead of our baseline PhD field definition; (v) including fixed effects for birth country instead of broader birth region; and (vi) not using survey weights (Appendix Figure A2). We also run our baseline regressions separately for each SDR survey year, showing that the class gap remains relatively consistent across time (Appendix Figure A3).

[16]Our tenure at an R1 regression implicitly combines the intensive and extensive margins by including people who are not tenured academics in the "0" group. We show this same regression, limiting our sample to tenured academics only, in Appendix Table A1 column 9.

[17]These include whether an institution is research-intensive at all; whether an institution is ranked in the top 50 or top 20; and using undergraduate institution rank.

3.2 The pipeline: PhD, tenure-track hiring, and tenure decision

At what stage of the PhD to tenure pipeline does the class gap in tenure institution type emerge? We do not see all stages of each individual's career in our SDR sample, so we use subsamples of our full data set to examine two junctures: from PhD to tenure-track job, and from tenure-track job to tenure.

To examine the PhD to tenure-track juncture, we limit our sample to those observed 1-9 years after their PhD, giving us our best guess of their first tenure-track job post PhD.[18] We then run a directly analogous set of regressions to those for Table 1, but using tenure-track positions instead of tenured positions. The results are shown in Table 2, columns 1-3. In this sample we once again find no class gap in the likelihood of being on the tenure track anywhere, but large class gaps on the other two outcomes: first-gen college grads are 1 percentage point less likely to be tenure-track at an R1 as compared to those a parent with a non-PhD graduate degree (representing 12% of the baseline mean), and among those on the tenure track at ranked institutions, they are at institutions ranked 7.7 log points lower. That is, there is no class gap on the extensive margin from PhD to tenure-track job, but there is a large class gap on the intensive margin.

To examine the tenure-track to tenure juncture, we limit our sample only to those who we observe in the SDR on the tenure track without tenure, and then again after their inferred tenure decision year. (This reduces our sample very substantially). Our core outcome is whether someone "got tenure", which we define following Sarsons et al. (2021) as having tenure at an institution with a rank which is higher or at most 5 rank points lower than their tenure-track institution.[19] We include our baseline fixed

[18] Using up to 9 years to incorporate time for a postdoc. This population is made up, on average, of later PhD cohorts relative to our main estimates in Table 1.

[19] This limits our sample to those on the tenure track at ranked institutions. We also limit to institutions for which we observe at least 5 individuals from that institution at the tenure juncture. See

effects with one alteration: instead of PhD institution fixed effects, we use fixed effects for tenure-track institution to implicitly compare individuals who are on the tenure track in the same department. We find a large class gap: first-gen college grads are 6.3 percentage points less likely to "get tenure" (9% of the baseline mean), compared to someone with a parent with a non-PhD graduate degree who was on the tenure track at the same institution (Table 2, column 4).[20]

3.3 Socioeconomic Background, Race, and Gender

To isolate the relationship between SEB and career outcomes, all our analyses condition on fixed effects for gender, race/ethnicity, and birth region. This is particularly important for race and birth region, which are both correlated with SEB and can themselves affect career progression. Thus, the class gap in career progression we identify is a gap based on differences in parental education, and *not* arising from correlated differences in race/ethnicity or country of origin.[21]

Strikingly, the class gap in the likelihood of ending up tenured at an R1 is as large as or larger than the analogous gender and racial/ethnic gaps (estimated controlling for parental education). But these gaps do not arise in the same way. While the class gap arises entirely at the intensive margin, the gender gap arises mostly at the extensive margin (the "leaky pipeline").[22] And racial and ethnic disparities in tenure outcomes arise both at the extensive and intensive margin.[23] Since gender and

Appendix C for how we infer the tenure decision year.

[20] The first-gen college grads who do not get tenure seem to mostly move to non-tenure-track academic jobs or industry, or to another tenure-track but not tenured academic job (Appendix Table A2). For academics on the tenure track at non-ranked institutions, we are not able to construct an equivalent "got tenure" outcome; there are no statistically detectable class gaps in getting tenure anywhere, but standard errors are large (Appendix Table A3).

[21] In addition, we re-run our baseline regressions limiting the sample only to White non-Hispanic US-born individuals, finding similar-sized class gaps in tenure institution type.

[22] Among the women who do stay in tenured academia, there is little detectable gender gap in the rank of their tenured jobs – or, indeed in the rate of "getting tenure" conditional on tenure track institution fixed effects (consistent with findings in Ginther and Kahn, 2014; Ceci et al., 2014; 2023).

[23] Black and Hispanic academics are more likely to go into tenured academia, conditional on our fixed

race/ethnicity are not our focus, we do not explore these dynamics further here, but present coefficients for gender and race/ethnicity for our main tables in Appendix B.

This comparison emphasizes that, just as race and gender gaps are important to study, class gaps are large enough to be worthy of serious scrutiny. Moreover, it emphasizes that class needs a distinct approach: it does not necessarily operate in the same way as gender or as race.

4 Mechanisms

Our findings show a class gap in career progression in tenure track academia, conditional on PhD program attended. First-gen college grads end up tenured at less research-intensive and lower-ranked institutions than their former PhD classmates. We find no class gap on the *extensive margin* of leaving tenure-track academia, suggesting that differential selection out of tenure-track academia is not the driving force.[24] Instead, the gap exists entirely on the *intensive margin* among tenure-track and tenured academics. Why might this class gap emerge between PhD program and tenure? We examine three possible channels: (1) Productivity (proxied by research output), (2) Preferences, and (3) Discrimination.

Channel 1: Research productivity. If lower-SEB academics produce less or lower-quality research, we would expect them to receive tenure-track or tenured job offers at lower-ranked institutions. This is particularly likely to be the case at R1 and ranked institutions for which research output is the main determinant of hiring and tenure

effects; but among those who are tenured, they are tenured at lower-ranked institutions (though the result for Black academics is not statistically significant). Black academics are also less likely to get tenure, conditional on tenure-track institution. Note that these estimated race/ethnicity gaps control for parental education; since Black and Hispanic academics are also more likely to be first-gen college grads, they will be disproportionately affected by class gaps too. Intersectionality between class, gender, and race/ethnicity is beyond our scope but worthy of further study

[24] We discuss this further, and estimate Lee-style bounds (Lee, 2009) on the potential role for differential selection by ability, in Appendix D.

decisions (Schimanski and Alperin, 2018). We examine whether differential research output can proximately explain the class gap in section 4.1.

Note that differential research output by SEB could be the result of three distinct factors. First, endowments: lower-SEB PhD recipients may enter their PhD program with fewer research-relevant skills or characteristics, on average, than their higher-SEB PhD classmates. Importantly, since our analyses condition on fixed effects for PhD institution and field, this channel requires differential endowments of research ability *within PhD program cohorts*.[25] Second, development: conditional on their skills on PhD entry, lower-SEB individuals may receive less skill development during the PhD and on the tenure track. This may be because of limited time or resources, or greater difficulties forming valuable mentorship relationships or other professional connections. Third, constraints: lower-SEB academics may simply have less time for research (e.g. if they need to do other paid work or attend to family responsibilities (Lee, 2017; Waterfield et al., 2019)).

Channel 2: Preferences. It may alternatively be the case that lower-SEB academics with similar options on average *choose* lower-ranked or less research-intensive tenured jobs than their otherwise equivalent higher-SEB peers.[26] One possibility is geographic constraints: lower-SEB academics may choose to be closer to family or community at the expense of institution rank (Gardner, 2013). Another possibility is financial constraints, if there is a trade-off between pay or financial security and institution quality. A third possibility is family constraints, if care responsibilities

[25] This means that to explain any research differential by differential endowments, it would need to be the case that these research-relevant skills or characteristics are unobservable to PhD admissions committees, and that lower-SEB PhD students have less of these skills than their higher-SEB classmates even conditional on observables like GRE scores, grades, and recommendation letters. (Note that affirmative action by socioeconomic background in PhD admissions is rare (Posselt, 2016), making it highly unlikely that lower-SEB individuals enter PhD programs with large differences in observable skills or characteristics.) See Appendix D for further discussion.

[26] Note that it does not appear to be the case that lower-SEB PhDs are less likely to want to get a tenured job at all: there is no class gap at the "extensive margin" (Table 1 column 1).

induce lower-SEB academics to choose less time-intensive jobs. A fourth possibility is pro-social preferences, if lower-SEB academics would prefer to work at a lower-ranked institution which served a less advantaged student body. We explore each of these in section 4.2.

Channel 3: Discrimination. Even conditional on their research productivity, lower-SEB individuals may receive tenure-track or tenured job offers from lower-ranked institutions. We call this channel "discrimination", following Bohren et al. (2023) in defining discrimination as "group-based disparities among equally qualified individuals". This can capture both direct discrimination, where two people are treated differently at the point of the hiring or tenure decision *only* based on differences in their socioeconomic background; and systemic discrimination, where SEB disparities *before* the hiring or tenure decision lead to different signals of researcher quality at the point of the hiring or tenure decision (for example, if a lower-SEB academic is less able to form strong mentorship relationships, and this results in lower-quality recommendation letters).

How might discrimination by SEB arise in the hiring or tenure process? The nature of the academic hiring and promotion process leaves substantial latitude for subjective judgments that go beyond observable measures of research quality (Rivera, 2017; Posselt et al., 2020): specifically, since individual opinions on the merits of research can differ widely (Lamont, 2009), hiring and tenure decisions must inevitably be based not only on quantifiable research measures, but also on more subjective evaluations of researcher quality by tenure or hiring committee members and letter writers.

Systemic discrimination may arise if lower-SEB individuals lack the kinds of cultural or social capital which are important for the formation of valuable professional relationships.[27] Lower-SEB academics may lack pre-existing academic social capital

[27] Following Bourdieu (1986), we define social capital as the availability of and quality of networks and

since they likely have fewer family or community ties in academia (Haney, 2015). Lower-SEB academics may also lack the kinds of cultural capital which are valued in academia. This may reduce their ability to form or deepen professional relationships (thus reducing their ability to develop academically valuable social capital). In qualitative studies, lower-SEB academics in the US and Canada report feeling excluded as a result of lacking cultural capital: feeling "isolated", "ill at ease", and as if they are "cultural outsiders" (Lee, 2017; Waterfield et al., 2019).[28] Having a more limited professional network, whether because of limited social or cultural capital, would affect the ability to signal research productivity – *even* conditional on actual research productivity. Most saliently, it may reduce the likelihood of having a high-profile recommendation or tenure letter writer; or may lead to weaker quality letters.[29] A more limited professional network may also mean lower-SEB academics are less able to generate other signals of research ability, like citations, awards, or prestigious presentations; and may have less help navigating the "hidden curriculum" of an academic career (Calarco, 2020), which can affect career prospects though channels like obtaining outside offers.

Direct discrimination by SEB may also arise in hiring or tenure decisions. In the US, socioeconomic background is rarely as directly observable as gender or race/ethnicity. Yet hiring decisions in elite occupations are often influenced by notions of "fit" with the existing culture and with idealized expectations of how a professional looks and

relationships which can provide useful resources; and define cultural capital as the acquired tastes, ideas, habits, and behaviors which confer status or recognition in the specific (academic) context.

[28] Examples from Waterfield et al. (2019) include not feeling "put together", bringing the "wrong things" to departmental potlucks, feeling unable to share personal stories, and pressure to speak in a particular way. "Almost all" participants singled out conferences as places where they felt particularly ill at ease. Lee (2017) cites an individual "chastised for my dress, my speaking, ... my hobbies", and others having difficulties fitting in because of implicit class-normed rules. A low-SEB academic in Haney (2015)'s survey wrote "I did not have experiences such as family vacations, attendance at cultural events such as opera, theatre... which showed in my interactions with others."

[29] Rivera (2017) finds that the prestige of the institution a letter writer comes from, and the reputation of the writer, are both weighed heavily in tenure-track hiring decisions.

behaves (Rivera, 2012; Rivera and Tilcsik, 2016; Friedman and Laurison, 2020). As a decision on a potential lifetime colleague, "fit" is central to tenure-track hiring and tenure decisions (Rivera, 2017; White-Lewis, 2020). Since academia, particularly at elite institutions, is made up predominantly of elite-origin individuals, there may be differential treatment of lower-SEB academics based on perceived "fit".[30] Indeed, lower-SEB academics report concerns about discrimination in qualitative studies.[31]

We cannot directly test for discrimination by SEB in our data. However, we can conceive of discrimination as the residual: to the extent that we *cannot* explain the class gap in tenure institution type by either differential research productivity or differential preferences, we can conclude that some combination of direct or systemic discrimination plays a role in explaining the class gap in tenure outcomes.[32] We also provide some exploratory evidence on a role for discrimination in section 4.3, examining coauthor networks and NSF award receipt.

4.1 Research Output

To evaluate whether differential research output explains the class gap in academia, we use our linked 2015 SDR - Web of Science - NSF award sample. This gives us a close-to-exhaustive set of the observable measures of research *quantity*, in terms of number of publications; *quality* in terms of journal impact factor and citations (using

[30]Judgments of ability may also be subconsciously influenced by class: Lamont (2009)'s research on grant-making found that judgments of "excellence" were often made on the basis of displaying cultural capital, and by fitting with subjective notions of "intuition, flair, elegance, and spark".

[31]One US academic in Lee (2017) said "I don't think it's a wise idea... to reveal my class background"; another that he was worried his background would adversely shape colleagues' opinion. Several low-SEB academics report trying, and finding it difficult, to hide their class origins because of their mode of dress, manners of speech, or even their teeth (Haney, 2015; Lee, 2017; Waterfield et al., 2019).

[32]Note, however, that the converse is not true: even if conditioning on research output explains all of the class gap, discrimination may still play a role in explaining this. For example, if discrimination based on perceived "fit" or social or cultural capital differences affect who gets access to the best mentorship during their PhD, this may affect someone's ability to produce high-quality research. This, in turn, would affect their job outcomes. In the framework of Bohren et al. (2023), this would be considered "technological" systemic discrimination.

CNCI, citations normalized for publication age and field); individual *contribution* in terms of authorship position and number of coauthors; and *funding success* using NSF award receipt. (See Appendix C for more details on these data).

Is there a class gap in research output? We first examine whether there actually is a class gap in research output, conditional on PhD program attended. Using all tenured academics in our linked SDR sample, we regress various research output measures on parental education alongside our baseline fixed effects X_i.[33] Our dependent variables are: (1) total number of publications, (2) number of first-author publications, (3) number of last-author publications, (4) average CNCI, (5) average journal impact factor, (6) number of NSF awards (bucketed), (7) share of publications which were in the top 10 percent by CNCI in their publication year, and (8) share of publications in a high impact journal.[34] We see a statistically significant class gap in research output across all research measures: first-gen college grads are on average 4 percentiles lower in the distribution of the number of publications, 3 percentiles lower in citations per paper, and 2 percentiles lower in average journal impact factor, than their former PhD classmates with a parent with a non-PhD graduate degree.

Predicting tenure institution type using research output: are lower-SEB tenured academics "underplaced"? The fact that lower-SEB academics produce fewer publications and have fewer citations than their higher-SEB former PhD classmates sug-

[33] We take all individuals who were tenured in 2015 and match them to their cumulative publication record as of 2015. To maximize our sample, we also incorporate all individuals who were untenured in 2015 but tenured in a later SDR year, and match them to their cumulative publication record as of 2017 (the last year we have Web of Science data). Of our final sample, 77% are first observed tenured in 2015, 9% in 2017, 8% in 2019, and 6% in 2021.

[34] For all except (6), (7), and (8), we use the PhD-field specific percentile rank of the variable, calculated within each of 10 broad PhD fields. We use the percentile rank because these variables are heavily skewed, but using a log transformation would not enable us to include zeroes. All of our results with percentile rank also hold in terms of statistical significance and sign when instead using logs or raw numbers. NSF awards are bucketed into a variable taking value 0 if 0 awards, 1 if 1 award, 2 if 2-3 awards, or 3 if 4+ awards. High impact journals are defined by Clarivate as the top 10% by impact factor. Results are shown in Appendix Table A4.

gests that research productivity may help explain the class gap in tenure institution type. But, the causality can run both ways: if lower-SEB academics get tenure-track jobs at institutions with less time or resources for research, they will also end up producing less research. Thus, we re-examine our two baseline outcomes which are least vulnerable to this reverse causality concern. First, we re-run our baseline regression for (log) tenure institution rank, controlling for detailed measures of research output. This asks "are low-SEB academics tenured at lower-ranked institutions than you would predict, based on their PhD institution and field, other demographics, *and* research output?" Note that this is a conservative test: it likely over-estimates the explanatory power of research since a lower-SEB academic who is on the tenure track at a lower-ranked institution may have still have less time or less funding for research, meaning that they have less or lower-quality research by the time we observe them.[35]

We show results in Table 3, Panel A (and visualize them in Figure 2). Column 1 presents the baseline results without research controls, for this more limited matched WOS-SDR sample. Column 2 incorporates our baseline research controls, which are second order polynomials in the number of publications, average CNCI, average impact factor, and average number of authors per paper, all interacted with PhD field group. Column 3 incorporates additional research controls: second order polynomials in first-author publications and in last-author publications, the number of NSF awards (bucketed), and two measures of the share of publications that were "hits" (share in the top 10 percent CNCI in the year published, and share in high impact journals), all also interacted with PhD field group.[36]

[35]Both of these analyses limit our sample to those at ranked institutions, where (i) research is the main determinant of hiring and tenure decisions (Schimanski and Alperin, 2018) and (ii) professors have substantial time and resources to dedicate to research.

[36]We have three PhD field groups: biological sciences, physical sciences, and social sciences. For the number of publications, first-author pubs, last-author pubs, CNCI, impact factor, and authors per paper, we again use the field-specific percentile rank. In Appendix Table A5 we show results using raw numbers instead of field-specific percentile rank; results are very similar.

If socioeconomic background *only* affects tenure outcomes through its relationship with research productivity, we should see no significant relationship between tenure institution type and parental education when controlling for research output ($\beta_1 = 0$). This is not the case: controlling for even our most detailed measures of research quantity and quality explains only around a third of the class gap. Specifically, in this sample there is a 15 log point class gap in tenure institution rank with our baseline fixed effects but without research controls. The class gap falls to 10 log points with the full suite of research controls, and remains statistically significant at the 1% level (Panel A).[37] Lower-SEB academics are "underplaced", tenured at substantially lower ranked institutions than you would predict by their educational history and research output, relative to their more socioeconomically advantaged peers.

At which kinds of institutions is this "underplacement" of lower-SEB academics particularly pronounced? We re-run our log rank regression, but without parental education, and show kernel density plots of the residuals by parental education in Figure 3. A negative residual tells us that a person is tenured at an institution that has a better rank than you would predict given their PhD program history and research output, and vice versa. The missing mass for first-gen individuals is predominantly in the left tail: higher-SEB individuals are more likely to be tenured at places that are *better-ranked than you would expect* given their research output. In contrast the distributions in the right tail of the graph are much more consistent across parental education groups, meaning there is less difference by SEB in the likelihood of being tenured at a place that is worse-ranked than you would expect given research output.

[37] The above regressions tell us that lower-SEB tenured academics are "underplaced" relative to their contemporaneous research record. But it is possible that at the time of the tenure decision, their outcome was a fair reflection of their research record – perhaps if lower-SEB academics were "slow starters" but then produced disproportionately more research post tenure. This does not appear to be the case: we find a similar-sized class gap in tenure institution type when controlling for cumulative research output at the (inferred) time of the tenure decision.

Getting tenure conditional on research output. Next, we re-run our "got tenure" regressions (Table 2 column 4), controlling for detailed measures of research output. Our baseline and full research controls remain the same as in the prior analysis, but here we control for cumulative research output *at the time of* the inferred tenure decision. Thus, we ask "are lower-SEB academics less likely to get tenure than their peers at the same tenure track institution, even controlling for their other demographics, field, *and* research output at the time of the tenure decision?".

We show these results in Table 3, Panel B (and visualize them in Figure 2). Controlling for research has very little effect on the class gap in the rate of "getting tenure", conditional on tenure track institution. With our baseline fixed effects, the class gap in "getting tenure" in this sample is 6.3pp; with our full suite of research controls, it reduces by less than one-tenth, to 6.0pp, and remains statistically significant at the 1% level (Panel B). Thus, even conditional on a high-dimensional measure of research output, there remains a large class gap in the likelihood of getting tenure for two people who are on the tenure track at the same institution.

Unobservable aspects of research quality. Our research measures cover almost all possible observable measures of research quality and quantity. These observable measures – publications, authorship credit, citations, journal impact factor, the number of "hit" publications, and the ability to attract funding awards – are widely agreed to be central to tenure decisions (Schimanski and Alperin, 2018). But some aspects of research quality may be unobservable to us but observable to tenure or hiring committees. For unobservable research quality to explain the large residual class gap, it would need to be the case that higher-SEB academics have very starkly better unobservable research quality than low-SEB academics, even conditional on all our detailed observable measures of research quantity and quality. The striking stability of the class gap coefficient when moving from our baseline to full research controls makes this unlikely: adding information on authorship contribution, NSF awards, and "hit" publications neither increases the explanatory power nor closes the class gap. Further measures of research quality or ability which are unobservable to us but observable to hiring or tenure committees therefore likely also have little additional explanatory power (unless they are highly uncorrelated with these observable measures of research quality). Even if we do assume unobservable research quality is uncorrelated with observed research output, we can bound the degree to which this unobservable research quality may explain our class gap using Oster (2019)'s bias correction. Under the assumption that the additional explanatory power of this unobservable research quality for tenure outcomes is half as big as the explanatory power of all our observable research measures combined, which we think is conservative, we estimate a class gap in tenure institution rank of 7.7 log points and a class gap in the likelihood of getting tenure, conditional on tenure-track institution, of 5.8 percentage points. (See Appendix D).

4.2 Preferences

In this section we explore reasons lower-SEB academics may be more likely to *choose* lower-ranked or less-research-intensive tenured jobs, relative to their higher-SEB peers with similar job options.

Distance from home. Lower-SEB academics may prefer to be employed at institutions closer to their home and family – perhaps because of family commitments or financial constraints – even at the cost of job quality. If part of the class gap is explained by those from less advantaged backgrounds being more likely to trade off institution quality for being closer to home, we should find that it is attenuated by controlling for this. We find no evidence for this: the class gap is essentially unchanged when controlling for a third-order polynomial in the distance between city of current institution and high school state (among those who went to high school in the US - see Appendix Figure A4).[38] Moreover, using a question in the SDR asking individuals to rate the perceived importance of 10 different components of a job, we find no class gap in the perceived importance of job location, conditional on our baseline fixed effects (Appendix Figure A5).

Financial constraints. Lower-SEB academics likely face greater financial constraints, so they may trade off tenure institution rank or research intensiveness for higher pay – and indeed, they rank pay and benefits as more important than their higher-SEB colleagues (Appendix Figure A5). But on average in tenure-track academia there is no tradeoff on this dimension: higher-ranked and more research-intensive institutions pay more.[39] Thus, stronger financial incentives for lower-SEB academics should push even more strongly toward getting a job at a higher-ranked (and higher-paying) institution.[40] Moreover, we find the class gap essentially unchanged when controlling for a third-order polynomial in the amount of student debt – one proxy for the degree of financial constraint an individual faces (Appendix Figure A4).

Family constraints. Lower-SEB academics may make different trade-offs between career and family. We re-run our baseline regressions, separately for those in our sample who ever had children vs. those who never had children, and find that the class gaps are similarly large for both groups, suggesting different career-family tradeoffs do not explain the class gap in tenure institution type (Appendix Figure A4).

[38] We also find a class gap in tenure institution type among foreign-born academics – again consistent with distance from home not being a key driving factor.

[39] A regression of log earnings for tenure-track and tenured academics on a dummy for R1 status, as well as fixed effects for survey year, 5-year PhD group, years since PhD, and PhD field, finds that R1 jobs pay on average 24 log points more; the analogous regression for institution rank finds that each 10-rank-point increment pays 1.8 log points more.

[40] Note that financial constraints may drive selection on the extensive margin (out of tenure-track academia into private industry), but that we do not see any class gap on the extensive margin overall, so the story would need to be one of a differential selection gradient by ability by SEB. We show evidence that this is unlikely in Appendix D2.

Institution type preferences. Lower-SEB academics may prefer to work at an institution which serves less advantaged students. Since private institutions tend to have higher-SEB student bodies (Chetty et al., 2020), we examine whether lower-SEB academics are more likely to be tenured at public institutions (vs. private), conditional on tenure institution rank group and our baseline fixed effects, but find no evidence of this.[41] Moreover, using the SDR question on perceived importance of job components, we find no class gap in the perceived importance of a job's contribution to society, conditional on our baseline fixed effects (Appendix Figure A5).

Other explorations. We estimate our baseline regressions separately for the three major field groups (biological sciences, physical sciences, and social sciences), finding similar-sized class gaps in each field group. This would suggest that factors common across academic fields are the key drivers of the class gap. We also estimate class gaps separately for people who did their PhD at programs ranked 1-30 or 30+, finding class gaps within both groups (Appendix Figure A4).

4.3 Discrimination

The above two sections explored the roles of differential productivity and differential preferences. We find that differential research productivity can explain around one third of the cross-sectional class gap in tenure institution rank, and one tenth of the class gap in "getting tenure" conditional on tenure-track institution. We find that differential preferences can explain essentially none of the class gap on either metric. It is likely, therefore, that the large residual class gaps reflect at least to some extent direct and/or systemic discrimination by socioeconomic background.

We cannot test whether there is direct or systemic discrimination by SEB directly in our data. In this section, we carry out two sets of exploratory analyses which are

[41] We also re-run our main regression separately for public and private institutions, finding class gaps in institution rank *within* public institutions and *within* private institutions.

suggestive of some kind of discrimination by socioeconomic background.

Professional networks. In this section, we explore whether lower-SEB academics have more limited professional networks. While we would ideally have data on academics' mentors, advisors, and recommendation and tenure letter writers, we cannot observe this in our data. We can, however, observe some information about coauthors. Specifically, for every individual in the linked 2015 SDR-Web of Science data, we observe (i) the number of coauthors they have on each paper, and (ii) demographic and professional information about the subset of these coauthors who also happened to be in the 2015 SDR. We can observe at least one coauthor in the SDR for over 23,000 individuals. To the extent that frictions in forming coauthor relationships exist for lower-SEB academics, this may be suggestive of frictions to forming other kinds of professional relationships too, including mentorship and sponsorship relationships.

We find three pieces of evidence from coauthor networks which suggest that lower-SEB academics have more difficulty forming broad and valuable professional networks.

First, lower-SEB academics have fewer coauthors per paper, conditional on our baseline fixed effects (Table A4 column 9).

Second, we find homophily by socioeconomic background in coauthorship. First-gen college grads have coauthors who are 0.7 percentage points more likely on average to also be first-gen college grads than you would predict, given these coauthors' PhD institution, PhD field, seniority, and other demographics(Table 4 Panel A, column 1). When considering only coauthors employed at academic institutions, there is even stronger homophily: the coauthors of a first-gen college grad are 1.5 percentage points more likely to also be first-gen college grads than you would predict based on their PhD field, PhD institution, current academic institution, seniority, and other demographics (column 2).[42] Our findings would be consistent with lower-SEB aca-

[42] Specifically, we take every individual in the 2015 SDR and residualize a dummy for whether they are

demics finding it easier to collaborate with other lower-SEB academics which, since the majority of academics at elite institutions are higher-SEB, would make the formation of valuable professional networks more difficult for lower-SEB academics.

Third, following an analogous procedure for coauthors' publication records, we find that first-gen college grads have coauthors who have fewer publications and citations, and publish in lower impact journals – relative to what you would predict based on these coauthors' demographics, PhD field and institution, and seniority (Table 4 Panel B, columns 1, 3, and 5).[43] We even find similar results when controlling for coauthors' current academic employer, although these results are noisier (columns 2, 4, and 6). This suggests that lower-SEB academics may face greater frictions in forming relationships with more productive or successful academics.

NSF awards. Our data also gives us all NSF awards received by any individual in the 2015 SDR. We regress a dummy for NSF award receipt at some point during 2016-20 on parental education, as well as fixed effects for gender, race/ethnicity, birth region, years since PhD, PhD year, PhD field, current institution, and tenure status, for all tenure-track or tenured individuals in the 2015 SDR.Without controlling for research output, we find that first-gen college grads were 3.3 percentage points less likely to receive an NSF award over 2016-20, as compared to their peers *at the same institution* with a parent with a non-PhD graduate degree (Table 5, column 1).

a first-gen college grad on fixed effects for their gender, race/ethnicity, birth region, PhD year, PhD field, and PhD institution. We then calculate the average of this residual across each individual's coauthors, weighting by their authorship share on each publication. We then regress this average coauthor residual on a dummy for the first-gen status of the original author. This gives the regression coefficients in Table 4, column 1. In column 2 we residualize on the same set of fixed effects as well as fixed effects for current academic employer institution. Columns 3 and 4 perform the analogous calculation for gender, and columns 5 and 6 for race/ethnicity, showing homophily by gender and race (echoing e.g. Boschini and Sjögren, 2007; Freeman and Huang, 2015). Notably, the degree of homophily by SEB is not much smaller than by gender or race.

[43] Specifically, we residualize each research measure on parental education and our baseline fixed effects, calculating the weighted average residual across coauthors, and regressing this on parental education. We use research at the time of the co-authored publication, and use the field-specific percentile rank.

Adding full research controls for pre-2016 research record, the gap falls only a little, to 2.8 percentage points (column 3). Even adding a control for pre-2016 NSF award receipt, we still see a class gap of around 2 percentage points in NSF award receipt, which is statistically significant at the 10% level. Since 18% of this sample receive at least one NSF award over 2016-20, this reflects a class gap of 11% (=0.02/0.18) in NSF award receipt probability relative to baseline. While it is possible that this class gap reflects unobservable research quality differences (even conditional on our very detailed measures of research output, which would reflect the measures of research output available to the NSF award committee), to the extent that research cannot explain the class gap in NSF award receipt, systemic or direct discrimination may play a role. A prestigious funding award like the NSF award is a decision based not only on prior academic record but also subjective judgments of potential and excellence, which in turn may be affected by evaluations from high-profile researchers and broader professional reputation (Lamont, 2009).[44]

5 Beyond Academia: Class gaps in other sectors

Only about 30% of our SDR sample are tenured or tenure-track. The rest work in industry, government, or non-tenure-track academia. We have much less information on the SDR recipients working in these sectors than we do on those in academia. Nonetheless, we can examine earnings, job satisfaction, and managerial responsibilities to see if there is evidence of a class gap in these other sectors. This will help us understand if our results on the class gap in career progression can generalize to

[44] We also find that publications where the author is a first-gen collge grad are less well cited than you would predict from the publication's field, year, type, and journal impact factor, and the author's other demographics, seniority, and institution of employment (Appendix Table A6.) This gap would be consistent either with lower-SEB academics writing lower-quality papers conditional on journal impact factor or with lower-SEB academics having more difficulty generating citations (e.g. if limited networks lead to fewer seminar invites).

other sectors of the US economy.

Earnings. We regress log annual earnings on parental education and our baseline fixed effects for PhD recipients employed in each sector. We find a class earnings gap of 2.7 log points for those in tenure-track academia and of 1.6 log points in industry (between first-gen college grads and people with a parent with a non-PhD graduate degree) – but no gap in government or non-tenure-track education (Table 6).[45]

Job satisfaction. We regress dummy variables for job satisfaction overall, as well as for nine sub-components, on parental education and our baseline fixed effects.[46] In tenure-track academia and industry – the two sectors where we found class earnings gaps – we also find class gaps in job satisfaction. First-gen college grads in tenure-track academia are 5.2% less likely to report being "very satisfied" overall relative to people with a parent with a non-PhD graduate degree, which represents; the analogous gap in industry is 2.4% (Figure 4).[47] For both sectors, the class gaps in job satisfaction are particularly large in three categories which closely reflect concepts of career progression: opportunities for advancement, intellectual challenge, and level of responsibility. And, just as we found no class earnings gaps in these sectors, we find no overall class gap in job satisfaction for PhDs working in government or non-tenure-track education.

Career progression in industry. Our findings on pay and job satisfaction suggest that there is a class gap in post-PhD career success for PhDs who go into industry, but not in government or non-tenure track education. We thus examine industry

[45] Torche (2018) finds only a small association between parental education and adult children earnings in the SDR, but does not control for PhD field or PhD institution, or choice of industry post-PhD.

[46] Our dependent variable is 1 if "very satisfied" and 0 otherwise; on most dimensions the share reporting "very satisfied" is close to half. The nine sub-components are satisfaction with: salary, benefits, job security, location, opportunities for advancement, intellectual challenge, level of responsibility, degree of independence, and contribution to society.

[47] Calculated as the coefficient on first-gen college graduate divided by the dependent variable mean. For tenure-track academia, this is 0.0277/0.529 = 0.052. For industry, this is 0.0113/0.473 = 0.024.

further. We re-run our earnings gap regressions interacting SEB and 5-year-buckets since PhD (and incorporating our baseline fixed effects). We find large increases in the class earnings gap over the course of a career (Figure 5, top left panel). In fact, in the first 5 years after PhD graduation, the class earnings gap is slightly positive, but the gap soon becomes negative, growing to an 8.1 log point gap by 20-25 years after the PhD. This may reflect slower progression for lower-SEB individuals to senior positions. In the other panels of Figure 5 we run analogous regressions with dependent variables reflecting managerial responsibilities – the log of 1 + the number of supervisees, and dummies reflecting whether someone reports being in any managerial occupation or a top managerial occupation. We see growing class gaps over the course of a career in all three metrics, suggesting slower progression to managerial roles for lower-SEB individuals.[48] Together, these findings on pay, job satisfaction, and managerial responsibilities suggest that the class gap in career progression is not a phenomenon unique to tenure-track academia: it exists in private industry as well.

6 Conclusion

Disparities in career outcomes by gender and race, across a range of elite occupations, have rightly attracted substantial attention from research and policy. Class background, in contrast, is rarely the focus of data collection, research, or DEI efforts in elite occupations (Ingram, 2021).

This paper documents large, persistent disparities in career outcomes by socioeconomic background in tenure-track academia. Specifically, we show that when comparing two PhD recipients from the *same institution and same field*, socioeconomic

[48] We do not see growing class gaps in earnings or these measures of managerial responsibilities over the course of the career in either government or non-tenure-track academia.

background as proxied by parental education is a strong predictor of whether someone ends up tenured at a research-intensive or highly-ranked institution. Disparities in research production explain some of the gap (which could result from differential endowments of research ability pre-PhD or differential ability to develop research ability and produce research during PhD and tenure track). But large class gaps in tenure institution type persist even conditional on very detailed controls for research quantity and quality, suggesting that lower SEB academics are "underplaced" relative to their research record. We find no evidence consistent with the hypothesis that this residual class gap in tenure institution type is explained by lower-SEB academics choosing to trade off job prestige or quality for other factors, like location or pay. Thus, we infer that much of the remaining class gap may be driven by systemic or direct discrimination, perhaps driven by perceived "fit" or differences in cultural or social capital. Supportive of this hypothesis, our coauthorship network analysis finds evidence consistent with barriers to professional network formation for low-SEB academics; and our NSF award analysis finds evidence consistent with disparate access to prestigious funding awards even conditional on prior research record.

Finally, we find class gaps in pay, job satisfaction, and progression to managerial responsibilities among PhDs in industry. We see our results as as a proof of concept that class likely matters for career progression in many elite occupations, not just academia. While tenure-track academia is a setting uniquely suited to examine the role of class in career progression – with detailed data on productivity, promotions, and firm quality – if anything class likely matters even more in occupations where productivity is less measurable, promotion decisions are less ostensibly meritocratic, and elite networking is more important. Identifying to what extent and at what stage class gaps in career progression exist in other elite occupations, and what drives them, is worthy of further study.

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Tables and Figures

Table 1: Baseline regression – Tenure outcomes, conditional on PhD institution and field

	(1)	(2)	(3)
Dep. var.	Tenure anywhere	Tenure at R1	Tenure institution rank (log)
Parental education (omitted category: non-PhD graduate degree)			
Less than college	-0.00285	-0.0127***	0.0916***
	(0.0055)	(0.0039)	(0.033)
College	-0.00440	-0.00567	0.0279
	(0.0060)	(0.0043)	(0.037)
PhD	0.0124*	0.0168***	-0.153***
	(0.0072)	(0.0056)	(0.045)
Demographics FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
PhD Field FE	Yes	Yes	Yes
PhD Institution FE	Yes	Yes	Yes
Dep Var Mean	0.26	0.10	3.93
Observations	239,065	239,065	31,596
Unique Individuals	76,841	76,841	10,960
Absorbed DF	489	489	394
Sample:	*All employed individuals 10-30 yrs since PhD*		*Tenured at ranked institutions only*

Source: SDR 1993-2021. *Notes:* Standard errors clustered at individual level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variables for cols 1 and 2 are binary variables taking value 1 if individual is tenured anywhere (col 1), or tenured at an R1 (col 2) and 0 if in any other job. Dependent variable for column 3 is the log rank of the tenure institution (field-specific graduate program rank from *USNWR*). Sample for all columns is restricted to people 10-30 years since PhD receipt, currently working in the US. Columns 1-2 cover SDR years 1993-2021 and column 3 years 1997-2021. Sample in column 3 is restricted only to those tenured at ranked institutions (by definition of the dependent variable). Regressions weighted by NSF-provided survey weights. Demographics FE are fixed effects for gender, race/ethnicity, and birth region. Time FE are fixed effects for survey year, years since PhD, and PhD year (5-year group). "Absorbed DF" shows degrees of freedom absorbed by fixed effects.

Table 2: Where in the pipeline does the class gap appear?

Juncture	PhD to tenure track			Tenure track to tenure
	(1)	(2)	(3)	(4)
Dep. var.	TT anywhere	TT at R1	TT institution rank (log)	Got Tenure
Parental education (omitted category: non-PhD graduate degree)				
Less than college	-0.00355	-0.00954***	0.0774**	-0.0625***
	(0.0045)	(0.0029)	(0.035)	(0.023)
College	-0.00482	-0.00741**	0.0146	-0.0373
	(0.0046)	(0.0030)	(0.040)	(0.025)
PhD	0.00623	0.0171***	-0.117	0.0168
	(0.0059)	(0.0043)	(0.048)	(0.025)
Demographics FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
PhD Field FE	Yes	Yes	Yes	Yes
PhD Institution FE	Yes	Yes	Yes	
TT Institution FE				Yes
Dep Var Mean	0.22	0.077	3.84	0.72
Observations	177,852	177,852	16,087	3,670
Unique Individuals	82,420	82,420	8,834	3,670
Absorbed DF	509	509	378	308
Sample:	*All employed individuals 1-9 yrs since PhD*		*TT at ranked institutions only*	*TT at ranked institutions only*

Source: SDR 1993-2021. *Notes:* Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Table shows regressions for the PhD to tenure-track juncture (columns 1-3) and tenure-track to tenure juncture (column 4). *PhD to tenure-track juncture:* Dependent variables for cols 1 and 2 are binary variables taking value 1 if individual is on the tenure track anywhere (col 1), or on the tenure track at an R1 (col 2), and 0 if in any other job. Dependent variable for column 3 is the log rank of the tenure-track institution (field-specific graduate program rank from *USNWR*). Sample for columns 1-3 is restricted to people 1-9 years since PhD receipt, currently working in the US. Columns 1-2 cover SDR years 1993-2021 and column 3 years 1997-2021. Sample in column 3 is restricted only to those on the tenure track at ranked institutions (by definition of the dependent variable). *Tenure track to tenure juncture:* Dependent variable is a binary variable taking value 1 if individual has tenure at the original tenure-track institution, or an institution ranked higher or at most 5 rank points lower, and 0 if doing anything else. Sample restricted to those on the tenure track without tenure at ranked US institutions in the last survey observation before their inferred tenure decision year (and for which we observe at least 5 individuals at that institution). *All:* Regressions weighted by NSF-provided survey weights. Demographics FE are fixed effects for gender, race/ethnicity, and birth region. Time FE are fixed effects for survey year, years since PhD, and PhD year (5-year group). "Absorbed DF" shows degrees of freedom absorbed by fixed effects. Standard errors are clustered at individual level in columns 1-3 and are robust in column 4.

Table 3: Tenure outcomes with research controls

	No research controls	With research controls	
	(1)	(2)	(3)
Panel A: Tenure institution rank (log)			
Less than college	0.149***	0.103**	0.101**
	(0.046)	(0.043)	(0.042)
College	0.0455	0.0563	0.0674
	(0.049)	(0.045)	(0.044)
PhD	-0.104*	-0.0672	-0.0542
	(0.055)	(0.049)	(0.049)
Observations	6,969	6,920	6,920
R-Squared	0.24	0.36	0.37
Adjusted R-Squared	0.20	0.32	0.34
PhD Institution FE	Yes	Yes	Yes
Panel B: Got tenure, conditional on tenure-track institution			
Less than college	-0.0635***	-0.0594**	-0.0596***
	(0.022)	(0.023)	(0.023)
College	0.00959	-0.0141	-0.0140
	(0.023)	(0.024)	(0.023)
PhD	0.00859	-0.0108	-0.0210
	(0.022)	(0.023)	(0.023)
Observations	1,907	1,894	1,894
R-Squared	0.61	0.65	0.67
Adjusted R-Squared	0.54	0.58	0.60
TT Institution FE	Yes	Yes	Yes
Demographics FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
PhD Field FE	Yes	Yes	Yes
Baseline Research Controls		Yes	Yes
Add'l Research Controls			Yes

Source: SDR 2015-2021, matched with Web of Science and NSF Awards. *Notes:* Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Panel A replicates our baseline tenure institution rank regression in Table 1, column 3, but with controls for research output. Panel B replicates our baseline "got tenure" regression in Table 2, column 4, but with controls for research output. Sample restricted to 2015 SDR respondents who were tenured at a US institution in 2015, or in the first SDR year we observe them with tenure after 2015. Demographics FE are fixed effects for gender, race/ethnicity, and birth region. Time FE are fixed effects for survey year, years since PhD (5-year group), and PhD year (5-year group). Panel A includes fixed effects for PhD institution, and Panel B for tenure-track institution. Columns 2 and 3 add controls for research output, all interacted with broad PhD field group. Baseline Research Controls are second order polynomials in: number of publications (field-specific percentile rank "fspr"), average CNCI per paper (fspr), average number of authors per publication (fspr), average impact factor per publication (fspr). Additional ('Add'l') Research Controls are second order polynomials in first author publications (fspr) and in last author publications (fspr), as well as NSF Award buckets (categorical var for 0, 1, 2, 3, or 4+), share of publications in top 10% CNCI, and share of publications in high impact journals. Regressions weighted by NSF-provided survey weight.

Table 4: Coauthor characteristics

Panel A: Coauthor homophily

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var	First-gen	First-gen	Female	Female	URM	URM
Parental education (omitted category: at least a college degree)						
First-gen college grad	0.00717***	0.0153***				
	(0.0025)	(0.0043)				
Gender (omitted category: male)						
Female			0.0325***	0.0392***		
			(0.0023)	(0.0040)		
Race/ethnicity (omitted category: neither Black nor Hispanic)						
Under-Represented Minority (Black or Hispanic)					0.0490***	0.0319***
					(0.0028)	(0.0045)
Observations	23,171	8,708	23,171	8,708	23,171	8,708

Panel B: Coauthor research output

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var	Publications	Publications	Citations	Citations	Impact Factor	Impact Factor
Parental education (omitted category: non-PhD graduate degree)						
Less than college	-0.00459***	-0.00386	-0.00668***	-0.00596**	-0.0100***	-0.00441
	(0.0017)	(0.0026)	(0.0017)	(0.0026)	(0.0018)	(0.0028)
College	-0.000602	-0.00325	-0.00299	-0.00550*	-0.00386*	-0.00248
	(0.0018)	(0.0027)	(0.0019)	(0.0028)	(0.0020)	(0.0031)
PhD	-0.00320	-0.00129	-0.00177	-0.000341	-0.000516	0.00147
	(0.0021)	(0.0031)	(0.0022)	(0.0031)	(0.0023)	(0.0035)
Observations	23,160	8,662	23,160	8,662	23,160	8,662

Source: Web of Science matched with 2015 SDR. *Notes:* Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Regressions are at author level. Dependent variables in each column are the average residual of an individual's coauthors. In Panel A, the residual is based on demographics, estimated from a regression of a dummy for first-gen status (cols 1/2), female (cols 3/4), or URM (cols 5/6) on fixed effects for all other demographics (parental education, gender, race/ethnicity, birth region), PhD year, PhD institution, and PhD field (cols 1/3/5) as well as fixed effects for current academic institution (cols 2/4/6). In Panel B, the residual is based on research output, estimated from a regression of cumulative publications (cols 1/2), cumulative CNCI (cols 3/4), or average journal impact factor (cols 5/6) (at time of publication of coauthored paper) on fixed effects for the coauthor's demographics (parental education, gender, race/ethnicity, birth region), PhD year, PhD institution, and PhD field (cols 1/3/5) as well as fixed effects for current academic institution (cols 2/4/6). Field-specific percentile rank is used for publications, CNCI, and journal impact factor.

Table 5: NSF Award Receipt (2016-20), conditional on research output

	(1)	(2)	(3)	(4)
Dep var:	Receipt of NSF award 2016-2020 (Binary: 1 if yes)			
Parental education (omitted category: non-PhD graduate degree)				
Less than college	-0.0331***	-0.0288**	-0.0279**	-0.0203*
	(0.012)	(0.012)	(0.012)	(0.011)
College	0.00234	0.00396	0.00407	0.000451
	(0.014)	(0.014)	(0.013)	(0.012)
PhD	0.00203	0.00315	0.00349	0.00342
	(0.016)	(0.016)	(0.016)	(0.015)
Demographics FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
PhD Field FE	Yes	Yes	Yes	Yes
Current Institution FE	Yes	Yes	Yes	Yes
Tenure Status FE	Yes	Yes	Yes	Yes
Baseline Research Controls		Yes	Yes	Yes
Add'l Research Controls			Yes	Yes
Prior NSF Awards				Yes
Dep Var Mean	0.18	0.18	0.18	0.18
Observations	10,654	10,485	10,485	10,485
R-Squared	0.29	0.32	0.32	0.42
Adjusted R-Squared	0.21	0.24	0.24	0.35

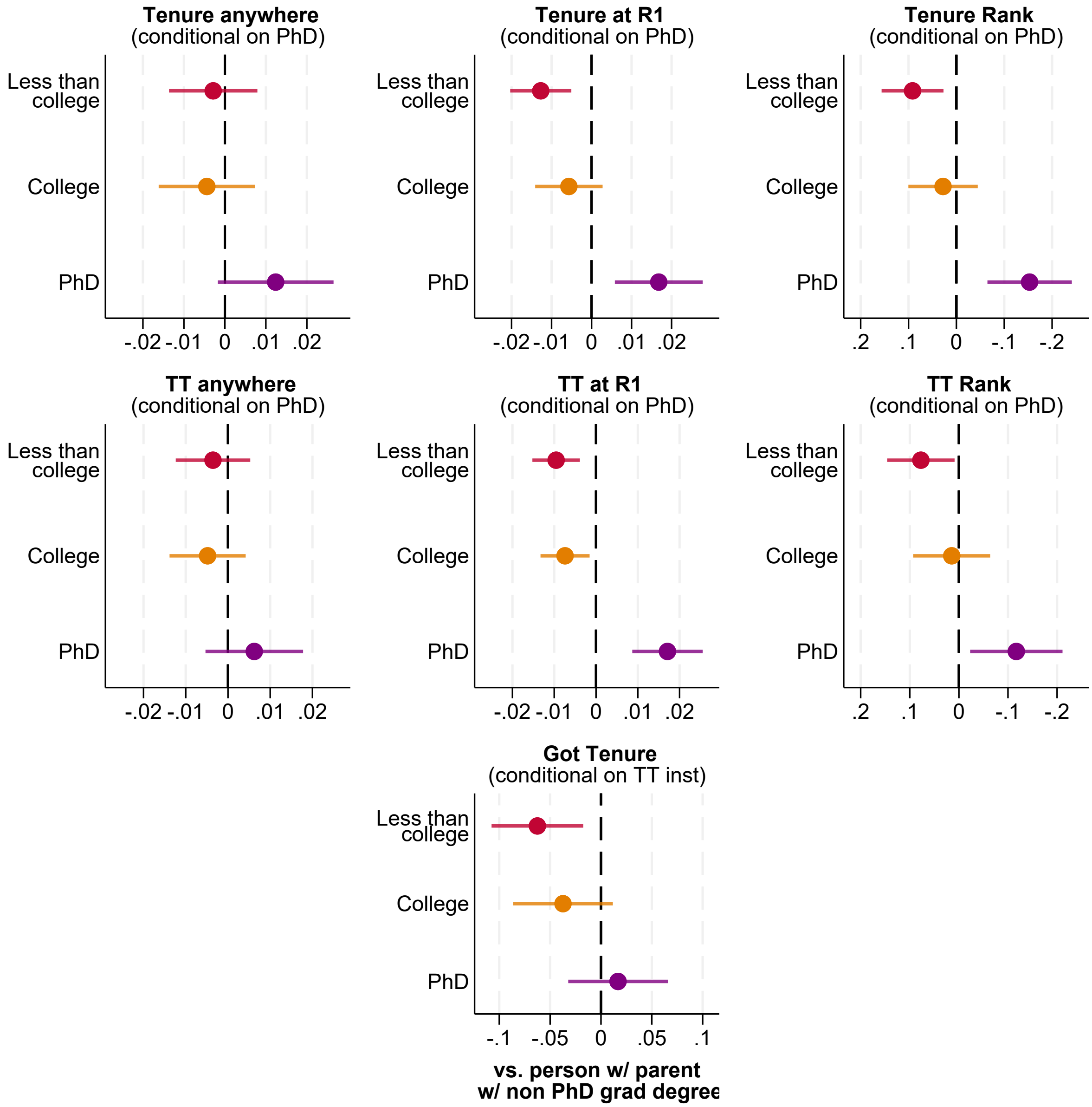
Source: 2015 SDR matched with Web of Science and NSF awards. *Notes:* Unit of analysis is the individual level. Sample limited to those with tenure or on the tenure track at an identifiable US academic institution in 2015. Sample excludes those with PhDs in Economics or Health related disciplines, since NSF awards are rare in these disciplines. Dep var is dummy taking value 1 if an individual receives an NSF award in any year 2016-2020 and 0 otherwise. Regressions weighted by NSF-provided survey weight. Demographics FE are fixed effects for gender, race/ethnicity, and birth region. Time FE are fixed effects for survey year, years since PhD (5-year group), and PhD year (5-year group). Research controls defined as in Table 3, with the exception that "Add'l Research Controls" does *not* include prior NSF awards in column 3; column 4 then adds controls for prior NSF award receipt.

Table 6: Class gap in earnings for PhD recipients, by sector of employment

Dep. var: Log earnings	(1)	(2)	(3)	(4)	(5)
Sector	Tenure-track academia	Industry	Government	Non-tenure-track education	Tenure track, w/ institution FEs
Parental education (omitted category: non-PhD graduate degree)					
Less than college	-0.0273***	-0.0162*	-0.000631	-0.00759	-0.0141**
	(0.0077)	(0.0096)	(0.011)	(0.011)	(0.0067)
College	-0.0144*	-0.00862	-0.00327	-0.0406***	-0.00926
	(0.0083)	(0.0098)	(0.012)	(0.012)	(0.0071)
PhD	0.0159	-0.000952	0.0125	-0.0220	-0.00300
	(0.0099)	(0.013)	(0.014)	(0.015)	(0.0084)
Demographics FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
PhD Field FE	Yes	Yes	Yes	Yes	Yes
PhD Institution FE	Yes	Yes	Yes	Yes	
Current Institution FE					Yes
Faculty Rank FE					Yes
Dep Var Mean	11.1	11.3	11.1	10.7	11.1
Observations	93,433	142,764	35,333	69,725	90,152
Unique Individuals	32,538	55,082	15,189	35,037	31,179
Absorbed DF	475	506	458	494	2,821

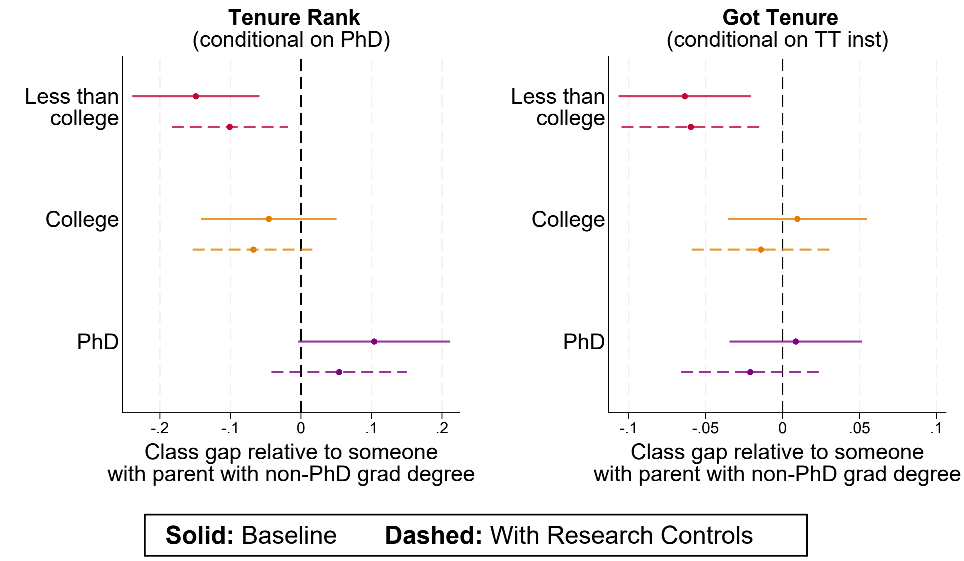
Source: SDR 1993-2021. *Notes:* Standard errors, clustered at individual level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Sample restricted to people up to 30 years since PhD receipt, currently working in the US; each column restricts sample to sector in title. Column 5 replicates column 1 but with fixed effects for institution and for faculty rank (assistant, associate, full professor). Regressions weighted by NSF-provided survey weight. Demographics FE are fixed effects for gender, race/ethnicity, and birth region. Time FE are fixed effects for survey year, years since PhD, and PhD year (5-year group). "Absorbed DF" shows degrees of freedom absorbed by fixed effects.

Figure 1: Baseline regression – Tenure outcomes



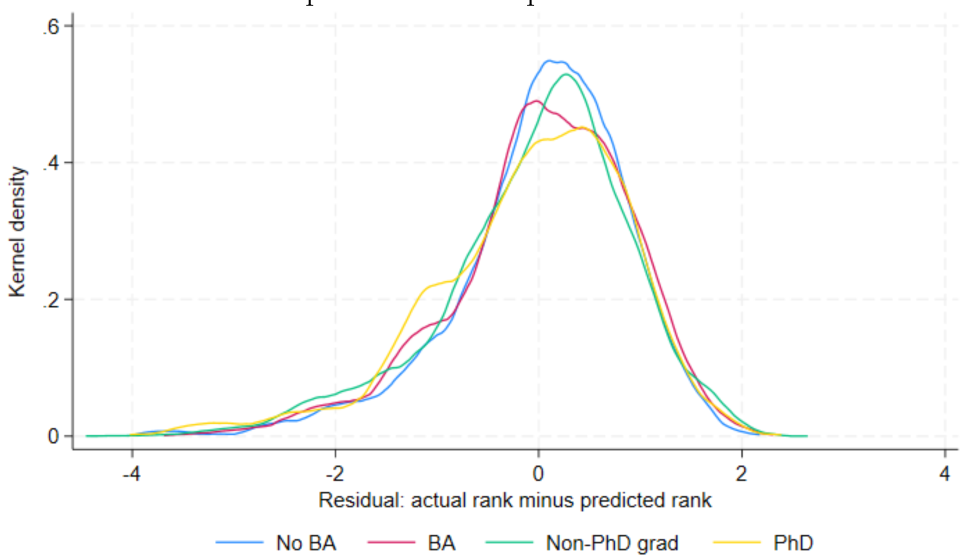
Source: SDR 1993-2021. *Notes:* Point estimates and 95% confidence intervals, from our baseline regressions: "Tenure" plots in the first row show coefficients from Table 1, "TT" plots in the second row show coefficients from Table 2 Columns 1-3, and "Got Tenure" plot in the third row shows coefficients from Table 2 Column 4. Dependent variable for each subplot is shown in the subplot title. All dependent variables are binary vars (1/0) except Tenure Rank and TT Rank which are the log of the tenure or tenure-track institution rank respectively. Coefficients are relative to the omitted category: people with a parent with a non-PhD graduate degree. Estimates for tenure and TT regressions are conditional on our baseline fixed effects: gender, race/ethnicity, birth region, time, PhD institution, PhD field. Estimates for "got tenure" are for those at ranked TT institutions only, and are conditional on tenure track institution fixed effects as well as demographic, time, and PhD field fixed effects. Regressions weighted by NSF-provided survey weight. Rank x-axes are inverted for ease of comparison with other outcomes.

Figure 2: Tenure outcomes with research controls



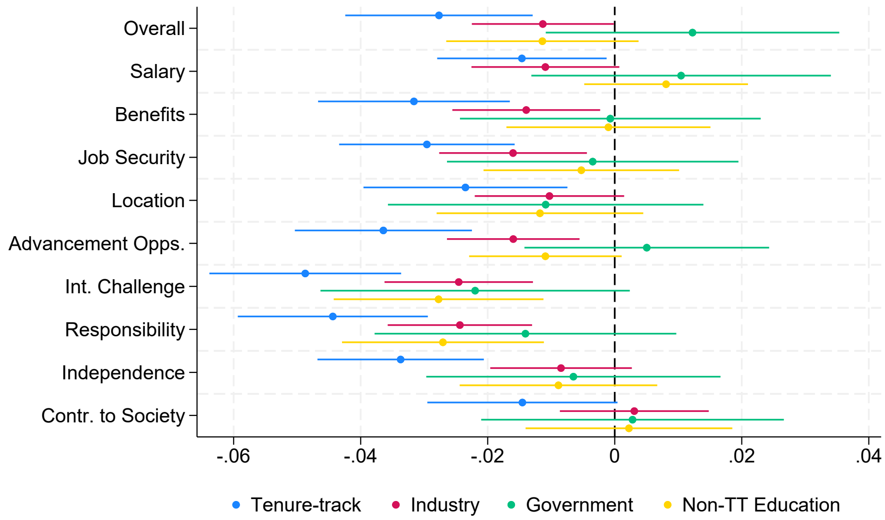
Source: SDR 1993-2021. *Notes:* Point estimates and 95% confidence intervals, from our regressions with research controls in Table 3. Tenure Rank subplot shows coefficients from Panel A, Columns 1 and 3. Got Tenure subplot shows coefficients from Panel B, Columns 1 and 3. Dependent variable for each subplot is shown in the subplot title. Coefficients are relative to the omitted category: people with a parent with a non-PhD graduate degree. Estimates for tenure rank regressions are conditional on our baseline fixed effects: gender, race/ethnicity, birth region, time, PhD institution, PhD field. Estimates for "got tenure" are for those at ranked TT institutions only, and are conditional on tenure track institution fixed effects as well as demographic, time, and PhD field fixed effects. Regressions weighted by NSF-provided survey weight. Research controls described in notes to Table 3.

Figure 3: Are individuals "underplaced" or "overplaced" relative to their research output?



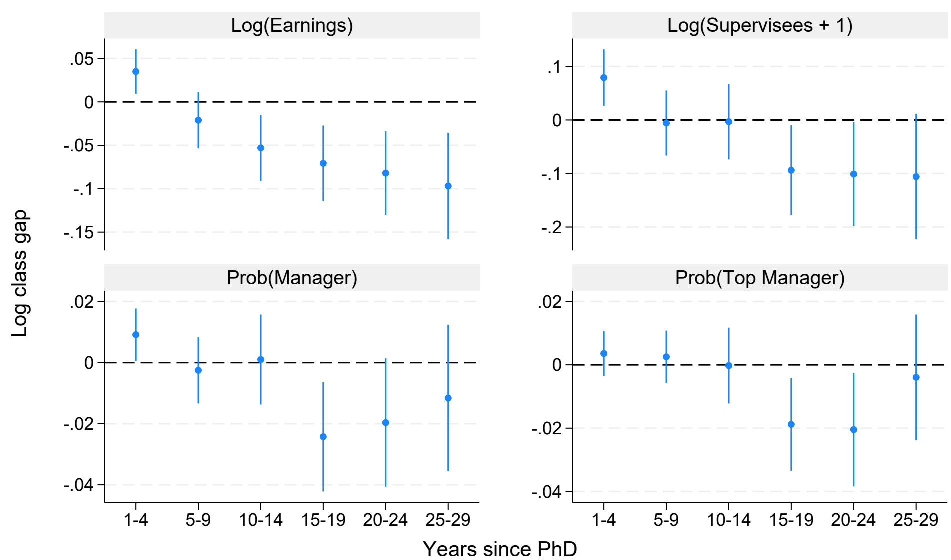
Source: SDR 2015-2021, matched with Web of Science, and NSF Awards. *Notes:* This figure shows kernel density plots of the residuals, by parental education group, from a regression of log tenure institution rank on our baseline fixed effects and full research controls. (replicating the regression in Table 3, Panel A, column 3, but excluding parental education). Regressions weighted by NSF-provided survey weight. Negative residuals reflect "overplacement" relative to a prediction based on research output and educational history.

Figure 4: Class gap in job satisfaction, by sector of employment



Source: SDR 1993-2021. *Notes:* Coefficient estimates and 95% confidence intervals from regressions of self-reported job satisfaction on parental education and our baseline fixed effects; only coefficients on first-gen college grads are plotted (relative to people with a parent with a non-PhD graduate degree). Regressions are run separately by sector; standard errors clustered at individual level; sample limited to people working in the US, less than 30 years since PhD receipt. Dep vars, listed on y-axis, are dummies taking value 1 if the individual is "very satisfied" with that aspect of their job, and 0 otherwise. (Int. challenge = intellectual challenge; Contr. to society = contribution to society).

Figure 5: Class gap in career progression in industry



Source: SDR 1993-2021. *Notes:* Coefficient estimates and 95% confidence intervals from regressions of dependent variables listed in subplot titles on parental education interacted with 5-year-group since PhD, and on our baseline fixed effects. Only coefficients on first-gen college grads are plotted (relative to people with a parent with a non-PhD graduate degree). Standard errors clustered at individual level; sample limited to people working in Industry in the US, less than 30 years since PhD receipt. Regressions weighted by NSF-provided survey weight. "Supervisees" = number of direct and indirect supervisees. "Prob(Manager)" and "Prob(Top Manager)" = dummies taking value 1 if occupation is any, or a top, managerial occupation respectively.

Online Appendix

A Appendix Tables and Figures

Table A1: Tenure Outcomes - Robustness - Alternate dependent variables

	(1) Tenure at Research Institution	(2) Tenure at R1 or R2	(3) Tenure at Top 50 (Grad rank)	(4) Tenure at Top 50 (Grad or BA)	(5) Tenure at Top 20 (Grad rank)	(6) Tenure Institution Log BA rank	(7) Tenure Institution Rank	(8) Tenure Institution BA Rank	(9) Tenure at R1 (tenured sample only)
Parental education (omitted category: non-PhD graduate degree)									
Less than college	-0.00813*	-0.0109**	-0.00883***	-0.00916***	-0.00513***	0.138***	4.296**	10.94***	-0.0423***
	(0.0045)	(0.0044)	(0.0031)	(0.0032)	(0.0020)	(0.031)	(1.87)	(2.55)	(0.011)
College	-0.00405	-0.00469	-0.00417	-0.00439	-0.00426*	0.0670*	1.151	4.474	-0.0151
	(0.0050)	(0.0049)	(0.0034)	(0.0035)	(0.0022)	(0.035)	(2.02)	(2.77)	(0.013)
PhD	0.0140**	0.0157***	0.0194***	0.0218***	0.0155***	-0.135***	-5.227**	-7.860***	0.0387***
	(0.0062)	(0.0061)	(0.0072)	(0.0049)	(0.0034)	(0.043)	(2.10)	(2.99)	(0.015)
Demographics FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
PhD Field FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
PhD Institution FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dep Var Mean	0.15	0.14	0.059	0.065	0.025	4.33	77.6	112.9	0.40
Observations	239,065	239,065	216,475	216,475	216,475	33,548	31,596	33,548	64,429
Unique Individuals	76,841	76,841	74,144	74,144	74,144	11,649	10,960	11,649	20,698
Absorbed DF	473	473	486	470	470	392	378	376	431

Source: SDR 1993-2021. *Notes:* Standard errors, clustered at the individual level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dep vars for cols 1-5 are binary variables taking value 1 if individual is tenured at a research institution (col 1), tenured at an R1 or R2 (col 2), tenured at a top-50 ranked institution by field-specific graduate program rank (col 3) tenured at a top-50 ranked institution by either graduate or undergraduate rank (col 4), and tenured at a top-20 ranked institution by graduate program rank (col 5), and 0 if in any other kind of job. All these include all working individuals, including those in non-tenured jobs in academia as well as jobs outside academia. Dep var for col 6 is the log undergraduate institution rank of the tenure institution. Dep vars for cols 7 and 8 are the field-specific graduate program rank or undergraduate institution rank of the tenure institution, respectively. (Ranks from *USNWR*). Dep var for col 9 is a binary variable taking value 1 if individual is tenured at an R1 and 0 if not tenured at an R1, but with the sample limited to tenured academics only (aka equivalent to Table 1, column 2, but with a more limited sample). Sample for all cols is restricted to people 10-30 years since PhD receipt, currently working in the US. Cols 1, 2 and 9 cover SDR years 1993-2021 and cols 3-8 years 1997-2021 inclusive. Sample in cols 6-8 is restricted only to those tenured at ranked institutions. Demographics FE are fixed effects for gender, race/ethnicity, and birth region. Regressions weighted by NSF-provided survey weight. Time FE are fixed effects for survey year, years since PhD, and PhD year (5-year group). "Absorbed DF" shows degrees of freedom absorbed by fixed effects.

Table A2: Getting tenure, conditional on tenure-track institution
(Sample: those on tenure track at ranked institutions)

Dependent variable: *(binary 1/0)*	(1) Tenure anywhere	(2) "Got tenure" (similar or higher ranked)	(3) Tenure at lower ranked	(4) Tenure track elsewhere	(5) Industry	(6) Government	(7) Non-TT education	(8) Not working
Parental education (omitted category: non-PhD graduate degree)								
Less than college	-0.0706***	-0.0625***	-0.00803	0.0139	0.0161	0.00779	0.0243	0.00842
	(0.022)	(0.023)	(0.011)	(0.011)	(0.012)	(0.0072)	(0.016)	(0.0084)
College	-0.0405*	-0.0373	-0.00321	0.00935	0.0155	0.00507	0.0157	-0.00513
	(0.023)	(0.025)	(0.011)	(0.012)	(0.012)	(0.0062)	(0.018)	(0.0076)
PhD	0.00756	0.0168	-0.00929	-0.0148	0.0178	-0.00761	-0.0171	0.0141
	(0.023)	(0.025)	(0.013)	(0.010)	(0.013)	(0.0048)	(0.017)	(0.0091)
Demographics FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
PhD Field FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Tenure-Track Inst. FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dep Var Mean	0.76	0.72	0.045	0.044	0.055	0.014	0.10	0.023
Observations	3,670	3,670	3,670	3,670	3,670	3,670	3,670	3,670
Absorbed DF	308	308	308	308	308	308	308	308

Source: SDR 1993-2021. *Notes:* Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dep vars are binary variables taking value 1 if the individual is working in that job type / industry in the next survey observation after the inferred tenure decision year. Outcomes in cols 2-8 are mutually exclusive and collectively exhaustive. (Col 2 refers to having tenure at the original tenure-track institution or an institution ranked higher or at most 5 rank points lower; this outcome is also shown in Table 2 column 4 in the main paper. Col 3 refers to having tenure at any other institution (ranked 5+ points lower or unranked). Col 4 refers to being on the tenure track without tenure at a new institution.) Sample restricted to those on the tenure track without tenure at ranked US institutions in the last survey observation before their inferred tenure decision year (and for which we observe at least 5 individuals at that institution). Regressions weighted by NSF-provided survey weights. Fixed effects are included for the tenure track institution. Demographics FE are fixed effects for gender, race/ethnicity, and birth region. Time FE are fixed effects for survey year, years since PhD (5-year group), and PhD year (5-year group). "Absorbed DF" shows degrees of freedom absorbed by fixed effects. Analogous outcomes for those on tenure track at non-ranked institution shown in Appendix Table A3.

Table A3: Getting tenure, conditional on tenure-track institution
(Sample: those on tenure track at non-ranked institutions)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	Tenure anywhere	Tenure at same institution	Tenure at R1	Tenure track elsewhere	Industry	Government	Non-TT education	Not working
Parental education (omitted category: non-PhD graduate degree)								
Less than college	-0.0378	-0.0422	-0.00476	-0.0141	-0.00709	-0.00446	0.0354*	0.0281**
	(0.030)	(0.031)	(0.0078)	(0.017)	(0.014)	(0.0085)	(0.019)	(0.012)
College	-0.0165	-0.0544	0.00790	-0.0146	0.00537	-0.00516	0.0120	0.0189*
	(0.032)	(0.034)	(0.0096)	(0.019)	(0.016)	(0.0090)	(0.020)	(0.010)
PhD	-0.0691*	-0.108***	0.0121	-0.00818	0.0204	0.0153	0.0299	0.0117
	(0.037)	(0.039)	(0.014)	(0.017)	(0.019)	(0.017)	(0.023)	(0.012)
Demographics FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
PhD Field FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Tenure-Track Inst. FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dep Var Mean	0.83	0.78	0.027	0.047	0.034	0.014	0.060	0.016
Observations	1,559	1,559	1,559	1,559	1,559	1,559	1,559	1,559
Absorbed Degrees of Freedom	373	373	373	373	373	373	373	373

Source: SDR 1993-2021. *Notes:* Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dep vars are binary variables taking value 1 if the individual is working in that job type / industry in the next survey observation after the inferred tenure decision year. Outcomes in columns 1 and 4-8 are mutually exclusive and collectively exhaustive. Sample restricted to those on the tenure track without tenure at non-ranked US institutions in the last survey observation before their inferred tenure decision year (and for which we observe at least 5 individuals at that institution). Regressions weighted by NSF-provided survey weights. Demographics FE are fixed effects for gender, race/ethnicity, and birth region. Time FE are fixed effects for survey year, years since PhD (5-year group), and PhD year (5-year group). "Absorbed DF" shows degrees of freedom absorbed by fixed effects. Analogous outcomes for those on tenure track at ranked institution shown in Appendix Table A2.

Table A4: Research output of tenured professors, conditional on PhD institution and field

	(1) Publications (fspr)	(2) First Author Pubs (fspr)	(3) Last Author Pubs (fspr)	(4) Average CNCI (fspr)	(5) Average Impact Factor (fspr)	(6) NSF Awards (buckets)	(7) Top 10% CNCI Share	(8) High Impact Journal Share	(9) Average Authors Per Pub (fspr)
Parental education (omitted category: non-PhD graduate degree)									
Less than college	-0.0419***	-0.0240***	-0.0428***	-0.0339***	-0.0232***	-0.0913***	-0.0148***	-0.0123**	-0.0192**
	(0.0087)	(0.0090)	(0.0083)	(0.0091)	(0.0085)	(0.032)	(0.0050)	(0.0056)	(0.0089)
College	-0.0181*	-0.0144	-0.0213**	-0.0130	0.0121	0.0133	-0.00644	0.00738	-0.00138
	(0.0094)	(0.0096)	(0.0089)	(0.0095)	(0.0090)	(0.035)	(0.0055)	(0.0063)	(0.0096)
PhD	0.0105	0.000339	0.0132	0.0104	0.0231**	0.0322	-0.00309	0.0130*	0.00710
	(0.011)	(0.011)	(0.010)	(0.011)	(0.011)	(0.040)	(0.0062)	(0.0071)	(0.011)
Demographics FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
PhD Field FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
PhD Institution FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	12,296	12,296	12,296	12,296	12,077	12,296	12,296	12,296	12,296
R-Squared	0.21	0.15	0.25	0.16	0.25	0.27	0.14	0.26	0.15
Adjusted R-Squared	0.18	0.13	0.22	0.14	0.22	0.24	0.11	0.24	0.12
Absorbed DF	371	371	371	371	371	371	371	371	371

Source: SDR 2015-2021, matched with Web of Science and NSF Awards. *Notes:* Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Sample restricted to 2015 SDR respondents who were tenured at a US institution in 2015, or in the first SDR year we observe them with tenure after this year (2017, 2019, or 2021), matched to their publication record as of 2015 (for 2015 observations) or 2017 (for 2017, 2019, or 2021 observations). Cols 1-3 reflect the field-specific percentile rank ('fspr') for the number of total publications, first author publications, and last author publications respectively. Col 4 is the field-specific percentile rank of the average CNCI across all publications, where CNCI is the category normalized citation count taking into account field and publication type. Col 5 is the field-specific percentile rank of the average journal impact factor across all publications. Col 6 is a categorical variable that separates number of NSF awards in to 0, 1, 2 or 3, and greater than 4. Cols 7 & 8 are the share of publications that were in the top 10% CNCI for the field and year of publication, or in a high impact journal, respectively. Col 9 is the field-specific percentile rank 9 of the average authors per publication. Regressions weighted by NSF-provided survey weight. Demographics FE are fixed effects for gender, race/ethnicity, and birth region. Time FE are fixed effects for survey year, years since PhD (5-year group), and PhD year (5-year group). "Absorbed DF" shows degrees of freedom absorbed by fixed effects.

Table A5: Tenure outcomes with research controls - robustness

	No research controls	With research controls	
	(1)	(2)	(3)
Panel A: Tenure institution rank (log)			
Less than college	0.149***	0.111***	0.112***
	(0.046)	(0.043)	(0.042)
College	0.0455	0.0583	0.0664
	(0.049)	(0.045)	(0.044)
PhD	-0.104*	-0.0841*	-0.0703
	(0.055)	(0.050)	(0.049)
Observations	6,969	6,920	6,920
R-Squared	0.24	0.35	0.37
Adjusted R-Squared	0.20	0.32	0.33
PhD Institution FE	Yes	Yes	Yes
Panel B: Got tenure, conditional on tenure-track institution			
Less than college	-0.0635***	-0.0582**	-0.0536**
	(0.022)	(0.024)	(0.023)
College	0.00959	-0.0106	-0.00523
	(0.023)	(0.025)	(0.024)
PhD	0.00859	-0.00888	-0.0181
	(0.022)	(0.024)	(0.024)
Observations	1,907	1,894	1,894
R-Squared	0.61	0.64	0.67
Adjusted R-Squared	0.54	0.58	0.60
TT Institution FE	Yes	Yes	Yes
Demographics FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
PhD Field FE	Yes	Yes	Yes
Baseline Research Controls		Yes	Yes
Add'l Research Controls			Yes

Source: SDR 2015-2021, matched with Web of Science and NSF Awards. *Notes:* Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Replicates Table 3, but using the raw numbers rather than field-specific percentile rank for research control variables in Columns 2 and 3.

Table A6: Citations per publication

	(1)	(2)	(3)	(4)	(5)
Dep. var	Any Cites	Log(Cites(5y))	Log(1+Cites(5y))	Log(CNCI)	Log(1+CNCI)
Panel A: Controlling for third-order polynomial in journal impact factor					
Less than college	-0.00518***	-0.0373**	-0.0417**	-0.0386**	-0.0480**
	(0.0020)	(0.017)	(0.016)	(0.019)	(0.020)
College	-0.00556**	-0.0186	-0.0274*	-0.0369**	-0.0476**
	(0.0022)	(0.016)	(0.016)	(0.019)	(0.021)
PhD	-0.00138	0.00583	0.00209	-0.0125	-0.0145
	(0.0022)	(0.019)	(0.018)	(0.021)	(0.023)
Panel B: Controlling for decile of journal impact factor interacted with broad PhD field					
Less than college	-0.00430**	-0.0296*	-0.0333**	-0.0334*	-0.0408**
	(0.0018)	(0.017)	(0.016)	(0.019)	(0.020)
College	-0.00485**	-0.0147	-0.0223	-0.0356**	-0.0447**
	(0.0021)	(0.016)	(0.015)	(0.018)	(0.020)
PhD	-0.00150	0.00817	0.00419	-0.0112	-0.0143
	(0.0021)	(0.018)	(0.017)	(0.021)	(0.022)
Dep Var Mean	0.95	2.50	2.51	-0.20	4.28
Observations	261,443	248,515	261,443	252,760	261,443
Unique individuals	11,428	11,060	11,428	11,187	11,428
Demographic FE	Yes	Yes	Yes	Yes	Yes
Institution FE	Yes	Yes	Yes	Yes	Yes
Pub. Year FE	Yes	Yes	Yes	Yes	Yes
Pub. Field FE	Yes	Yes	Yes	Yes	Yes
Pub. Type FE	Yes	Yes	Yes	Yes	Yes
Num. Authors FE	Yes	Yes	Yes	Yes	Yes

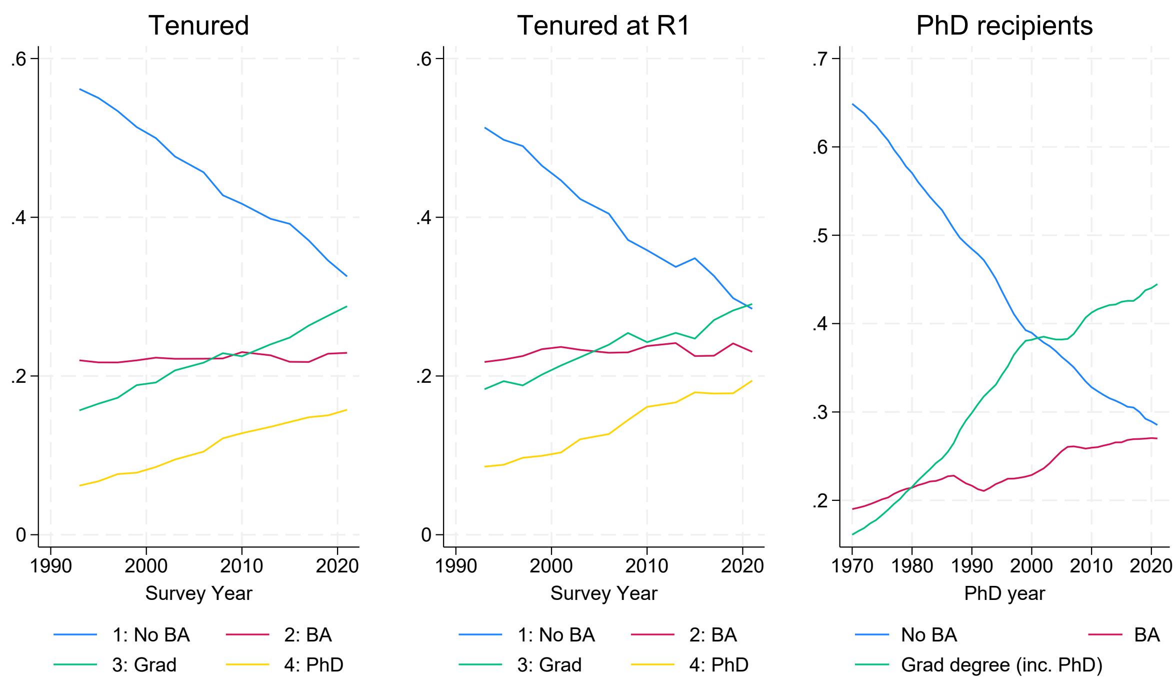
Source: Web of Science bibliometric data, matched with 2015 SDR. *Notes:* Standard errors clustered at individual level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. This Table shows the coefficients on parental education in regressions of citations per publication on the parental education level of the author, alongside various controls. The regressions are run at the publication-author level. The dependent variables are, respectively: (1) Any Cites = a binary variable taking the value 1 if the publication has any citations in the first 5 years, and 0 otherwise; (2) Log Cites(5y) = the log of the number of citations in the first 5 years; (3) Log(1+Cites(5y)) = the log of 1 + the number of citations in the first 5 years; (4) Log(CNCI) = the log of the CNCI for the publication; (5) Log(1+CNCI) = log of 1 + CNCI. Sample is restricted to academics that were on the tenure track at a US institution in the 2015 SDR, and to publications which were Articles or Reviews, from 1997 onward (which is the first year we have access to impact factor information). Demographics FE are birth region, gender, and race/ethnicity, alongside seniority (5-year bucket between publication year and PhD receipt). Institution FE are fixed effects for the author's academic institution of employment as of 2015 SDR. Pub Type FE are fixed effects for publication type (article or review), and indicators for whether the publication was in a high impact or a low impact journal (or neither). Pub Field reflects a narrow categorization of the publication's primary field, per Clarivate. Num. Authors FE is a fixed effect for the number of authors (separated into buckets of : 1, 2, 3, 4, 5-9, 10-19, 20-49, and 50+). Panel A also has controls for a third order polynomial in the impact factor of the publication. Panel B instead has fixed effects for the decile of the publication impact factor interacted with the broad PhD field. Weighted by NSF provided survey weights.

Table A7: Tenure outcomes of US SEH PhD recipients, by parental education group (Sample: 2021 SDR, PhD recipients 1991-2011)

Parental education	Share tenured anywhere	Share tenured at R1	Share tenured at top 50
Less than college	21.8%	7.6%	3.9%
College	21.8%	8.6%	4.5%
Non-PhD grad degree	24.9%	10.0%	6.2%
PhD	28.7%	14.1%	10.0%

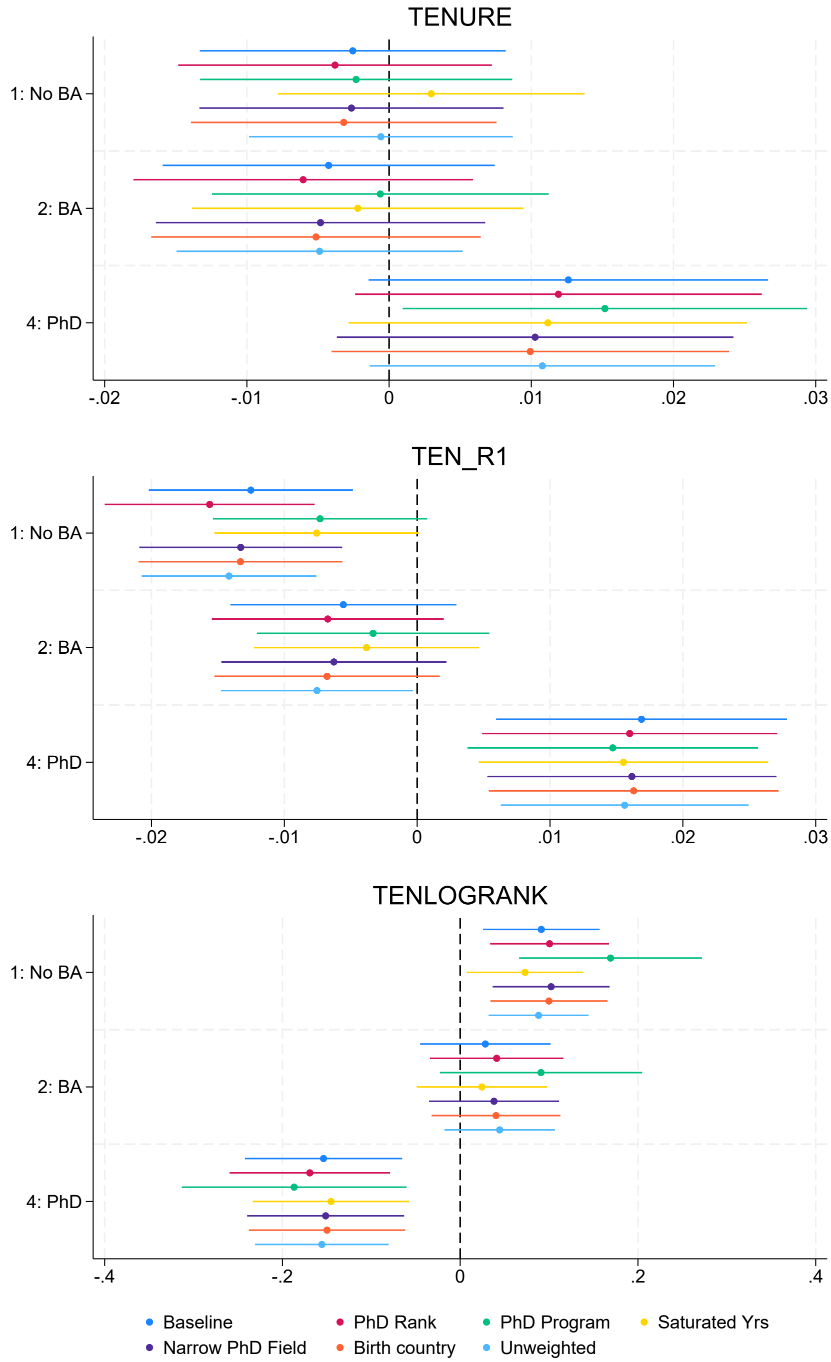
Source: SDR 2021, matched with SED 2021. *Notes:* Sample restricted to those in the 2021 SDR, who are 10-30 years since PhD receipt and working in the US. Table shows shares among each parental education group who are tenured, tenured at an R1 institution, and tenured at a top 50 ranked institution (per *USNWR* grad program rank), respectively. Weighted by NSF-provided survey weights.

Figure A1: Parental education shares of tenured professors, tenured professors at R1s, and PhD recipients (sample limited to those with US PhDs in SEH fields)



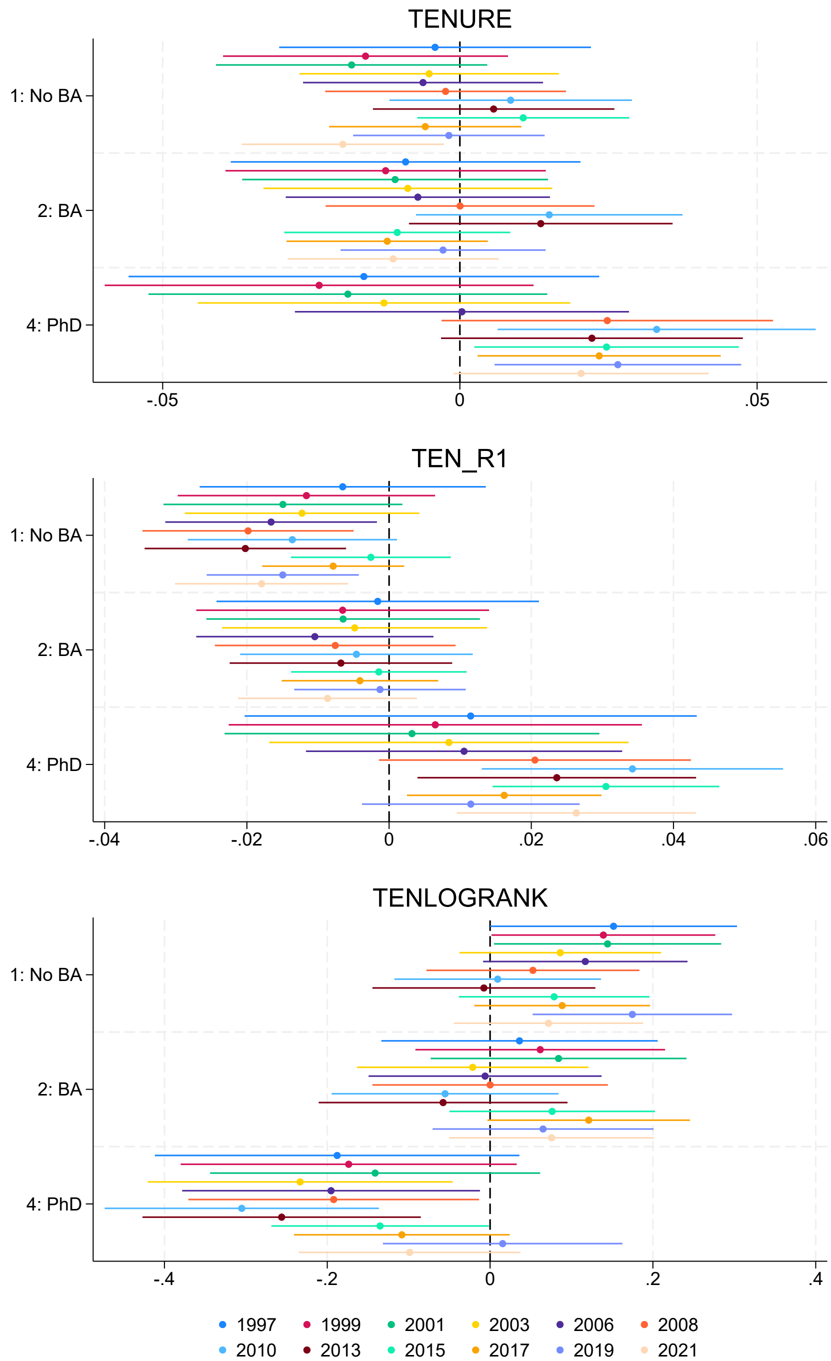
Source: Tenured professors: SDR 1993-2021, matched with SED 2021. PhD recipients: SED 2021. *Notes:* Figures show the share of each group (tenured profs, tenured at R1s, and PhD recipients) who have each level of parental education. Tenured professor sample limited to those tenured at US institutions and weighted by NSF-provided survey weights. Figures only show people with a US PhD in a Science, Engineering, or Health field (including social sciences), because of SDR and SED sample restrictions. PhD recipients figure combines people with a parent with a non-PhD graduate degree and with a PhD into one category.

Figure A2: Tenure outcomes - Robustness - Alternate fixed effects or weights



Source: SDR 1993-2021. *Notes:* Each sub-plot is a coefficient plot showing coefficients and 95% confidence intervals from regressions of the dependent variable indicated in the sub-plot titles on parental education, as well as our baseline fixed effects as in Table 1. Dependent variables are: TENURE = tenure anywhere, TEN_R1 = tenure at R1 institution, TENLOGRANK = log rank of tenure institution. Dependent variables and sample restrictions are as in Table 1. Each color represents a different regression specification, which modifies our baseline specification in some way. All controls and fixed effects are as in Table 1 except the modifications, listed in order: Dark blue: Baseline. Pink: PhD Rank FEs instead of PhD institution FEs. Green: PhD Program FE (institution X field X decade) instead of PhD institution and PhD field FEs. Yellow: Saturated survey year, age, PhD year, and years since PhD FEs. Purple: Narrowest PhD field category FE instead of baseline PhD field category. Orange: Birth country FE instead of birth region FE. Light blue: Unweighted regressions.

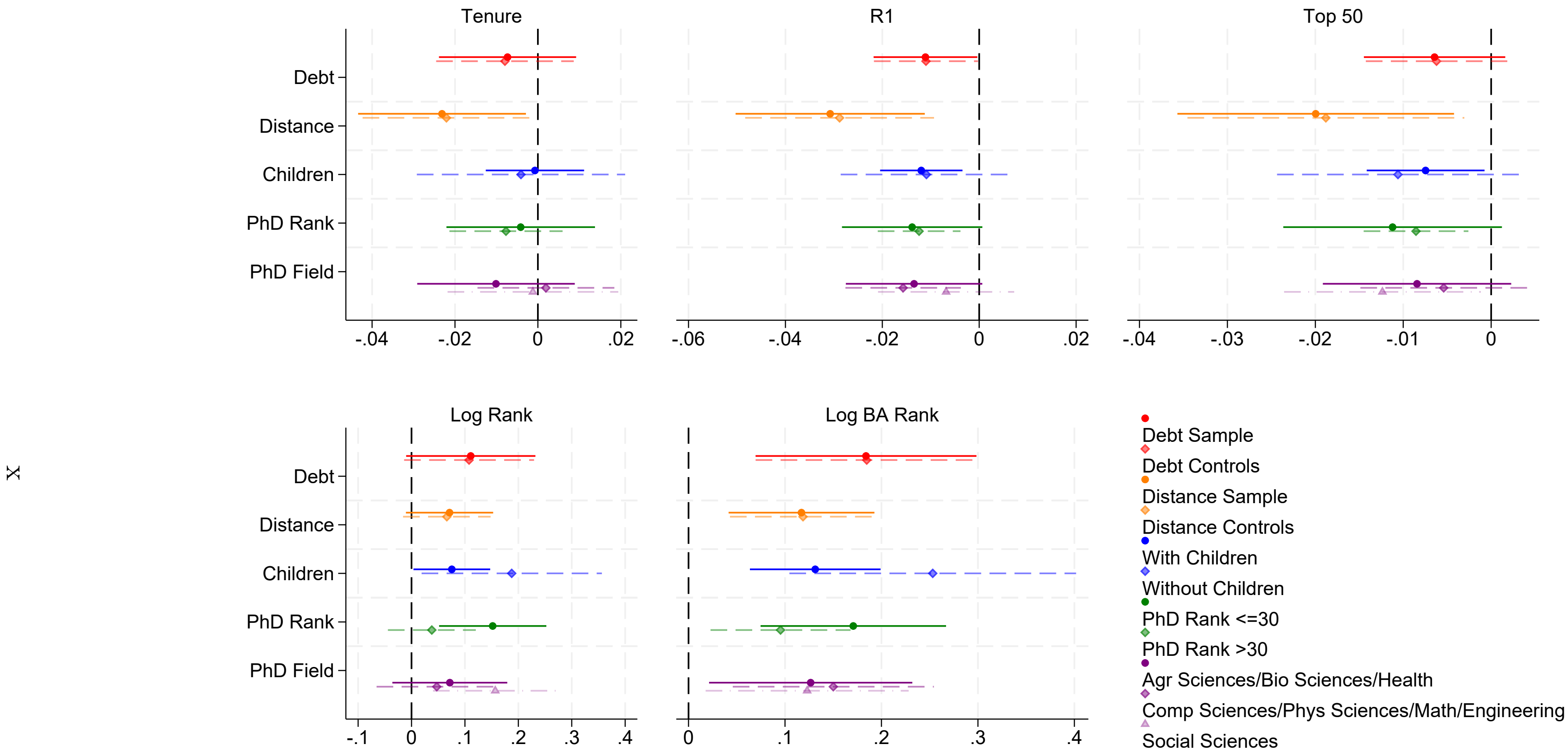
Figure A3: Tenure outcomes - Robustness - Year-by-year regressions



Source: SDR 1997-2021. *Notes:* Each sub-plot is a coefficient plot show coefficients and 95% confidence intervals from regressions of the dependent variable in the sub-plot title on parental education, as well as our baseline fixed effects as in Table 1. Dependent variables are: TENURE = tenure anywhere, TEN_R1 = tenure at R1 institution, TENLOGRANK = log rank of tenure institution. Each color represents a regression run on one specific survey year, as denoted in the legend. Results are also similar for 1993 and 1995 for our first two dependent variables - tenure anywhere, and tenure at an R1. We do not have results for 1993 and 1995 for our dependent variables that rely on rank data due to a change in the institution coding in the SDR between the 1995 and 1997 surveys.

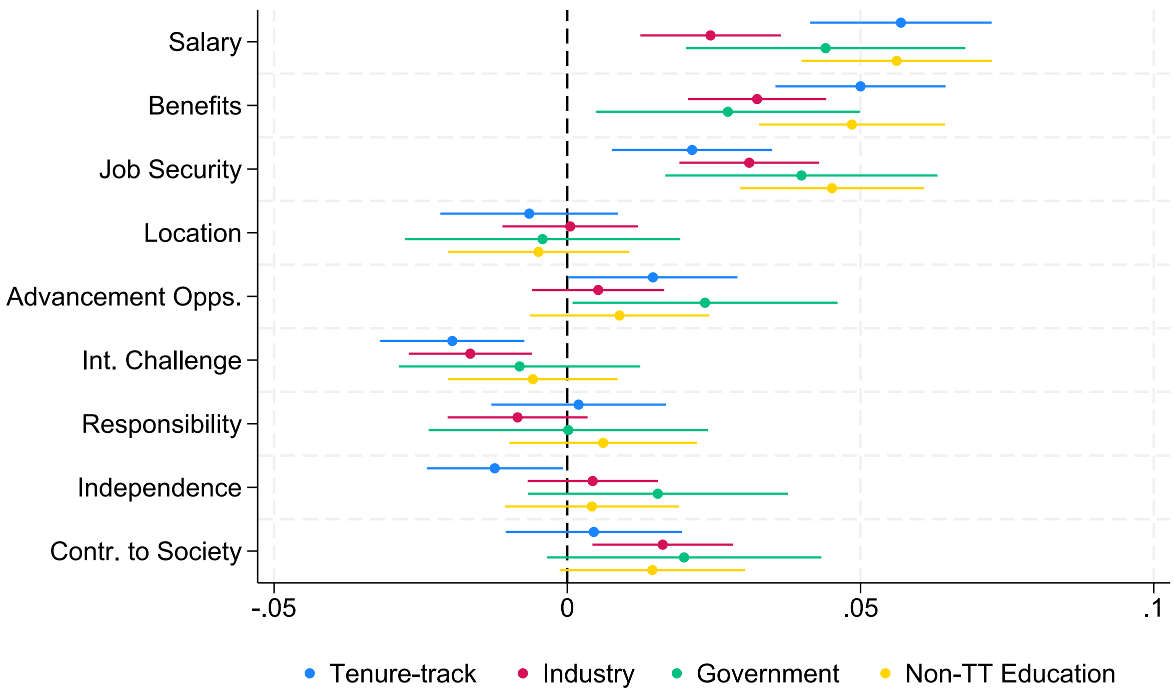
Figure A4: Class gap in tenure outcomes - Heterogeneity

Difference in outcomes between first-gen college graduates and people with a parent with a non-PhD graduate degree



Source: SDR 1993-2021. *Notes:* Each sub-plot is a coefficient plot showing coefficients and 95% confidence intervals from regressions of the dependent variable (sub-plot title) on parental education and our baseline fixed effects, with regressions weighted by survey weights and standard errors clustered at individual level. Only the coefficient on first-generation college graduates is shown, with the omitted category being people with no parent with a graduate degree. Dependent variables are our three baseline (Tenure anywhere, Tenure at R1, and Log Tenure Institution Rank) plus two additional robustness checks (Tenure at a top-50 ranked institution, and Log Tenure Institution Undergraduate (BA) Rank). The five categories on the y-axis show five different axes of heterogeneity: *Debt*: baseline regressions, with sample limited to those with information on student debt levels ("debt sample") and adding controls for a third order polynomial in total student debt level ("debt controls"). *Distance*: baseline regressions, with sample limited to those with information on high school state ("distance sample") and adding controls for a third order polynomial in distance between current employer city and high school state using population-weighted centroids ("distance controls"). *Children*: baseline regressions run separately for those who ever have, or never have, children in our linked SED-SDR dataset. *PhD Rank*: baseline regressions run separately for those who did their PhD at a program ranked 1-30, or greater than 30, on the most recent US News and Report graduate program rankings. *PhD Field*: baseline regressions run separately for three PhD field groups. See Appendix C for field group definitions.

Figure A5: Class gap in perceived importance of job components: Difference between first-generation college graduates and people with a parent with a non-PhD graduate degree, conditional on our baseline controls



Source: Survey of Doctorate Recipients 1993-2021 inclusive, matched with Survey of Earned Doctorates.
Notes: The coefficient plot shows results of regressions of a series of dependent variables for perceived importance of each component of a job on parental education, alongside our baseline fixed effects. Only the coefficients on first-generation college graduates are plotted, with the omitted category being people with a parent with a non-PhD graduate degree. Each coefficient plotted shows the point estimate and 95% confidence interval. Regressions are weighted by the NSF provided survey weight, and have standard errors clustered at the individual level. Sample is limited only to people working the US, less than 30 years since PhD receipt. The dependent variables, listed on the y-axis, are binary variables taking the value 1 if the individual reports that each aspect of a job is "very important" to them, and 0 otherwise. (Int. challenge = intellectual challenge; Contr. to society = contribution to society). These regressions are run separately for each of the four sectors of employment – tenure-track academia, industry, government, and non-tenure track education – indicated by the coefficient colors as detailed in the legend.

B Appendix: Gender and Race/Ethnicity Coefficients

Table B1: Gender and race/ethnicity coefficients from Table 1

	(1)	(2)	(3)
Dep. var.	Tenure anywhere	Tenure at R1	Tenure institution rank (log)
Gender (omitted category: male)			
Female	-0.0248***	-0.0133***	-0.0112
	(0.0046)	(0.0032)	(0.029)
Race/ethnicity (omitted category: White Non-Hispanic)			
Asian, Non-Hispanic	-0.0452***	-0.0125*	-0.0767
	(0.010)	(0.0071)	(0.071)
Black, Non-Hispanic	0.0436***	0.0104	0.0613
	(0.011)	(0.0074)	(0.066)
Hispanic, All Races	0.0311***	-0.00140	0.193***
	(0.0100)	(0.0073)	(0.056)
Other, Non-Hispanic	-0.0295**	-0.0133	0.0493
	(0.015)	(0.0095)	(0.098)

Source: SDR 1993-2021. *Notes:* Standard errors clustered at individual level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Shows gender and race coefficients from Table 1.

Table B2: Gender and race/ethnicity coefficients from Table 2

Juncture	PhD to tenure track			Tenure track to tenure
	(1)	(2)	(3)	(4)
Dep. var.	TT anywhere	TT at R1	TT inst. Rank (log)	Got Tenure
Gender (omitted category: male)				
Female	-0.0134***	-0.0105***	0.0437	-0.0211
	(0.0037)	(0.0024)	(0.030)	(0.019)
Race/ethnicity (omitted category: White Non-Hispanic)				
Asian, Non-Hispanic	-0.0448***	-0.0134***	-0.0253	0.0473
	(0.0072)	(0.0051)	(0.073)	(0.046)
Black, Non-Hispanic	0.0294***	0.0268***	-0.205***	-0.141***
	(0.0085)	(0.0055)	(0.066)	(0.041)
Hispanic, All Races	0.0370***	0.00802	-0.0211	-0.0102
	(0.0076)	(0.0051)	(0.061)	(0.038)
Other, Non-Hispanic	-0.0174*	-0.00712	0.0265	0.0307
	(0.0100)	(0.0063)	(0.086)	(0.054)

Source: SDR 1993-2021. *Notes:* Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Shows gender and race coefficients from Table 2.

Table B3: Gender and race/ethnicity coefficients from Table 3

	No research controls	With research controls	
	(1)	(2)	(3)
Panel A: Tenure institution rank (log)			
Gender (omitted category: male)			
Female	-0.0247	-0.0918***	-0.0881**
	(0.039)	(0.035)	(0.035)
Race/ethnicity (omitted category: White Non-Hispanic)			
Asian, Non-Hispanic	0.127	0.0690	0.0932
	(0.10)	(0.097)	(0.095)
Black, Non-Hispanic	0.0974	-0.198**	-0.181**
	(0.092)	(0.081)	(0.083)
Hispanic, All Races	0.0644	-0.0500	-0.0655
	(0.070)	(0.065)	(0.065)
Other, Non-Hispanic	0.00184	-0.125	-0.106
	(0.10)	(0.100)	(0.10)
Panel B: Got tenure, conditional on tenure-track institution			
Gender (omitted category: male)			
Female	-0.00377	0.00598	0.0122
	(0.018)	(0.018)	(0.018)
Race/ethnicity (omitted category: White non-Hispanic)			
Asian, Non-Hispanic	0.0885**	0.0889**	0.0660
	(0.044)	(0.044)	(0.042)
Black, Non-Hispanic	-0.0280	-0.00252	-0.0105
	(0.041)	(0.040)	(0.042)
Hispanic, All Races	-0.00854	0.00165	0.00652
	(0.038)	(0.038)	(0.038)
Other, Non-Hispanic	0.0542	0.0607	0.0659
	(0.047)	(0.042)	(0.045)

Source: SDR 2015-2021, matched with Web of Science and NSF Awards. *Notes:* Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Shows gender and race coefficients from Table 3.

Table B4: Gender and race/ethnicity coefficients for Table 6

Dep. var: Log earnings	(1)	(2)	(3)	(4)	(5)
Sector	Tenure-track academia	Industry	Government	Non-tenure-track education	Tenure track, w/ institution FEs
Gender (omitted category: male)					
Female	-0.0922***	-0.268***	-0.0897***	-0.213***	-0.0735***
	(0.0061)	(0.0087)	(0.010)	(0.0091)	(0.0054)
Race/ethnicity (omitted category: White Non-Hispanic)					
Asian, Non-Hispanic	0.0173	0.00864	-0.0366*	0.0148	-0.0138
	(0.016)	(0.017)	(0.021)	(0.021)	(0.013)
Black, Non-Hispanic	0.000576	-0.0245	-0.0159	0.0182	0.0122
	(0.014)	(0.020)	(0.021)	(0.022)	(0.012)
Hispanic, All Races	-0.00613	-0.0531***	-0.0186	-0.0595***	0.00461
	(0.011)	(0.017)	(0.018)	(0.018)	(0.010)
Other, Non-Hispanic	-0.00603	-0.00766	-0.0529**	-0.0180	-0.00585
	(0.024)	(0.023)	(0.025)	(0.027)	(0.019)

Source: SDR 1993-2021. *Notes:* Standard errors, clustered at individual level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Shows gender and race coefficients from Table 6.

C Appendix: Data

Parental Education: The Survey of Earned Doctorates (SED) asks respondents the highest level of education of each parent.[49] We create a 4-level categorical variable reflecting the highest level of education between the two parents, or the level of education of one parent if only one is reported. The four categories are mutually exclusive and collectively exhaustive: No parent with a bachelor's degree or higher, at least one parent with a bachelor's degree / four-year college degree, at least one parent with a graduate degree that is not a PhD, and at least one parent with a PhD. 9.7% of individuals in our 1993-2021 SDR sample have no information on parental education; we drop them from all analyses.

Gender: We use the GENDER variable from the SDR which indicates if the respondent reports identifying as male or female at the time of the survey. Among those who are not missing parental education, there are no observations missing gender. 64% are male.

[49] Prior to 2018, the SED asked respondents specifically to indicate the highest level of education of their mother and father. Starting in 2018, respondents were asked to report up to two parents' or guardians' highest level of education, regardless of gender. In either instance, the two variables used to report parental education are EDFATHER and EDMOTHER.

Race: We construct a 5-level categorical variable from the SED, shown here with their share of observations listed in parentheses: White Non-Hispanic (64.9%), Black Non-Hispanic (5.7%), Asian Non-Hispanic (19.5%), Other Non-Hispanic (2.3%), and Hispanic All Races (7.6%). Other Non-Hispanic includes American Indian/Alaskan Native, Multiple races, and individuals who didn't answer the RACE question in the SED but indicated not Hispanic on the Hispanic indicator. We drop any individuals who report neither race nor ethnicity, which accounts for an additional 55,619 observations (9% of the sample who are not missing parental education). We follow the National Science Foundation classification in defining Under-Represented Minorities (URM) as Black Non-Hispanic or Hispanic of All Races.

Birth Region: The SED reports country of birth for those born outside the US and state of birth for those born within the US. We construct our variable "birth region" as a 14-level categorical variable, with categories as follows (and their share of observations listed in parentheses): US (68.1%), Eastern Europe (1.9%), Western Europe (3.2%), East Asia (11.2%), Southeast Asia (1.4%), South Asia (5.0%), West and Central Asia (1.5%), North America excluding the US (2.0%), South America (2.4%), Central America (0.4%), Caribbean (0.7%), Africa (1.9%), Oceania (0.3%), and missing (0.15%).

PhD Field: We use four different levels of granularity of PhD field:

- *PhD Field Group:* 3 categories: Biological Sciences (includes Health, Agricultural, Environmental), Physical Sciences (includes Math, Computer Science, Engineering), Social Sciences (includes Psychology). We use this categorization in Table 3 (interacting our research control variables with PhD Field Group) and for heterogeneity analysis in Appendix Figure A4.
- *Broad PhD Field:* 10 categories. This follows the NSF's grouping of PhD fields into broad fields, but breaks out Economics separately from the other Social Sciences. The categories are: Agricultural and Environmental Sciences; Biological Sciences; Health Sciences; Engineering; Computer and Information Sciences; Mathematics and Statistics; Physical, Geological, Atmospheric, and Ocean Sciences; Psychology; Social Sci-

ences excluding Economics; Economics. We use this to calculate field-specific percentile ranks of our research output measures (publications, CNCI, journal impact factor, etc) as used in Table 3 and Appendix Table A4.

- *PhD Field:* 75 categories. **This is our baseline PhD field definition**, which we use for our fixed effects in all regressions. We construct this field definition using the first two digits of the NSF's 3-digit PhD field classification (described below).
- *Narrow PhD Field:* 267 categories. This is the NSF-provided PhD field definition (and thus the narrowest classification available in our data). The full list is available in the SED 2021 codebook Appendix F ("Historical SED Field of Study/Specialties List"). We use narrow PhD field fixed effects in a robustness check in Table A1.

Earnings: The SDR earnings variable indicates total earned income before deductions in the year prior to the survey. Earnings is available starting in survey year 1995. We adjust earnings to 2021 US dollars using the CPI. Among all individuals in our sample (1995-2021) who were working in the US, 0.56% are missing earnings. For those in the 2021 SDR, the median earnings was $115,000, the 25th percentile $80,000, and the 75th percentile $168,646.

Debt: In recent years the SED has asked individuals their level of undergraduate and graduate debt. Over 90% of respondents with PhDs in 2000 or later have information for undergraduate and graduate debt. These variables designate debt in five or ten thousand dollar buckets. We impute debt using the midpoint of all buckets; for the highest buckets, we impute a value. We then add imputed undergraduate and graduate debt.

Institution code imputation: The SDR includes institution codes (IPEDS codes) for the institution at which each respondent is currently employed if they are working for an academic institution in the survey year. This variable, INSTCOD, however, is occasionally missing and/or uses outdated institution codes in earlier years. Since institution codes are important for our analyses (for fixed effects and ranks/R1 status), we impute INSTCOD where we can. This primarily involved imputing institution code for survey waves w where the institution code is missing in wave w, but the is individual at the same institution in waves $w-1$ and $w+1$, and has the same tenure status in all three waves.

Institution ranks: Our core measure of institution rank is the field-specific graduate program rank from *US News and World Report.* In 2023 we downloaded the most recent program rankings for as many relevant fields as possible: audiology, biology, business, chemistry, computer science, criminology, earth science, economics, engineering, history, mathematics, medicine, nursing, physics, political science, psychology, public health, public policy, sociology, and statistics. We then imputed ranks for the fields which did not have *USNWR* rankings, using the average rank for each broad PhD field for each institution (weighting the field-specific ranks by the number of individuals in each of those fields at that institution). This means for example that an institution which has an average ranking of N across the social sciences for which we have ranks would also receive that same rank N in the smaller social sciences for which we are missing ranks (like anthropology, gender studies, area studies, and demography). We then merge these field-specific ranks into our SED-SDR data using the PhD field of the individual in question. Note that this will give us accurate rankings for the PhD program an individual attended, but means that if someone is employed in an academic department which is *not* the field of their PhD, we may erroneously impute a higher or lower field-specific rank for their institution than is appropriate for the department they are employed in. In all we obtain PhD program ranks for 94.3% of individuals, and institution ranks for 48% of the individuals on the tenure track at a named US institution.[50]

Carnegie classifications: One of our main dependent variables is whether an institution is an R1. This is a Carnegie classification, which defines R1 institutions as doctorate-granting institutions that have very high research activity.[51] We primarily use the 2015 Carnegie

[50]Since we do not have rankings for all institutions, we also supplement our field-specific rankings with US News and World Report's 2022 undergraduate institution rankings in a robustness check. We observe undergraduate institution ranks for 52% of the individuals we observe on the tenure track at a named US institution. The institutions for which there are ranks on one or the other metric are not fully overlapping: around 4.5% of tenure-track observations at a named US institution have a field-specific graduate program ranking but no undergraduate institution ranking, and about 8.5% for the converse.

[51]An R2 institution is a doctorate-granting institution that has high research activity, and a research institution is any doctorate-granting doctoral or professional university. The Carnegie Commissions use measures such as research expenditure, number of research doctorates awarded, and number of research-focused faculty to determine the level of research activity at institutions.

Classification, but supplement it with the 1994 Carnegie Classification. We have Carnegie classifications for 99% of individuals who are on the tenure track at a named US institution.

Inferring tenure decision dates: Most individuals in the SDR data answer no more than two or three surveys throughout the 1993-2021 sample period. Therefore we frequently only observe snapshots of someone's career: we always observe their PhD year and information, and then observe a few later snapshots in later surveys (the median person in our data answers 3 surveys; the 90th percentile is 7). For our main "got tenure" analysis (Table 2, column 4), we need to know when someone received tenure. For tenured faculty who filled out the SDR in 2010 or later, the year they received tenure is asked directly. For other tenured faculty, and for anyone in other positions (non-tenure track, tenure-track without tenure, or employed outside academia), there is no question asking whether or when a tenure decision was taken. As such, we need to infer the likely tenure decision year for this group. We restrict our sample to individuals who we observe in a non-tenured tenure-track academic job and then later again after the tenure decision has likely occurred. We define year t as the last year in which we observe individual p in a non-tenured tenure-track job at institution i. If year t is more than 5 years since the individual's PhD receipt, and if we observe the individual again in a different position no more than 5 years later in another SDR survey wave, we denote year $t+1$ the likely tenure decision year. (Typically, there are two years between SDR survey waves, so this gives us the year between the two closest observations of the same individual). For about 5% of individuals in this sub-sample, this process gives us more than one tenure decision year. This could reflect moves where an individual left a tenure-track job without facing a tenure decision, *or* moves where an individual left a tenure-track job because they did not get tenure. In our baseline analyses, we limit to the last tenure decision year we observe.

Web of Science bibliometric data: To construct publication-level variables, we gained access to author-publication level Web of Science bibliometric data. Web of Science (WoS) is a widely used database of bibliographic and citation information for over 250 fields and over 21,000 journals, conferences, and books from 1900 to present. The WoS data was linked to all individuals in the 2015 SDR by NCSES (Ginther et al., 2023). This data contains metrics for items published from January 1990 to December 2017.

NSF Award data: NCSES has matched data on all NSF Awards awarded to individuals in the 2015 SDR survey. Our baseline variable using this data is a categorical variable of the number of NSF awards broken down into 0 awards, 1 award, 2 or 3 awards, and 4 or more awards. For our baseline analysis sample with research controls (e.g. Table 3), 66% of the sample have no NSF awards, 10% have 1 NSF award, 10% have 2-3 NSF awards, and 14% have 4 or more NSF awards.

D Appendix: Additional Discussions

D.1 Differences in endowments of research ability within PhD program

Consider in what ways the endowment of research ability might differ, by SEB, for two graduates of the same PhD program. Note that both of these individuals were admitted to and chose to enter the same PhD program, suggesting both that (i) the admissions committees deemed them relatively similar on future research ability and (ii) the individuals thought this program was their best available option. Denote the information observed by the admissions committee as the vector $\mathbf{s}$, which they aggregate into index S and use to form an expectation of future research ability r.

First, it is possible that the admissions committee uses different cutoff rules for lower-SEB vs. higher-SEB students, such that for high-SEB students, all students with $\underline{S_1} < S < \bar{S}$ are admitted, but for low-SEB students, all students with $\underline{S_2} < S < \bar{S}$ are admitted, wherethe cutoff for lower-SEB students is lower than for higher-SEB students, $\underline{S_2} < \underline{S_1}$. This could be a result of affirmative action for low-SEB students, for example. We believe, however, that this is unlikely: Posselt (2016)'s detailed ethnographic study of elite PhD admissions found no evidence of affirmative action based on socioeconomic background, and, indeed, socioeconomic background is rarely observable to PhD admission committees.[52]

[52]Similarly, Lamont (2009)'s examination of academic grant-making found that very few panelists on grant

Second, it is possible that the admissions committee uses the same cutoff rules for lower-SEB and higher-SEB students, but the lower-SEB students happen to be the more “marginal admits” in any given PhD program: that the observed characteristics S for lower-SEB students may be systematically at the lower end of the interval $\{\underline{S}, \bar{S}\}$. This may be true of large PhD programs, but seems less likely to be true of small programs.

Third, it is possible that the observed characteristics S may on average be the same for higher and lower SEB students within a given PhD program, but that there are other characteristics which are unobservable to the PhD admissions committee, but reflect research ability, which are positively correlated with SEB even conditional on observables. That is, the PhD admissions committee sees the same *expected* research potential in their lower-SEB and higher-SEB admits, but in fact the higher-SEB has more research potential that is unobservable to the PhD admissions committee. This might be, for example, that when comparing a high-SEB individual and a low-SEB individual with the same grades, GRE scores, prior research experience, and recommendation letters, the high-SEB individual may have greater tacit knowledge of how to write well or greater experience with the creative part of the research process.[53] The reverse, however, seems equally plausible: to have obtained equivalently good *observable* measures of academic success S pre-PhD, it seems a priori more likely that lower-SEB individuals would have had to exhibit more determination, hard work, and entrepreneurial spirit than their higher-SEB colleagues, and one would also expect these characteristics to make someone a successful researcher.

Unfortunately, we have no data that enables us to test these possibilities. A test of the first and second, but not third, possibilities above would be to see whether lower-SEB admits are on average worse on observables, such as pre-PhD grades, GRE scores, research experience, or recommendation letters, than their higher-SEB counterparts from the same PhD program. This presents a useful opportunity for further study.

committees consider class diversity.

[53] Note that we exclude social and cultural capital from consideration here. We see these as factors which are correlated with SEB and enable researchers to produce better research in future, but do not reflect higher underlying research ability.

D.2 Differential selection by ability out of tenure-track academia

In Table 1 we find that there is no class gap in whether or not someone ends up a tenured professor (extensive margin), conditional on our baseline fixed effects, but that among those tenured there is a large class gap in whether they end up at an R1 or highly ranked institution (intensive margin). The fact that there is no extensive margin class gap suggests that differential selection out of academia probably does not explain our results. However *differential selection gradients on ability within parental education group* could reconcile the absence of an extensive margin class gap with the large intensive margin class gap. This would be possible if the highest-ability first-gen college grads are more likely to select out of academia, and vice versa, while the highest-ability people with a parent with a non-PhD graduate degree are more likely to select into academia, and vice versa – and, crucially, if this differential gradient in selection out of academia by ability nets out at no aggregate differences in the share ending up in tenured academia.

Three of our core empirical findings suggest this differential selection on ability is unlikely to explain our findings. First, we find a class gap not just at the point of the tenure-track job market, but also at the point of getting tenure, *conditional on tenure-track institution* fixed effects (Table 2 column 4). It seems much less likely that among professors who are already on the tenure track at the same institution (i.e. who selected into academia after their PhD), there is differential selection on ability to leave the tenure track.

Second, we find large class gaps even conditional on detailed measures of research output (Table 3). Thus, to explain the large class gap conditional on research output, we would need to believe there is differential selection by parental education group on *unobservable* research ability among people with a similar observable research record.

Third, we find a class gap in salary and career progression for PhDs in private industry, conditional on our baseline fixed effects (Table 6). If the higher-ability first-gen college grads were more likely to select out of academia and into (higher-paying) industry jobs, we should expect to see the opposite.

We also carry out two additional explorations to test this. To the extent that financial constraints motivate selection out of academia, we might expect to see that the degree of selection out of tenure-track academia for first-gen college grads is greater in fields where the salary gap between tenure-track academic jobs and industry jobs is greater. We estimate the log salary gap between tenure-track academia and industry, conditional our baseline fixed effects, separately for each of the 10 broad PhD fields. We also estimate our baseline extensive margin regression in Table 1 column 1 separately for each of the 10 broad PhD fields. We find no evidence that the class gap in selection out of tenure-track academia is greater for the fields where there is a larger industry-academia salary gap. Similarly, we might expect to see that differential financial constraints mediate the extensive margin class gap. Re-estimating our baseline extensive margin regression controlling for a third order polynomial in total student debt, we find no evidence for this: the coefficient stays almost identical to that in the baseline regression.

Finally, it is possible to bound the degree of differential selection on ability out of tenure-track academia which would be consistent with our findings. To do so, we carry out an exercise inspired Lee (2009), asking: What degree of differential selection on ability would there need to be to explain our baseline results? Specifically, we run our baseline regression from Table 1 column 3, but exclude parental education (regressing log tenure institution rank on our baseline fixed effects). We extract the residuals, and denote individuals with a negative residual "high ability" (a negative residual means that an individual's tenure institution rank is higher ranked than their demographics and PhD program would predict). We then randomly drop x% of high-ability individuals from the group with a parent with a non-PhD graduate degree (mimicking a scenario where these high-ability individuals were more likely to select into industry). We re-run our baseline regression to estimate the class gap in tenure institution type conditional on our baseline fixed effects. We run this 100 times for values of x between 5% and 25% and estimate the average class gap for each x across each set of 100 iterations, as shown in the table below. The class gap is closed only when

25% of the high-ability set of people with a parent with a non-PhD graduate degree are dropped. This means that to explain the class gap in tenure institution rank by differential selection on ability within PhD program across parental education group, you would need to believe that high-ability people with a parent with a non-PhD graduate degree are around 25% more likely to select into academia than their similarly high-ability PhD classmates who are first-gen college grads.

Table D1: Estimated class gap under different assumptions about selection on ability

x	0	5%	10%	15%	20%	25%
Class gap coefficient	0.092	0.081	0.061	0.040	0.017	-0.005

D.3 Bounding the role of unobservable research output

In the main text we argue that unobservable differences in research output are unlikely to explain the class gap in tenure institution rank or in "getting tenure". Specifically, denote our baseline research controls R_b, our additional research controls R_a and unobservable research measures U (which are assumed to be unobservable to us but observable to tenure committees). Controlling for our baseline research controls R_b increases the R-squared, showing that the measures of research we use are important in explaining tenure outcomes, and reduces the class gap, showing that lower-SEB individuals in academia have less impressive research records even conditional on our baseline fixed effects. However, adding our vast suite of additional research controls R_a barely shifts the estimated class gap and adds little to the estimated R-squared for either regression. This is because these additional research measures are sufficiently correlated with our baseline research measures, such that adding them does not further shift either the explanatory power of our regressions or our estimate of the class gap. In order for unobservable research measures U to explain a large share of the residual class gap when controlling for R_b and R_a, it would need to be the case that these measures are (1) highly uncorrelated with our existing suite of research measures R_b and R_a, (2) highly correlated with socioeconomic background even conditional on our research measures and baseline fixed effects, and (3) important for tenure outcomes. The fact that R_a adds so little explanatory power relative to R_b – despite the fact that it incorporates a wide range of additional important outcomes like NSF award receipt, first- and last-author publications, and the share of "hit" publications – suggests to us that unobservable research quality U is similarly unlikely to fulfil assumptions (1)-(3) above.

Even if unobservable research quality U is substantively uncorrelated with our existing regressors, however, we can attempt to bound the degree to which it might bias upward our coefficient on the class gap using the method developed by Oster (2019). Oster shows that under certain assumptions, the bias-adjusted coefficient β^* can be approximated as

$$\beta^* \approx \tilde{\beta} - \delta \left[\mathring{\beta} - \tilde{\beta}\right] \frac{R_{max} - \tilde{R}}{\tilde{R} - \mathring{R}} \tag{1}$$

where $\mathring{\beta}$ and $\mathring{R}$ are the coefficient and R-squared of the regression without the observed controls, $\tilde{\beta}$ and $\tilde{R}$ are the coefficient and R-squared of the regression *with* the observed controls, R_{max} is the maximum possible R-squared from a hypothetical regression where the relevant unobserved controls are included, and δ is the coefficient of proportional selection across observables and unobservables. Using our baseline regression (Table 3 column 1) as the regression without the observed controls, and our regression with full research controls (Table 3 column 3) as the regression with the observed controls, we can bound the possible bias arising from being unable to control for unobservable aspects of research quality which are (i) unobservable to us, (ii) observable to the tenure committee, and (iii) uncorrelated with existing regressors. To do so, we also need to select values for R_{max} and δ. Oster suggests setting $\delta = 1$, reflecting an assumption that the observables are at least as important as the unobservables (in our case, that the detailed observable measures of research output we have – publications, citations, journal impact factor, authorship position and contribution, NSF awards – are at least as important as unobservable research quality in affecting the tenure decision or tenure institution). We then follow Oster in estimating a bias-adjusted class gap, under four different values of R_{max}. For our first three scenarios, we set $R_{max} = \tilde{R} + k \cdot (\tilde{R} - \mathring{R})$, for three values of k: 0.25, 0.5, and 1. These assume, respectively, that the incremental explanatory power of unobservable research quality in tenure decisions is one quarter of, one half of, or equal to, the explanatory power of our observable research measures. In Scenario 4, we instead follow Oster's benchmark recommendation in setting $R_{max} = 1.25\tilde{R}$, which assumes that the addition of unobservable research quality measures would increase the explanatory power of our regression by 1.25 times (relative to our regression which *already* included all our detailed measures of research quantity and quality as well as PhD institution, field, and researcher demographics). Table D2 shows these assumptions and the bias-corrected coefficient estimates β^* under the three scenarios. We see scenarios 1 or 2 as the most plausible, since think it likely that all our combined observable research quality measures have more explanatory power for the tenure decision, collectively, than unobservable research quality. But even in the more conservative scenarios 3 and 4, the class gap remains substantial.

Table D2: Bias-corrected class gaps under different assumptions

	Log tenure rank	Got tenure
Parameters from regression output		
$\mathring{\beta}$	0.149	-0.0635
$\mathring{R}$	0.24	0.61
$\tilde{\beta}$	0.101	-0.0596
$\tilde{R}$	0.37	0.67
Scenario 1: $R_{max} = \tilde{R} + 0.25(\tilde{R} - \mathring{R})$		
R_{max}	0.403	0.685
β^*	0.0.089	-0.0586
Scenario 2: $R_{max} = \tilde{R} + 0.5(\tilde{R} - \mathring{R})$		
R_{max}	0.435	0.7
β^*	0.077	-0.0577
Scenario 3: $R_{max} = \tilde{R} + (\tilde{R} - \mathring{R})$		
R_{max}	0.5	0.73
β^*	0.053	-0.0557
Scenario 4: $R_{max} = 1.25\tilde{R}$		
R_{max}	0.4625	0.8375
β^*	0.067	-0.0489