Mandatory Retirement and Age, Race, and Gender Diversity of University Faculties*

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Abstract

While many have documented the changing demographics of universities, understanding the effects of prohibiting mandatory retirement ("uncapping") has proved challenging. We digitize detailed directories of all American law school faculty from 1971-2017 and show that uncapping in 1994 had dramatic effects. From 1971 to 1993, the percent of faculty above 70 – when mandatory retirement would typically have been triggered – remained stable at 1%, but starting in 1994, that proportion increased to 14%. We use a permutation test of moving cohorts to show that these increases are attributable to uncapping. Roughly 39% of faculty members would counterfactually have been subject to mandatory retirement. Effects were less pronounced at public schools, which were more likely to have defined benefits retirement plans. Second, we show that schools with the highest proportion of faculty over 70, and thus most impacted by uncapping, also exhibit the slowest integration of female and minority faculty members. Our study highlights cross-cutting effects of civil rights laws: preventing age discrimination can have collateral effects on racial and gender integration.

*We thank Claire Greenberg for helpful research assistance, Brandon Anderson, Abhay Aneja, Dick Buxbaum, Lawrence Friedman, Sandy Handan-Nader, Dan Hulsebosch, Anne Joseph O'Connell, Pam Karlan, Jenny Martinez, Goodwin Liu, Jess Saunders, Laura Trice, and participants at the faculty workshop at Berkeley Law for helpful comments and conversations, Alain Kelder, Jason Watson, and Beth Williams for help in digitizing the AALS volumes, and Amy Applebaum for sharing demographic reports on the entry-level hiring pool.

1 Introduction

In 1986, Congress amended the Age Discrimination in Employment Act (ADEA) to prohibit mandatory retirement in most forms of employment. Due to the unique characteristics of the tenure system, Congress granted a seven-year exemption for faculty at institutions of higher education and mandated further research on potential effects of "uncapping" on colleges and universities. Some voiced concerns about fiscal pressures, innovation, and productivity (Casper and Mac Lane, 1990; Pratt, 1989). For example, then-University of Chicago Provost Gerhard Casper argued uncapping was a "grave mistake" that would cause "the heavy hand of old ideas [to] restrict new contributions in the classroom and laboratory" (Casper and Mac Lane, 1990). A much smaller minority worried about effects on diversification. A working group of the American Association of University Professors noted that the retention of predominantly white male faculty could "preclude[] replacement by women and minorities" (Brown et al., 1987).

However, the leading studies on the topic, which examined a small number of early uncapping states, concluded that uncapping would have negligible effects. The National Academy of Sciences' report concluded uncapping "is unlikely to affect the vast majority of colleges and universities because most faculty members now retire well before age 70" (National Research Council, 1991). Another influential study concluded, "most of higher education will not be seriously affected" (Rees and Smith, 1991). A committee assembled by the American Association of Law Schools (AALS) similarly reported, "it does not foresee a dramatic alteration in the overall retirement pattern of law faculty following 1993" (The Association of American Law Schools, 1990). Based on these reports, Congress allowed the university exemption to lapse in 1994, thereby uncapping American universities.

The accuracy of these early predictions remains contested. Over the past decades, the age composition of university faculty has shifted substantially, leading to what has been called the "graying" of faculty and academic research (Kaiser, 2008; Jane, 2012). These trends have been documented in a variety of fields, including in engineering, medicine, the humanities, and the sciences (Blau and Weinberg, 2017; Conn, 2010; Ghaffarzadegan and Xu, 2018; Hershel and Liu,

2009). Researchers have found significant increases in the average age of faculty, declines in rates of faculty retirement, and shifts in the distribution of research dollars to older faculty. The National Institutes of Health, for example, predicted that by 2020 grantees over the age of 68 would outnumber those under 38 (Kaiser, 2008).

A particular challenge in existing research lies in isolating the effects of the *policy* intervention uncapping. Moreover, no study has been able to assess whether uncapping affected the pace of racial and gender diversification amongst faculty, in spite of strong reasons to expect such cross-cutting effects. In many academic fields, uncapping went into effect at a time when the composition of senior faculty was predominantly white and male, whereas women and minorities were increasingly comprising a larger share of the hiring pool (National Science Foundation, 2019). To present day, the entry-level hiring pool continues to be more demographically diverse than incumbent faculty (National Science Foundation, 2019; Li and Koedel, 2017). Delayed retirements due to uncapping may have slowed hiring and hence diversification.

We address the gaps in the literature using a setting that offers a unique opportunity to study the effects of uncapping. For over fifty years, the American Association of Law Schools (AALS) has published directories containing rich biographical and demographic details of all U.S. law faculty. We digitize these directories from 1971 to the present and assemble nearly five decades of data on faculty composition, including gender, age, and racial minority status of 14,908 faculty members in 166 schools. This data—rare in its scope across institutions, its comprehensiveness within institutions, and its inclusion of individual demographic detail—permits us to study the effect of uncapping on both the age composition and diversification of faculties.

First, we leverage the arbitrariness of the typical mandatory retirement age of 70 to isolate the effect of uncapping as distinct from secular demographic changes. We show that uncapping had dramatic long-term effects on the age composition of faculties. The proportion of faculty members above 70 was stable at around 1% in all years prior to uncapping, but increased by ten-fold after uncapping. Among faculty who would have been subject to mandatory retirement between 1994-2017 (i.e., who would have turned 70 in the period), 39% elected to work past age

70. Using a nonparametric permutation test of cohorts reaching retirement eligibility immediately before and after uncapping, we show that these patterns are attributable to uncapping. We also provide evidence of the role of retirement incentives, as public schools, which disproportionately retained (defined benefit) plans that muted incentives to delay retirement, appeared less affected by uncapping. Second, we show that the sharp rise in retirement-eligible faculty is associated with reduced racial and gender diversification. We use covariance-adjusted permutation inference to rule out no effects of retirement eligible faculty on female and minority faculty members, and offer evidence that the most likely mechanism is in reducing the volume of entry-level hiring.

Our paper proceeds as follows. Section 2 provides a brief review of the existing and related literature. Section 3 discusses our data sources. Section 4 presents results of the effects on the age of faculties, the mediating effect of retirement incentives, and the effects on racial and gender diversity. Section 5 discusses limitations and Section 6 concludes.

2 Extant Literature

Since the early reports, a small number of studies has attempted to address the effects of uncapping on universities. One leading study of a national sample of institutions from 1987-96 found that fewer faculty retired upon reaching age 70 and 71 after institutions uncapped (Ashenfelter and Card, 2002). The data, however, included only three years of observations after federal uncapping. An analysis over a longer time window may be important because (a) uncapping was prospective, not retroactive, and the effect would hence necessarily be gradual, accumulating as more faculty reached the age of 70; (b) rapid hiring of junior faculty in the 1960s in response to enrollment increases from baby boomers created a “bulge” of faculty who did not face the retirement age of 70 until the late 1990s and early 2000s (Ashenfelter and Card, 2002); and (c) potential long-term effects of uncapping may have been mitigated by institutional adaptations, such as the adoption of retirement incentive programs (Clark and Ghent, 2008).

Other research has analyzed the effects of uncapping using data from specific institutions (Larson and Gomez Diaz, 2012; Clark and Ghent, 2008; Weinberg and Scott, 2013; Clark et al., 2001;

Ehrenberg et al., 2001) or from longitudinal surveys of scientific doctoral degree recipients (Blau and Weinberg, 2017; Ghaffarzadegan and Xu, 2018), finding some evidence of delayed retirements and increases in the average age of faculty. While these studies are valuable, many of their designs make it harder to disentangle secular trends (e.g., increased life expectancy, changing attitudes about work) from the effects of uncapping, and it is less certain whether single-institution studies generalize to the population of universities. The only study of professional schools finds that at one university, in contrast to its non-professional schools, retirement behavior was unaltered. Researchers attributed this difference to lucrative opportunities available in medicine, law, and business following retirement (Weinberg and Scott, 2013).

As far as we are aware, no prior study has examined the collateral effects of uncapping on the pace of racial and gender diversification, in spite of the acknowledged importance of faculty diversity for innovation, research, and students (Brest and Oshige, 1995; Nielsen et al., 2017, 2018; Bertrand, 2011; Bayer and Rouse, 2016). The closest study, which focused on one institution, inferred that uncapping did not negatively affect diversification, as the proportion of female and minority faculty increased over time (Weinberg and Scott, 2013). Yet if uncapping delays retirement, it may slow a *positive rate* of diversification, particularly because entry-level pools have become more diverse over time (National Science Foundation, 2019; Kay and Gorman, 2008).[1]

Last, existing work on the effects of civil rights laws has focused on the direct effects on protected groups (Oyer and Schaefer, 2003; Donohue and Siegelman, 1990) or groups at the intersection of protected categories (Crenshaw, 1989; Best et al., 2011). In the age discrimination context, researchers have documented the effects of the ADEA on employment of older workers (Lahey, 2008; Neumark and Button, 2014) and the challenges older minority and female workers in securing protections (Delaney and Lahey, 1989). Our work contributes to this literature by highlighting the cross-cutting tension across civil rights laws: protection along one dimension (age) may undercut advancement along another (gender and race).

[1] See also Appendix Figure A3.

3 Data

We digitize and parse over 42,000 pages from 43 volumes of the annual Directories of Law Teachers published by the AALS between 1971-2017. These directories contain biographical information (e.g., degrees, employment history), titles, school affiliations, and demographic attributes for most of the observation period, including birth year, gender, and minority status. Minority faculty members are those self-identifying as Asian American, African American, Mexican American, Native American or Alaskan Native, Hispanic American, or Pacific Islander. We provide additional details in Appendix A, but the overall process worked as follows.

First, we use an optical character recognition engine to extract the text stream in each volume. Where the volume was not available in PDF format, we scanned the volumes. When PDFs were available, we used our own optical character recognition engine (Prime Recognition), as this generated higher accuracy than using the existing text stream.

Second, we parse school affiliation listings, individual biographical sketches, and minority faculty listings, which come from separate sections in each directory. We classify law teachers into tenured/tenure-track faculty, emeritus faculty, clinical faculty/instructors, and librarians based on titles. For this task, we create a dictionary of all variations of titles. We then consult individual CVs, school directories, and faculty handbooks to map these titles onto classifications and account for variation across schools. Because uncapping affected only tenure and tenure-track faculty, we exclude academic librarians, clinical faculty, and adjunct faculty.

Third, we develop semi-automated record linkage methods to structure the data as a relational database of faculty and schools over time. To ensure that our database links faculty with significant name changes across years, we compare all possible pairs of unique faculty members in our database using a liberal fuzzy match and manually verify all matches. Fourth, we augment school information (e.g., ranking of school, public vs. private school).

Last, we engage in considerable manual and semi-automated validation, completion, and correction of data fields. For instance, we look up CVs and biographies of all faculty (a) missing birth years after 2007 (when AALS ceases to report birth year) and (b) changing employment status in

years where the volume was not published (2008, 2012, and 2013). Where birth year is missing, we impute age based on degree dates. The imputation model has an R^2 of 0.98 for when degree and birth year are both observed. Where gender is missing, we use a model based on Social Security Administration baby names and manually look up all faculty with gender-ambiguous names (e.g., "Taylor"). Our estimated accuracy with gender prediction is 99%. We also assess sensitivity to minority self-reporting by using ethnicity predictions from a neural network trained on census names.[2]

Our data has several virtues. In contrast to longitudinal surveys, it contains the entire faculty composition of each school. The directories span over two decades before and after uncapping, allowing us to observe long-term changes in faculties. The fact that specific birth years were reported for most volumes enables us to measure age reliably. Information on gender and minority status permit us to study the effects of uncapping on diversification.

The final dataset consists of 14,908 unique tenure or tenure-track faculty members, including 3,544 white women, 757 minority women, and 901 minority men. It covers 166 law schools, 43% of which are public schools, with 269,881 school-faculty-year tuples. Because of the unique dynamics, our main analysis excludes historically black universities, schools outside the continental United States, and the Judge Advocate General's School. As we identify effects based on changes, we also exclude schools after a merger or split and law schools that existed exclusively before or after uncapping. Our results are the same including these schools.[3]

Figure 1 displays the faculty age distribution for each year before uncapping (left panel) and after uncapping (right panel), demonstrating a substantial demographic shift over time. This growth reflects broader demographic changes over time, and we focus specifically on effects around the mandatory retirement age of 70 (gray vertical line) in our analyses.

[2]See Appendix B.4.
[3]See Appendix F.6.

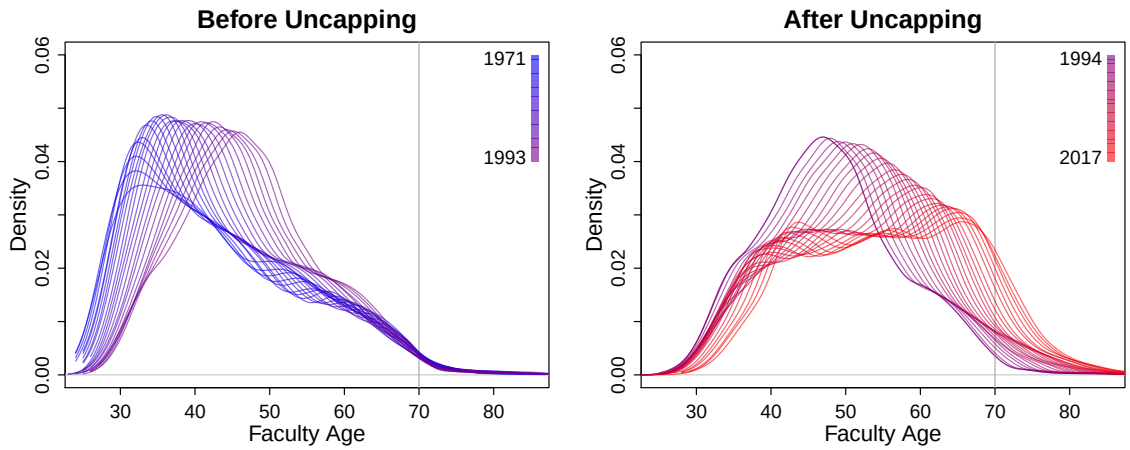

Figure 1: Kernel density plot of faculty age across all schools for each year.

4 Results

4.1 The Effect of Uncapping on Retirement

We first examine the effects of uncapping on faculty retirement behavior. Figure 2 displays the percentage of faculty above 70 from 1971 to 2017. Each dot represents one school, weighted by faculty size, with year on the x-axis and the proportion of faculty above 70 on the y-axis. While retirement eligibility depends on individual circumstances and institutions, we use the phrases "above 70" and "retirement-eligible" interchangeably to refer to faculty aged 70 or above, who would have been subject to mandatory retirement without uncapping. Whereas this percentage was stable and approximately 1% in all years before uncapping, the proportion of the faculty above 70 has grown sharply after uncapping, increasing from 1.4% in 1993 to 14.0% in 2017. Harvard Law School and New York University School of Law, for example, had 2 and 1 faculty members over 70 in 1993, but by 2017, one-fifth and nearly one-third of their faculties would have been subject to mandatory retirement, respectively.[4]. To place this increase in context, Figure 3 compares population demographics over time. The proportion of the U.S. population above 70 is much smoother and does not exhibit any break point around 1994.

To isolate the immediate effects of uncapping, we construct neighboring cohorts $c \in \{1, 2\}$ of

[4]The outlier school in the beginning of the observation period, with nearly 40% of its faculty above 70 in the early 1970s, is U.C. Hastings, which affirmatively hired attorneys at the tail-end of their careers to boost its reputation, leading its faculty to refer to Hastings as the "65 Club" (Barnes, 1978)

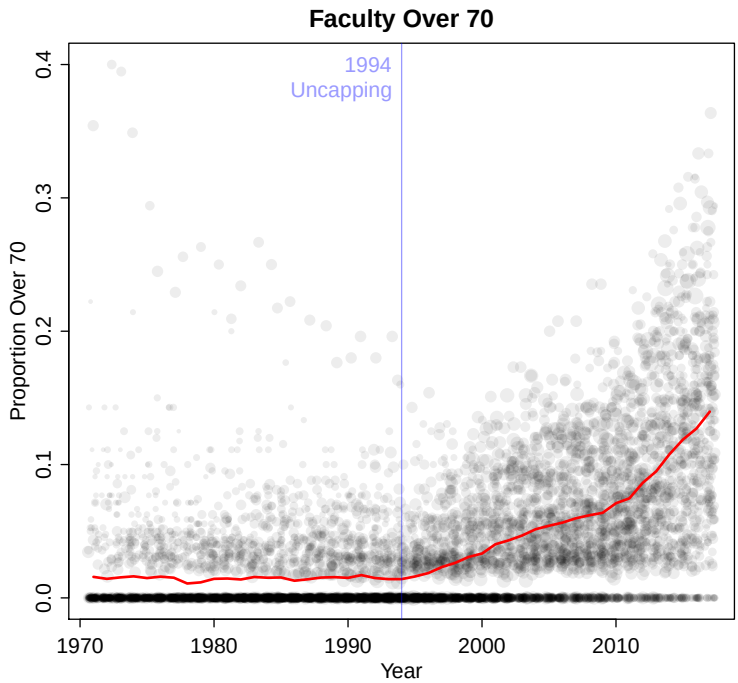

Figure 2: Proportion of faculty over the age of 70. Each dot represents a school and the size of the dots represents the overall faculty size. The red line represents the the average proportion across all schools for a given year.

faculty who were either just subject to or not subject to mandatory retirement solely due to birth year. We compare faculty turning 70 during the three years before uncapping (1991-1993) with faculty turning 70 during the three years after uncapping (1995-1997). The left panel of Figure 4 presents Kaplan-Meier survival curves in these cohorts. While curves are comparable prior to age 69, they sharply diverge after the retirement age of 70. We test for the difference in survival curves using a logrank test. Under the null hypothesis of no distributional difference between cohorts, the χ^2 test statistic should follow a χ^2_1 distribution:

$$\chi^2 = \sum_c \frac{(\sum_t O_{ct} - \sum_t E_{ct})^2}{\sum_t E_{ct}} \tag{1}$$

where $\sum_t O_{ct}$ is the sum of observed departures in cohort c over age t and $\sum_t E_{ct}$ is the sum of expected departures in cohort c over age t. Membership in the uncapped cohort is associated with a median increase in faculty tenure of 7.5 years (p-value = 0.0015). We note that because the sharpest shift occurs right at the mandatory retirement age, the difference is unlikely explained by secular demographic shifts.

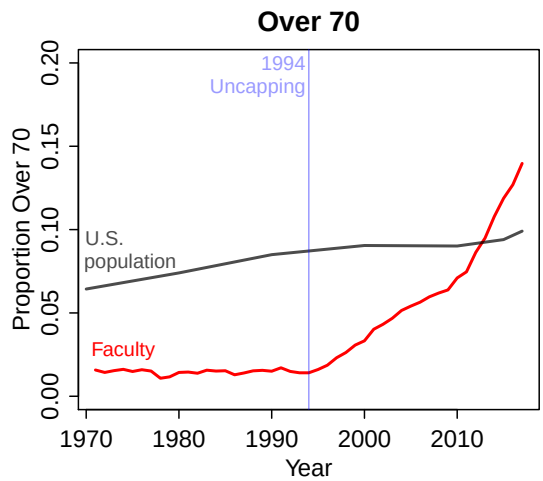

Figure 3: Proportion of faculty over the age of 70. The red line represents the the average proportion of faculty over 70 across all schools for a given year. The gray line represents the proportion of the U.S. population over 70 for a given year. Source for U.S. population data is the U.S. Census.

We further conduct a nonparametric permutation-based test to rule out the possibility that the increase is due to trends of aging between the cohorts. We treat each year between 1988-2013 as a placebo year of uncapping, denoted by the set $\omega \in \{1988, \ldots, 2013\}$ of size 26. Let χ^2_ω represent the test statistic from Equation 1 given placebo year ω of uncapping, capturing the difference in the survival curves of faculty cohorts turning 70 within 3 years before and after each placebo year. We calculate this test statistic across all placebo years and calculate the one-tailed p-value by comparing the observed test statistic χ^2_{1994} against the placebo distribution:

$$\Pr(\chi^2_{1994} \leq \chi^2_\omega) = \frac{\sum_\omega \mathbb{1}(\chi^2_{1994} \leq \chi^2_\omega)}{26} \tag{2}$$

If the shift in the left panel of Figure 4 is due to aging, the observed χ^2_{1994} test statistic should be drawn from the placebo distribution. The right panel of Figure 4 presents the distribution of test statistics. In contrast to what would be expected under the null, the observed distributional shift around 1994 is an extreme outlier. We can hence reject the null hypothesis of no effect attributable to uncapping in 1994 (p-value = 0.04, the lowest possible p-value with 26 test statistics).

Our results show that uncapping appeared to have substantial effects on the age composition of law schools. Prior to uncapping, very few faculty continued to serve past the age of 70, due to mechanistic enforcement of mandatory retirement policies. Among faculty who would have

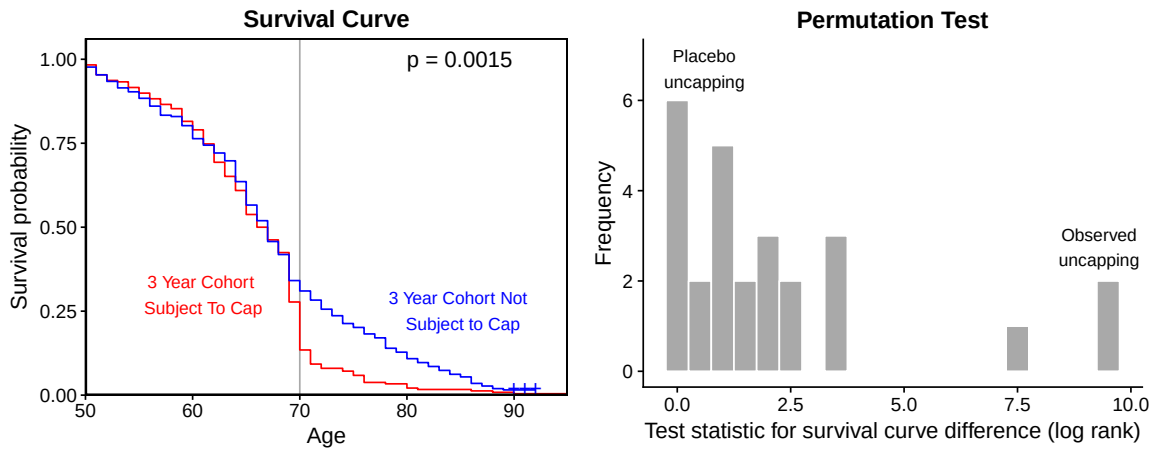

Figure 4: Survival analysis. Left: Kaplan-Meier survival curves for a cohort subject to the cap in red (i.e., tenured faculty active at the age of 50 who turned 70 in the 3 years before uncapping) and a cohort not subject to the cap in blue (i.e., tenured faculty active at the age of 50 who turned 70 in the 3 years after uncapping). We reject the null hypothesis that the distributions are the same using a logrank test, with p-value reported on the top right. Right: Distribution of logrank test statistics for differences in survival curves for cohorts of faculty turning 70 three years before and after the observed uncapping year and placebo uncapping years (all other years between 1988-2013).

turned 70 between 1994-2017, 39% elected to work past age 70. While we focused on neighboring cohorts to isolate the short-run effects of uncapping, Figure 2 also suggests that the long-run cumulative effects are substantial. Roughly 14% of faculty positions are occupied by retirement-eligible faculty in 2017.

4.2 The Impact of Retirement Incentives

We now examine whether differences in retirement incentives may have mitigated the effects of uncapping on retirement behavior. This mechanism is important for two reasons. First, it provides another avenue to distinguish whether the growth in retirement-eligible faculty post-1994 is due to secular trends or uncapping. If such growth were purely driven by secular trends, we would not expect retirement incentives to interact with uncapping. Second, if retirement incentives do in fact mitigate the effect of uncapping, these findings would highlight an important policy lever for states and universities in addressing the changing demographics of faculty. To examine the impact of retirement incentives, we explored a wide range of data sources, but comprehensive historical information at the individual school level about retirement programs are exceedingly difficult to

recover. We hence leverage the fact that there are well-known differences in retirement incentives across public and private schools.

Most faculty nearing retirement at public institutions prior to and in the two decades following uncapping had defined benefit (DB) retirement plans, whereas most retirement-age faculty at private institutions had defined contribution (DC) plans (King and Cook, 1980; Holden and Hansen, 2001; Ehrenberg and Rizzo, 2001). In DB plans, the employer guarantees to pay employees an annual pension throughout retirement, which is determined by a formula that multiplies employees' years of service, average salary, and other factors. In contrast, in a DC plan, employers and employees make annual contributions (typically as a percentage of employee salary) into an investment fund. Employers do not guarantee a specified benefit at the time of retirement; rather, the benefit reflects the total contributions and dividends as affected by market fluctuations. Although an increasing number of public institutions began in the 1990s and early 2000s to offer a DC plan exclusively, a choice between a DB and a DC plan, or a hybrid plan, these changes primarily applied to new hires (Lahey et al., 2008). Thus, for most of our observation window, we expect that faculty at public institutions who were retirement-eligible were covered under DB plans.

DB plans tend to have weaker incentives to delay retirement compared to DC plans (Rees and Smith, 1991; Clark and Ghent, 2008; Ehrenberg and Rizzo, 2001; Issacharoff and Harris, 1997). As Issacharoff and Harris put it, "Defined-contribution plans ... clearly create incentives toward late retirement" (Issacharoff and Harris, 1997). This is so for at least three reasons. First, because often "defined benefits plans have large, age-specific retirement incentives at the early and normal retirement ages," pension wealth in DB plans "rises more slowly and can actually decline, once the worker becomes eligible to start receiving benefits" (Clark and Ghent, 2008). In contrast, DC plans have been described as "more age neutral in their retirement effects and the present value of the pension continues to rise with continued employment" (Clark and Ghent, 2008). Effective age-specific retirement incentives are more likely to be integrated into DB plans because of the plan's structure. As Ehrenberg explains, "It is easy to build retirement incentives into DB plans by offering individuals credit for additional years of service if they retire before a specified age.

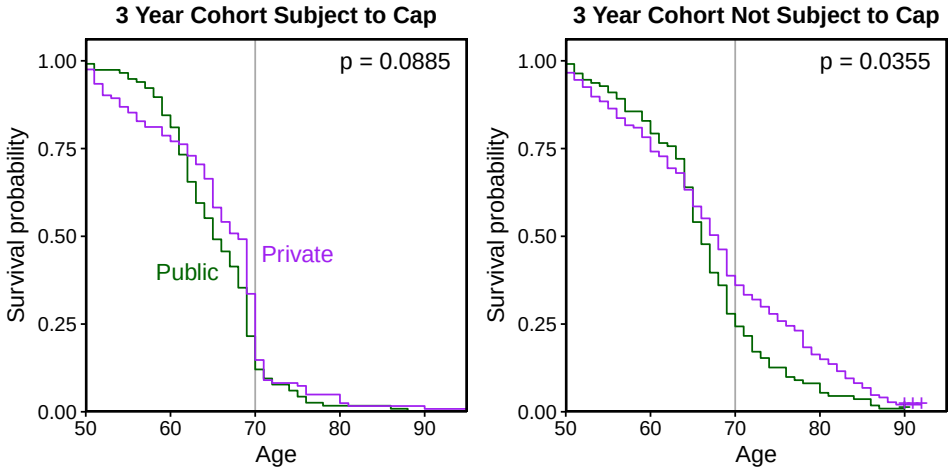

Figure 5: Survival Curves By School Type. Kaplan-Meier survival curves for three year cohorts subject to cap (left) and not subject to cap (right) by public school (green) and private school (purple). While most faculty retire by 70 pre-1994, the survival curve shifts more substantially to the right for private schools post-1994.

It is much more difficult and expensive, however, to build effective retirement incentives into DC programs, because additional contributions made by employers to encourage retirement are subject to federal and state income taxes in the year the contributions are made" (Ehrenberg et al., 2001).

Second, DB plans may provide greater certainty about benefits. Under DB plans, employers guarantee to pay employees a predetermined annuity for life. Under DC plans, employees assume the risk that they will outlive the funds in their accounts and face uncertainties about whether market downturns or poor investment decisions will significantly erode their funds (Michel et al., 2010). Such market uncertainty may be why we observe such a substantial increase in retirement-eligible faculty after the Great Recession.

Third, DB plans at public institutions may also spur earlier retirements because they provided greater pension wealth than DC plans at private universities.[5] Many studies have reported that public-sector DB pensions tend to offer annuities that are more valuable, on average, than private-sector DC plans (Craig, 2014; Kiewiet and McCubbins, 2014).[6]

We hence expect that fewer faculty would continue to work past age 70 at public law schools.

[5]Prior studies have found that pension wealth is positively correlated with retirement probabilities (Ashenfelter and Card, 2002; Clark et al., 2001).

[6]That said, it is difficult to confirm whether such a disparity existed between plans at public and private law schools. We are unaware of a study that has compared pension wealth or retirement benefits at public versus private law schools.

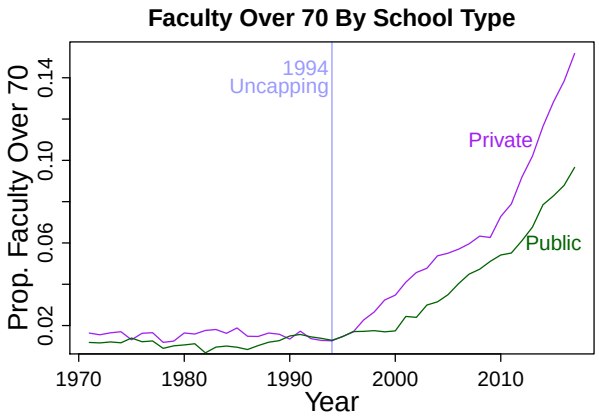

Figure 6: Faculty Over 70 By School Type. Average proportion of faculty over the age of 70 at private (purple) and public (green) law schools. The vertical line indicates the year mandatory retirement was uncapped (1994).

Figure 5 presents Kaplan-Meier survival curves comparing three year cohorts subject to and not subject to the cap at public (green) and private (purple) institutions. This figure shows that faculty at public law schools are significantly less likely to continue working past age 70 than faculty at private law schools after uncapping. Figure 6 presents more detailed results on the temporal dynamics associated with uncapping. Prior to 1994, public and private schools differ very little in the proportion of retirement-eligible faculty. After 1994, there is a sharp divergence between public and private schools, with the retirement-eligible faculty significantly higher at private schools than at public schools. In 2017, roughly 10.7% of public school faculty were above 70 compared to 16.1% of private school faculty. These findings suggest that retirement incentives play an important role in mediating the effect of uncapping.

While retirement plan type is the most widely studied distinction between retirement incentives at public and private institutions, we acknowledge that other differences between may exist between public and private schools. That said, Figures 5 and 6 show that the difference emerges around the time of uncapping. For a difference between public and private institutions to explain this divergence would require a source confounding *contemporaneous* to 1994. The only plausible time-varying intervention that differentially affected public and private schools would have been the Supreme Court's decision in *Kimel v. Florida Board of Regents*, 528 U.S. 62 (2000). In *Kimel*,

the Court held that public institutions were immune from suits alleging violations of the federal ADEA. As a result, public universities may have faced weaker repercussions for continuing to enforce mandatory retirement than private universities. Yet there are reasons to doubt that *Kimel* explains these findings. First, the divergence between public and private schools appears immediately after uncapping, as seen in Figure 5, nearly six years before *Kimel*. Second, the effects of *Kimel* were limited, as many public universities remained subject to state age discrimination statutes and the federal government could still bring discrimination suits against public universities (Bodensteiner and Levinson, 2001). In any case, while *Kimel* could weaken the explanation of the role of retirement benefits, it would strengthen the case of the role of mandatory retirement.

In short, our findings suggest that retirement benefits play a significant mediating role in the effects of uncapping on the age distribution of faculties.

4.3 Effects on Racial and Gender Diversity

We now investigate the collateral effects of uncapping on gender and racial diversity. The main mechanism we focus on is (a) whether uncapping reduced the volume of entry-level hiring due to billet and resource constraints, and (b) whether uncapping hence reduced the number female and minority candidates hired, given that much more diversity exists in the entry-level pool.

To understand this mechanism, it is valuable to observe the long-term context surrounding uncapping. Figure 7 provides cross-sectional snapshots of the demographics of law schools at the beginning of our observation period in 1971 (top), the year before uncapping in 1993 (middle), and the most recent observed year in 2017 (bottom). The left column of panels displays the age distribution by race, with majority faculty in blue and minority faculty in red. The right column of panels displays the age distribution by gender, with male faculty in green and female faculty in yellow. The top panels show that there were very few women and minorities serving as faculty at the beginning of our observation period. Only 1.7% of law professors were minority faculty and only 3.1% of law professors were women.

The middle panel of Figure 7 shows that at the time of uncapping, faculty turning 70 within five

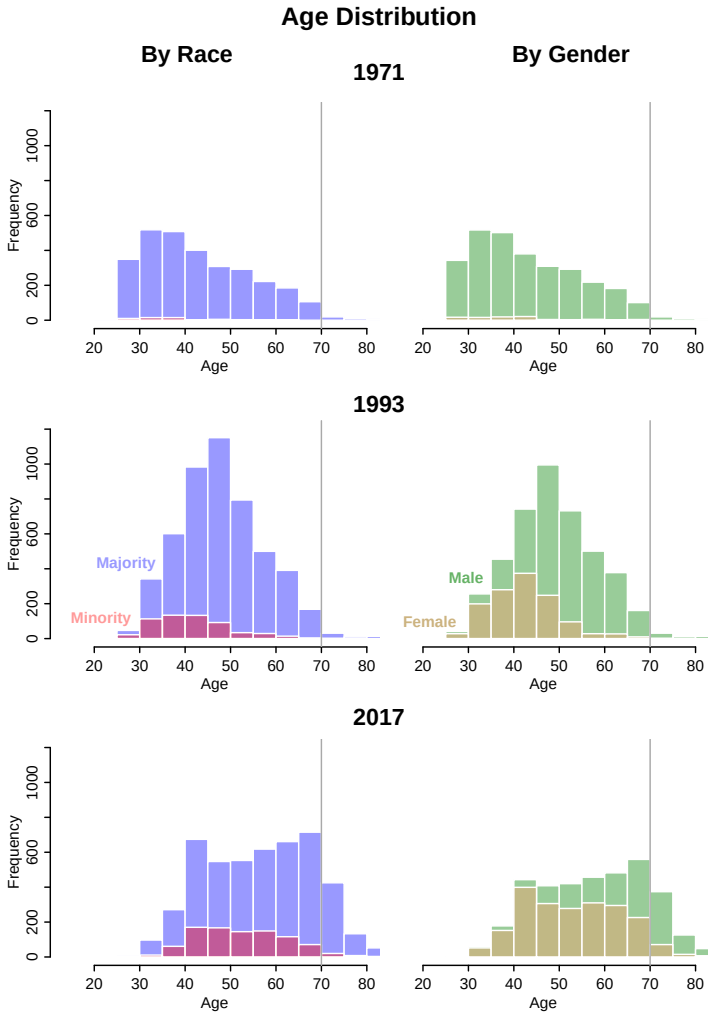

Figure 7: Age distribution of faculty by race and gender in 1971, 1993 and 2017. The left panel shows overlayed histograms of minority and majority faculty members in each age range, while the right panel shows overlayed histograms of female and male faculty members in each age range.

years were disproportionately white (98%) and male (92%). As can be seen by the age distribution differences between (a) majority and minority faculty in the middle left panel and (b) male and female faculty in the middle right panel, the primary source for gender and racial diversity for much of the observation period was in entry-level hiring. For instance, in 1993, roughly 47% of women were tenured, compared to 80% of men. This difference stems from formal and informal barriers into the profession in the 20th century (Abel, 1989). Only 4% of lawyers were female in 1970 and because faculty positions typically require a J.D., and often some degree of practice experience, diversification of law faculties lags behind diversification of the profession (Epstein, 1993, p. 5).

The bottom panel confirms that the faculty that have benefited from uncapping by working past age 70 have been disproportionately white males. This trend is not merely the case in the cross-section, but in the decades following uncapping, white men comprise 85%of retirement eligible law faculty. Even in the most recent observed year, over 80% of retirement eligible faculty were men.

Figure 8 presents data for all schools from 1971-2017 of the average proportion of each faculty that is female (left) and minority (right). Each dot represents a school, weighted by faculty size. Recall that from Figure 2, the proportion of faculty above 70 remained constant and close to zero from 1971 to 1993. The time trend plotted in red in Figure 8 shows that the proportion of female and minority faculty increased steadily prior to uncapping. After uncapping, the rate of racial diversification appears to have slowed substantially. The decrease in diversification does not appear to be a result of diminished diversity in the entry-level pool. Using hand-collected information from the "register of candidates" for the central faculty hiring conference, we find that the proportion of applicants who are female and who are minorities has been increasing over time from 1990 to the present.[7] While the slowing rate of diversification in hires, given an increasingly diverse entry-level pool, is interesting and important in its own right, the question remains to what extent the slowing rate of diversification is attributable to uncapping.

[7]See Appendix Figure A3

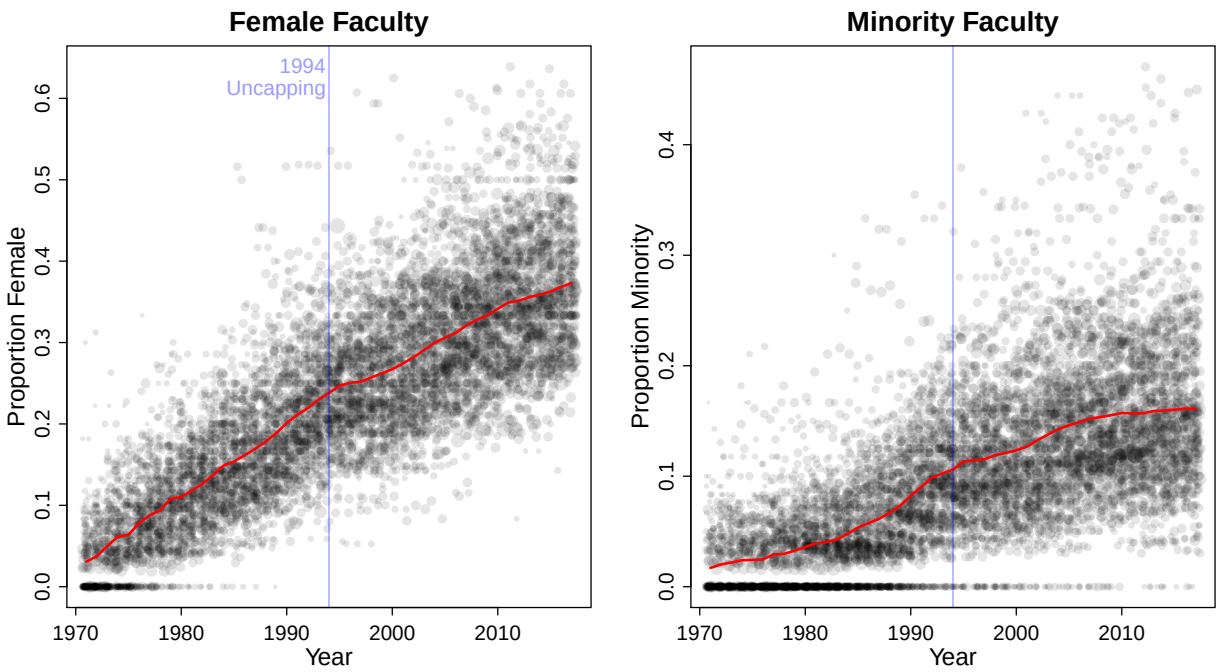

Figure 8: Female and minority faculty. Proportion of faculty that is female (left panel) and minority (right panel) over time. Each dot represents a school, weighted by faculty size, and red lines plot the average proportions across all schools for each year.

To examine this more systematically, we leverage variation in the proportion of the faculty above 70 across schools. The intuition behind this approach is that (a) faculty hiring is constrained by budgets and billets; and (b) the extent that uncapping constrains entry-level hiring depends on the number of positions occupied by retirement eligible faculty. To provide graphical intuition, we divide schools into the most and least affected by retirement-eligible faculty, based on whether the proportion above 70 is above or below the median across the observation period. If uncapping affects hiring via the posited mechanism, we should observe these schools diverge after uncapping in hiring of female and minority faculty. Panels A, B, and C of Figure 9 confirm this dynamic. While gender and minority integration was indistinguishable between the most and least affected schools prior to 1994, the schools most affected by uncapping were substantially slower to diversify after 1994. What is particularly compelling about these visualizations is that the divergence occurs exactly around 1994, while pre-trends are nearly identical.

The bottom row of Figure 9 splits schools by rank to examine whether these trends differed

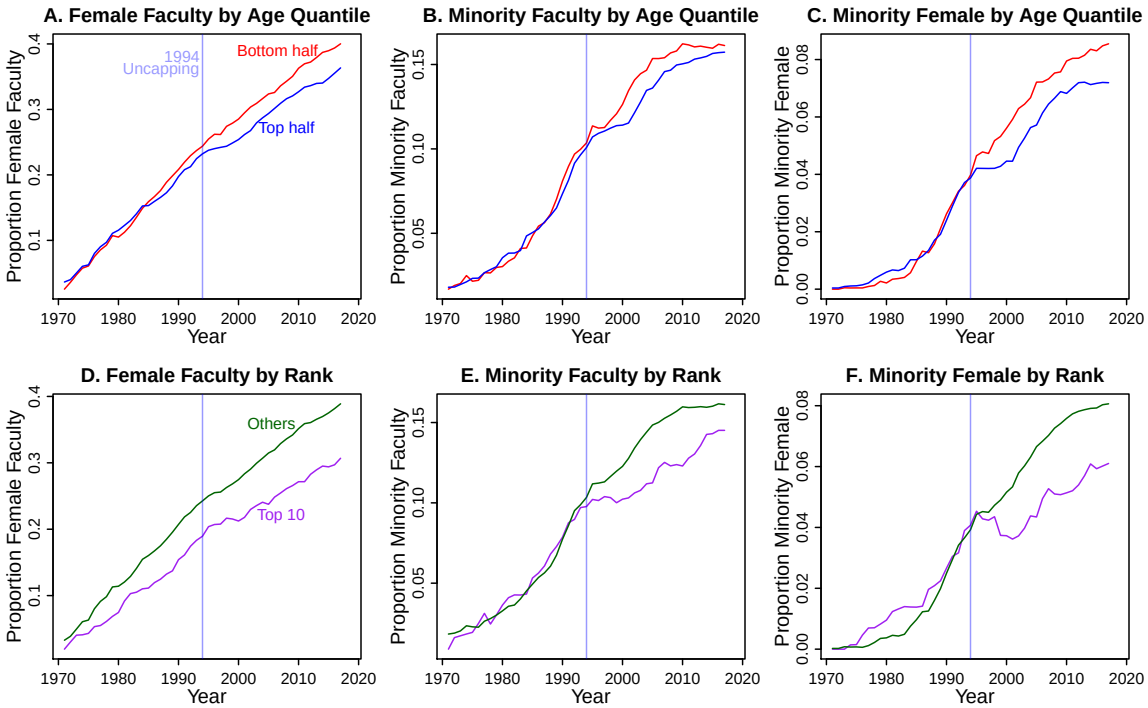

Figure 9: Diversification by age quantile and rank. Proportions of minority, female, and minority female by age quantile (top row) and by rank (bottom row). In the top row, the blue (red) line represents schools most (least) affected by uncapping. In the bottom row, the purple (green) line represents top 10 (all other) law schools.

by rank of school. The panels show that top 10 schools (purple) appeared to be more affected by uncapping, particularly for minority hiring.[8] These differences across ranks are consistent with early research on uncapping in the college setting, which found that a higher school rank (as proxied by the average student SAT score) was the strongest predictor of delayed faculty retirement (Rees and Smith, 1991).

We now formalize a test of the impact of uncapping on diversification. We test for the effects of the proportion of faculty above 70 in the preceding academic year on the number of entry-level hires and the number of female, minority, and minority female faculty. To rule out mechanistic effects, we measure retirement eligible faculty in the *preceding* year, when an entry-level hire would typically be made, with a faculty member joining the subsequent year. Our regressions control for school fixed effects to account for (time-invariant) school differences (e.g., public school, re-

[8]Due to fluctuations in ranks, we plot the 14 schools that have been ranked in the top 10 by conventional rankings during the observation period, but results are the same using only the most recent top 10 ranked schools.

gion, general size) and year fixed effects to account for (school-invariant) yearly differences (e.g., diversity of the entry-level pool). Our effects are hence identified by changes in the retirement eligible faculty within the same school over time. Such institution- and time-specific variation in the retirement-eligible faculty – driven by faculty demographics and individual decisions to retire – provide plausibly exogenous variation in how much uncapping affected an institution by constraining billets. We separately model the counts of junior, female, minority, and minority female faculty, denoted as y_{st}, in school s in year t using a quasi-Poisson model:

$$y_{st} \sim \text{Poisson}(\mu_{st}) \tag{3}$$

$$\mu_{st} = n_{st} \exp(\alpha_s + \beta_t + \theta T_{s(t-1)}) \tag{4}$$

$$\text{Var}(y_{st}|\mathbf{X}) = \phi \mu_{st} \tag{5}$$

where n_{st} is an offset for the log of the total number of faculty observed at school s in year t, α_s are school fixed effects, β_t are year fixed effects, $T_{s(t-1)}$ is the proportion of faculty above 70 at school s in year $t-1$, and ϕ is a dispersion parameter.[9] To account for intra-school correlation, standard errors are clustered by school.

Table 1 reports (quasi-poisson) regression results, with main model results in row (1). We reject the null hypothesis that the proportion of retirement-eligible faculty is not associated with diversity of the faculty. First, schools with a greater proportion of faculty over 70 have a smaller junior faculty (column (A)). An increase in retirement-eligible faculty of 12 percentage points (the magnitude observed since uncapping) is associated with a reduction of 9.3% in junior faculty. Second, schools with a greater proportion of faculty above 70 have significantly fewer minority and female faculty members (columns (B) and (C)). An increase in retirement-eligible faculty of 12% is associated with an 6.0% reduction in female faculty and 7.9% reduction in minority faculty. As seen in Panel F of Figure 9 and Table 1 row (1) column (D), we also find suggestive evidence that uncapping may have been most detrimental to the inclusion of minority female professors. The point estimate is substantively larger than the estimate for the aggregated female and minority

[9] This parameter relaxes the assumption of a conventional Poisson model that mean equals variance.

		(A) Junior faculty	(B) Female faculty	(C) Minority faculty	(D) Minority female faculty
(1) Main Sample	Prop. Over 70	-0.81^{***}	-0.51^{***}	-0.69^{**}	-1.00^{**}
		(0.32)	(0.15)	(0.27)	(0.50)
	N=7,470				
(2) Post-1994 Entrances	Prop. Over 70	-0.48^{**}	-0.40^{***}	-0.52^{**}	-0.82^{*}
		(0.24)	(0.13)	(0.23)	(0.45)
	N=7,679				
(3) HBCUs, HI & PR	Prop. Over 70	-0.49^{**}	-0.42^{***}	-0.66^{***}	-0.99^{**}
		(0.23)	(0.13)	(0.23)	(0.40)
	N=7,862				
(4) Alternative Aging Measure	Prop. Over 70 base 1993	-0.97^{***}	-0.39^{***}	-0.61^{**}	-0.88^{***}
		(0.31)	(0.16)	(0.25)	(0.34)
	N=4,111				

Table 1: Regression Results. Quasi-Poisson count regression results. Row (1) regresses proportion faculty over 70 in the prior year on count junior, female, minority, and minority female faculty with faculty size as an offset. The remaining rows present robustness checks. Rows (2) and (3) include schools opened after 1994 and HBCUs and schools located in Hawaii and Puerto Rico, respectively. Row (4) uses number of faculty over 70 divided by number of faculty in 1993 as the chief explanatory variable. All regressions have school and year fixed effects. Standard errors are clustered at the school level. */**/*** denote statistical significance at α- levels of 0.1, 0.05, and 0.01 respectively.

category. That said, due to the small number of minority female faculty members, we cannot reject the null that the effect of uncapping on minority women is the same as the effect on white women and minority men.

To further test the null hypothesis of no relationship between faculty above 70 and diversity, we again conduct a nonparametric permutation-based test. We permute the time-series vector of the proportion of faculty above 70 across schools and use the coefficient on the proportion of faculty over 70 as noted above in Equation 3 as the test statistic. If there is no effect of faculty above 70 on junior faculty hiring and diversification, the coefficients should be drawn from the placebo distribution. The results are presented in Figure 10. In contrast to what would be expected under the null, the observed coefficients fall in the tail-end of the distribution, allowing us to reject the null hypothesis of no effect of retirement eligible faculty on faculty diversity.

We present a series of robustness checks in rows (2)-(4) of Table 1. First, our main sample excluded law schools that opened post-1994.We exclude these schools in our main analysis because the research design aims to examine differences before and after elimination of mandatory retire-

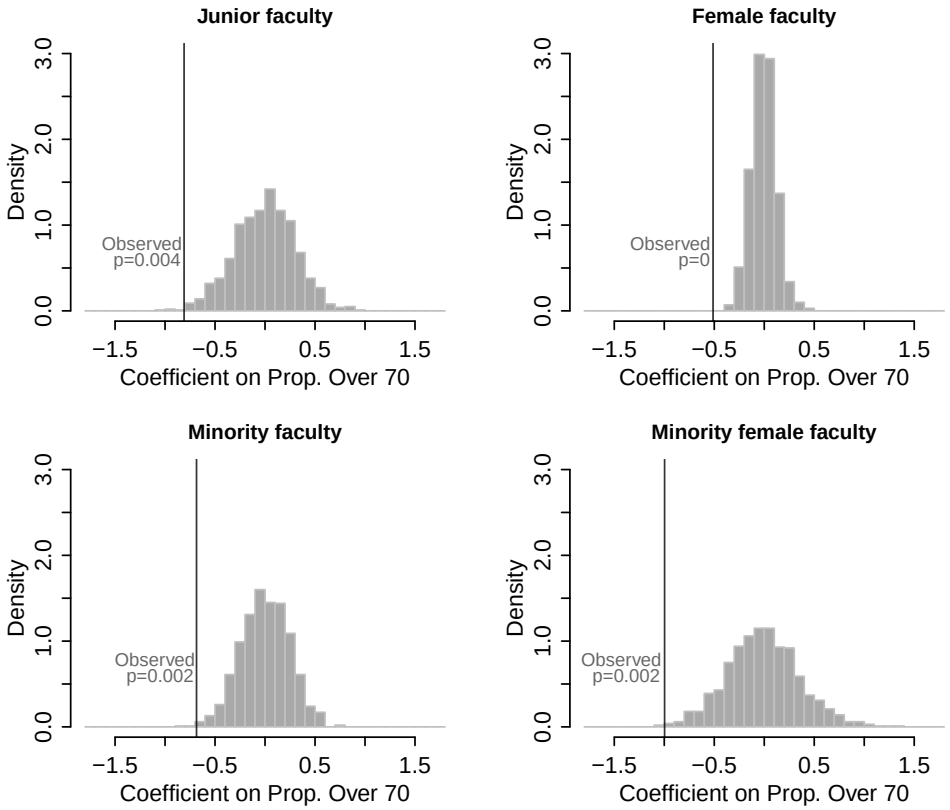

Figure 10: Permutation test. Permutation distribution of coefficients on proportion of faculty over 70 on junior (top left), female (top right), minority (bottom left) and minority female (bottom right) faculty.

ment. Yet the emergence of new law schools (a) may have been endogenous to uncapping and (b) may have mitigated effects of uncapping on diversification. By freezing the composition of incumbent law schools, uncapping may have facilitated market entrance and enabled these schools to hire more diverse faculties due to reduced hiring at incumbent schools. The creation of new schools might hence have aided the diversification of law faculty, even if diversification slowed amongst incumbent schools. Row (2) of Table 1 estimates our models including these newly established institutions, and we find comparable results.

Second, our main sample excluded historically black colleges and universities (HBCUs) and non-continental schools in Hawaii and Puerto Rico. In 1993, 61.6% of faculty were minority at these schools, compared to 10.3% at other schools. Including these schools might affect our analysis by weakening the association between retirement-eligible faculty and diversity and reducing observed diversification amongst all law schools. However, if eliminating mandatory re-

tirement reduced entry-level hiring in other schools, it may collaterally have assisted HBCUs and non-continental schools in recruiting minority academics. If true, this effect would mean that the ADEA may not have reduced diversity overall, but increased inter-school segregation. We hence estimate our models including HBCUs and non-continental schools in Row (3) of Table 1. Again, the negative associations between (a) retirement-eligible faculty and (b) junior, minority, or female faculty persist.

Third, we examine the possibility that our estimates are confounded by differential growth of schools. Schools may, for instance, have responded to the increase in retirement-eligible faculty by strategically expanding the size of the faculty, potentially motivated by the effects on faculty diversity. We assess this possibility by testing for differences in faculty size as a function of retirement-eligible faculty in the same fixed-effects framework of the previous analyses. We find no evidence that a high proportion of retirement-eligible faculty increases the size or growth of a school. This result makes sense given that many schools face a fixed number of billets and a budget constraint for growth.

A related concern is that the growth strategy of a school may simultaneously affect retirements and junior hiring. For instance, if a school has declining student enrollments, that may reduce the number of authorized faculty searches, but also lead the school to be more tolerant of delayed retirements. Alternatively, a school may be investing in growth, therefore discouraging retirements while hiring junior faculty. It is worth noting at the outset, that substantively, such school encouragement or discouragement of retirement risks liability under the ADEA, so it is not clear how likely this mechanism is. In addition, the second mechanism would, if anything, understate our findings, as it biases estimates against a finding of a negative association between retirement-eligible faculty and diverse faculty. We nonetheless construct an alternative measure of faculty aging to assess robustness to such potential differences in school growth. We do so by calculating the proportion over 70 using a static denominator, namely the faculty size in 1993, prior to federal uncapping. The measure is therefore the number of faculty over 70 in a specific year divided by the total number of faculty at a school in 1993. (Time-invariant size differences are accounted

for by school fixed effects.) Row (4) of Table 1 shows the regression results using the proportion of faculty over 70 with 1993 faculty size as the base. Because 1993 is used as the base, we fit regressions for the 1993-2017 period. Our findings remain the same.

Fourth, it is possible that as tenured faculty were less likely to retire, schools instead attempted to diversify by hiring of clinical faculty. Clinical faculty are typically hired primarily as instructors for legal clinics that teach students how to handle cases for clients. These positions have less emphasis on scholarship and academic research and typically are not on the formal tenure-track. We find no evidence to support this hypothesis. While clinical faculty are more likely to be female, the rate of gender integration slows even more dramatically post-1994 for clinical faculty. Clinical faculty are less likely to be minority, and integration along racial lines also slows post-1994 (Appendix D). These results suggest that uncapping, if anything, also affected clinical hiring.

Last, we present a wide range of additional robustness checks in Appendix F. We assess sensitivity to (a) potential changes in minority self-identification (using machine learning algorithms to impute race based on name based on census data), (b) exclusion of data after 2011, the year the AALS directory moved to a new data collection system, potentially compromising data quality, (c) including academic librarians, (d) using a fully balanced panel, and (e) including schools that underwent mergers or splits with other schools during the observation window. In all instances, the results remain comparable.

4.4 Policy Simulation

While our focus has been on Congress' decision to eliminate mandatory retirement in higher education in 1994, we here consider the substantive impact of three policy alternatives.

First, we predict faculty diversity if Congress indefinitely exempted colleges and universities from uncapping, similarly to the indefinite exemption it extended to companies with respect to high-level executives. In this scenario, we assume that schools continued to enforce mandatory retirement at age 70 throughout the observation period. Second, we consider an alternative analogous to social security reform proposals: indexing mandatory retirement age to life expectancy at

age 70 (Isaacs and Choudhury, 2017). For this simulation, we increase the mandatory retirement age to 71 in 1994, 72 in 2003, and 73 in 2009 based on increases in life expectancy in the population.[10] Third, we consider if Congress had extended the university exemption from uncapping for 15 years instead of 7 in the 1986 ADEA Amendments. Leading higher education groups such as the American Council on Education and the Association of American Universities had advocated for this longer exemption period as a way to "ease out...the large 'bulge' of faculty members who initially had been recruited into academe in the 1960s and who were scheduled to retire in large numbers only in the late 1990s and beyond" (Pratt, 1989). This proposal would have allowed schools to continue enforcing mandatory retirement until 2001 as opposed to 1994.

Using our regression estimates above, we predict faculty diversity in each year under each of these three scenarios. We calculate 95% confidence intervals using a block bootstrap, resampling with replacement by school to account for intra-school correlation. For each alternative, Figure 11 displays the number of "additional faculty," with 95% confidence interval: the difference between the number of female (left), minority (middle), and minority female (right) faculty predicted and the number observed under uncapping in 1994. As shown in the top panel, continued mandatory retirement at age 70 may have enabled significantly greater gender and racial diversity than we observe under uncapping. For example, across law schools in 2017, we would predict 140 more female professors and 80 more minority professors, including 53 minority female professors.

Shifting the mandatory retirement age gradually in accordance with life expectancy increases may have also enabled greater diversity than observed under uncapping as shown in the middle panel of Figure 11. The magnitude of the gain in gender and minority representation, however, would only be half of the gain if mandatory retirement had continued. If the mandatory retirement age had been indexed to life expectancy, we might predict 71 more female professors and 42 more minority professors, including 26 more minority female professors, across law schools in 2017. The jagged gains reflect the fact that the life expectancy adjustment is done on a yearly basis.

[10]The measure of life expectancy we use for this analysis is the average number of years a person who attains age 70 can expect to live. Life expectancy data was obtained from the National Center for Health Statistics at the Centers for Disease Control and Prevention.

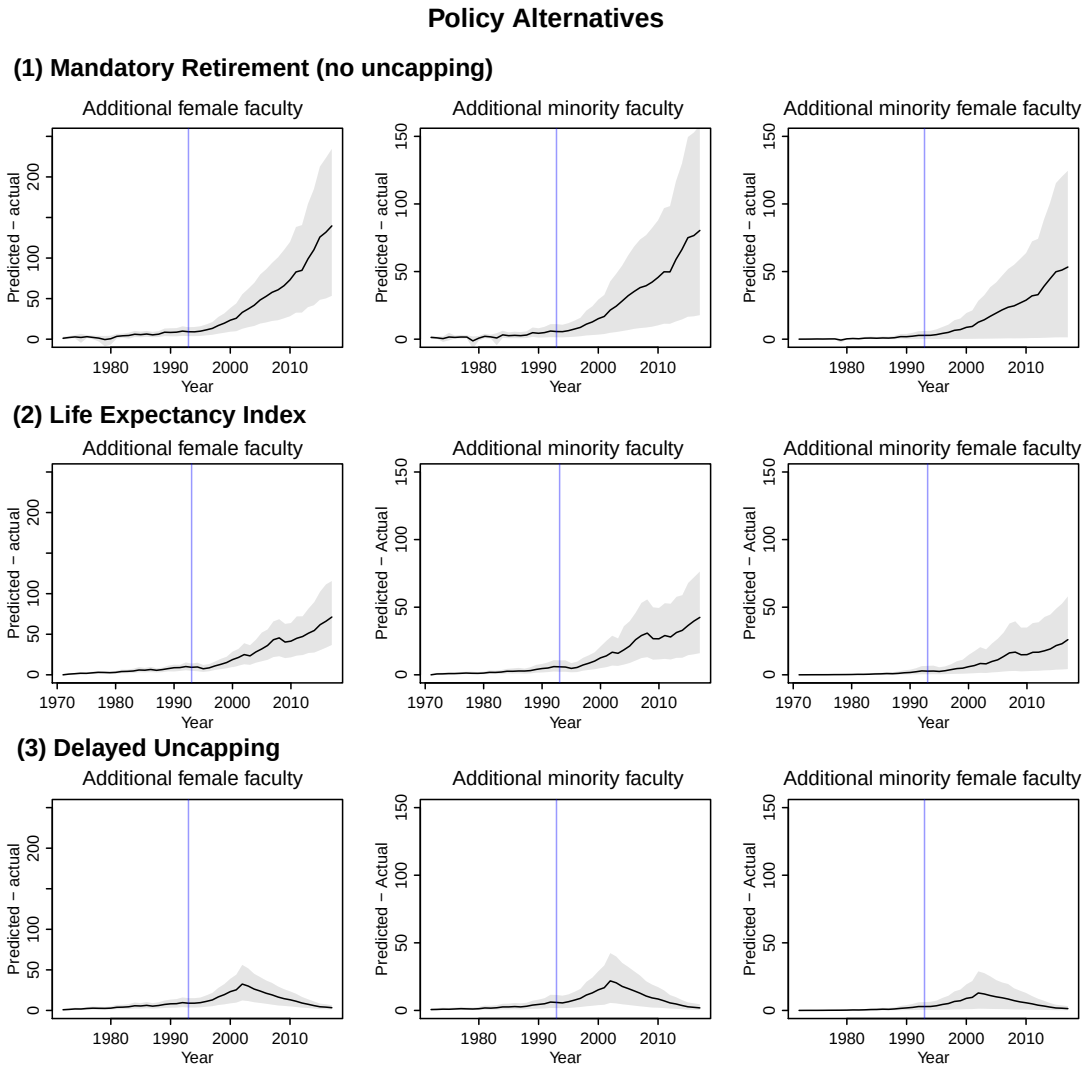

Figure 11: Policy alternatives. Difference in the number of female (left), minority (middle), and minority female (right) faculty predicted under three alternative policy scenarios and observed under the actual uncapping which took effect in 1994. The three scenarios include continuing mandatory retirement (top), indexing increases in the mandatory retirement age to increases in life expectancy at 70 (middle), and delaying uncapping until 2001 (bottom). Confidence intervals are calculated using a block bootstrap, resampling schools with replacement.

Finally, the bottom panel shows that delaying uncapping until 2001 may have resulted in short-term diversity gains in the early 2000s, but would have resulted in indistinguishable rates of diversification over the long term. Contrary to claims by proponents of delaying uncapping, retirement of bulge hires in 1990s would not have addressed the age-diversity trade-off for more than a few years.

These simulation results help substantively inform the magnitude of the effect of uncapping and calibrate the impact of policy alternatives. That said, these simulations do not account for general equilibrium effects, most importantly that alternative policies may also affect labor market entry by minorities and women. The direction of such general equilibrium effects is unclear. In a world with mandatory retirement, if schools engaged in substantially more entry-level hiring, fewer qualified females and minorities might have been available, making our estimates an upper bound. On the other hand, greater opportunities on the entry-level market may incentivize more females and minorities to enter the academic market, making our simulation results a lower bound. While such effects are hard to quantify, our simulation results suggest that the long-term effects of uncapping may have been substantial.

5 Limitations

We now note several potential limitations to our study. First, while our findings provide strong evidence that mandatory retirement would have substantially altered the age, gender, and racial composition of faculty, we cannot answer a broader counterfactual. It is possible, for instance, that with increasing life expectancy and "bulge" hires nearing retirement-eligibility, universities may independently have been pressured – absent a congressional ADEA amendment – to reform mandatory retirement policies. Our estimates should hence be interpreted as speaking to the effects of uncapping relative to retaining the pre-1994 exemption allowing universities to retain mandatory retirement policies.

Second, while we have provided comprehensive evidence of the effects of uncapping in one domain, it is unclear whether these findings generalize to higher education. There are at least

some reasons to believe that our findings generalize. Law school faculty are subject to the same tenure policies and retirement benefits as faculty in other departments.[11] The relationship between uncapping and diversification stems from three conditions that have been separately documented in other academic fields, most notably in the sciences: (1) delayed retirement of incumbent faculty, (2) increasing diversity of the entry-level hiring pool, and (3) billet and budget constraints on faculty size. The aging of STEM faculty has been widely documented (Blau and Weinberg, 2017; Kaiser, 2008), as has the increasing diversity of entry-level STEM cohorts. In the last two decades, the proportion of doctoral degree recipients in STEM fields that were women increased by between 4-11 percentage points, and the share from underrepresented racial minority backgrounds doubled (National Science Foundation, 2019). Yet, with some exceptions, the number of faculty positions in STEM fields has remained constant or grown slowly (Larson et al., 2014). While these three conditions have been studied separately, our work demonstrates that the connections between them may be critical to understanding efforts to diversify institutions.

Third, because the AALS directory does not distinguish between minority groups, we are unable to examine effects on individuals from specific minority groups (e.g., African American vs. Asian American). Understanding such nuances may be important given the evidence of different enrollment trends across demographic subgroups (Chung et al., 2017). A related concern is that self-identification may bias our findings. In the Appendix, we use name-based ethnicity imputations to show that self-identification does not appear to affect results.

Fourth, although we have spent extensive time validating our digitization of the volumes, there may still be some degree of measurement error. While such errors may affect individual data points, our large set of robustness checks presented in the Appendix suggest they are unlikely to undercut the broad patterns we report here.

Last, while our evidence suggests that uncapping may have slowed diversification at law schools, it of course remains only one policy lever. Many other dimensions affect the representation of women and minorities in universities (Moss-Racusin et al., 2012; Sheltzer and Smith, 2014), and

[11] One important distinction is whether salaries are based on "soft money," but we are not aware of evidence that suggests that aging trends are distinct across hard and soft money environments.

our study only points to one structural source.

6 Conclusion

Through collection of a novel data source, we have provided some of the richest, inter-university results to date on the effects of uncapping. Countering earlier findings that uncapping had no effects on professional schools and was associated with increased faculty diversity, we show that the magnitude of impact of uncapping at American law schools has been substantial. Eliminating mandatory retirement succeeded in reducing one form of discrimination against those it was designed to protect (individuals above 70). Due to the demographics at the time of uncapping, the immediate benefits extended primarily to white males – a finding consistent with prior research (Rutherglen, 1995; Schuster and Miller, 1984; Issacharoff and Harris, 1997). But it may simultaneously have impeded the entry of female and minority academics into faculty positions. Our results reveal an underappreciated tension internal to civil rights law: protecting one dimension (age) may undercut advancement along other dimensions (gender and race).[12] Seemingly neutral laws may have substantial disparate impact.

We close with several other points. First, our study highlights considerable weaknesses in the evidence base leading Congress to allow the faculty exemption to lapse in 1994. The leading contemporaneous reports were unable to isolate the long-run effects of uncapping. Comprehensive retrospective analyses may be much better powered to detect cumulative effects. Second, our public school results suggest that university benefits may play a substantial role in facilitating retirements. Our evidence shows that the proportion of faculty above 70 grew particularly in the wake of the Great Recession, when (defined contribution) retirement accounts faced significant losses. More generous retirement policies may directly benefit the elderly and indirectly benefit

[12]Our findings share some similarities with the tension between the use of seniority preference in employment decisions such as promotion and layoffs and the retention and advancement of women and minorities. Like uncapping, when seniority preference accrued disproportionately to white male workers due to discrimination against other groups, these preferences had adverse effects on integration (Cooper and Sobol, 1969). This tension between seniority and integration has received considerable attention in the legislative development, legal evolution, and academic study of Title VII and labor statutes (Rutherglen, 2012), but the effects of uncapping on the advancement of women and minorities has been largely overlooked.

minority and female aspiring faculty. Third, our work uncovers patterns in minority hiring that, to our knowledge, have not been documented to date, at least in the law school context. Most of the gains in minority hiring occurred in the 1980s and 1990s, with substantial flattening beginning in the mid-2000s, most acutely following the Great Recession (see right panel of Figure 8). Last, our suggestive results that the effects are acute for minority women are particularly troubling given the barriers associated with "intersectionality" in the academy (Merritt and Reskin, 1992; Multicultural Women Attorneys Network, 1994).

In sum, we hope that this study has provided more rigorous grounding of a key cause driving the shift in the age composition of university faculty and an expanded understanding of its collateral effects on efforts to diversify higher education.

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Appendix

A Data Collection

The Association of American Law Schools (AALS) annually publishes a directory of law teachers, which lists faculty and selected staff at all member and fee-paying non-member institutions of the AALS. To extract relevant information, we perform the steps described below.

A.1 PDF Scans and Optical Character Recognition

We rely on three regularly reported sections in the AALS directory: (1) the "Law Teachers by School at Member Schools / Non-Member Fee-Paid Schools" section, which lists all law teachers affiliated with a specific school; (2) the "Alphabetical List of Teachers (with Biographies)" section, which provides biographical sketches of law teachers (including birth year, gender, degrees, degree conferral dates, and employment history); and (3) the "Minority Law Teachers" listing of all law teachers who identify as a member of a minority group and their school affiliation for that year. Figure A1 displays representative pages of these sections from the 2011-2012 directory.

We start with the 1971 volume, because volumes are more regularly formatted starting in that year and because pre-1971 data is unlikely to provide much leverage over assessing effects of uncapping in 1994. For 1971-2007 and 2009-2011, we use PDF scans from HeinOnline. AALS did not publish directories in 2008, 2012, and 2013, so we manually collect data for these years (see section B.1). HeinOnline does not have volumes for 2014-2017, so we scanned these volumes ourselves. In total, our PDF scans comprise over 42,000 pages from 43 directories.

To maintain high accuracy in optical character recognition, we use Prime Recognition, which preserves line spacing, letter case, and column breaks to facilitate information extraction.

A.2 Information Extraction

We extract fields from the ASCII text streams using regular expressions in Python. For school listings, we collect faculty name and title, as well as school of affiliation for each year. From biographical sketches, we collect faculty name, title, school affiliation, birth year where available, gender where available, and degree years where available (undergraduate, law, and graduate degrees) for each year. From the minority listing, we parse the faculty name and school affiliation for each year.

A.3 Faculty Classification

We classify law teachers into tenured/tenure-track faculty, emeritus faculty, clinical faculty/instructors, and librarians based on titles. We create a dictionary of all variations of titles and map these into classifications. First, we identify the titles that plainly indicate the class. For instance, "Prof." and "Ass't Prof." are plainly tenured/tenure-track, whereas "Prof. (Adj.)" and "Visiting Prof." are not. Second, for all ambiguous titles, we verify the correct class by: (a) searching the school's online directory or obtaining the school's faculty handbook; (b) validating information in individual CVs.

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Ahmad, Muneer I., Yale
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Alvare, Helen M., George Mason
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Andrew, Ruby, Southern Univ.
Andrews, Penelope, CUNY at Queens
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Ashley, Philip S., DePaul
Atwell, Barbara L., Pace
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Badrinarayana, Deepa, Chapman
Baez, H. Beau, Charlotte
Bagchi, Aditi, Pennsylvania
Bagley, Margo A., Univ. of Va.
Bahamonde-González, José, Maryland
Bai, Lynn, Cincinnati
Bailey, Kimberly D., Chicago-Kent
Bailey, Carlton, Ark., Fayetteville
Baker, William G., Western New England
Baker, Isaiah, American
Baker, Anthony V., John Marshall, Atlanta
Baker, Angela V., Rutgers - Camden
Baldia, Sonia, Widener
Baldwin, Bridgette, Western New England
Baldwin, Anthony, Mercer
Balganesh, Shyamkrishna, Pennsylvania
Ball, Carlos A., Rutgers - Newark
Ballard-Thrower, Rhea, Howard
Balleste, Roy, St. Thomas
Baluarte, David, American

Figure A1: Page scans of AALS Directory for 2011-2012. The top left panel displays the first page of the "Law Teachers by School" listing; the top right panel shows the first page of the biographical sketches; and the bottom panel shows the minority law teachers listing.

These steps allow us to accurately classify titles and account for variation across schools. For example, while "Acting Prof." does not designate a tenure-track position at Stanford Law School, it designates a tenure-track position at UC Berkeley.

A.4 Record Linkage

We develop a record linkage algorithm to construct a relational database to observe when faculty members (a) are affiliated with a school, (b) have been granted tenure and (c) have been granted emeritus status. In each year, we match faculty across school, faculty, and minority listings, using fuzzy matching on name, title, and school affiliation.[13]

We then match faculty members across years. The algorithm used to carry out this record linkage involves the following steps, tuned after substantial experimentation:

1. Using the list of faculty members in 1971, we create a new faculty ID to uniquely identify each tenured/tenure-track faculty member.

2. For each subsequent year, we iterate through each faculty member and:

   (a) Record the name and school affiliation for the faculty member.

   (b) Search in the previous three years within that school for a fuzzy match on name.[14] For example, if 'John Doe' is at Stanford Law School in 1984, we search for name matches at Stanford from 1983 to 1981. If a match is found, the existing faculty ID is assigned.

   (c) If there is no match within school in the past three years, search all faculty members across all schools in the past three years for a fuzzy match on name and an exact match on birth year, undergraduate degree year, law degree year, or graduate degree years. If a match is found, the already created faculty ID of the match is assigned to this faculty member. In instances of direct conflicts (e.g., a match on birth year but not degree year), no match is made.

   (d) If no match is found when using supplementary biographical information, search all faculty members across all schools in the past three years for an exact match on name alone. Matches are manually verified before linking records.

   (e) If there is still no match for the faculty member, create a new faculty ID.

There are, to be sure, several limitations to this record linkage algorithm. Some faculty members may have gaps in service of more than three years and thus fail to be matched. The algorithm is less successful in linking law teachers with significant name changes across years (e.g. addition

[13] We allow for a Levenshtein distance of two when matching on each of name, title and school affiliation. This threshold is wide enough to recognize matches even when there are small differences in name representation (e.g., use of middle initial) and OCR errors in our text streams (added, omitted, or incorrectly specified characters), but is narrow enough to prevent false matches.

[14] We use a three year threshold at several points in the algorithm for several reasons. First, a three year window improves computational efficiency. Second, many schools maintain a policy for faculty to go on leave (e.g., for government service), but cap that period at two years. Third, expanding the search window increases the risk of false matches. As noted later in this section, we also compare all possible pairs of faculty and manually verify any matches. This process allows us to accurately match any faculty members who leave a school's service for more than three years.

of a middle name, contraction of first name, changing surname). We hence carry out additional checks to ensure that we are accurately identifying unique faculty members. First, we compare all possible pairs of unique faculty members in our database using a more liberal fuzzy matching on name and, where available, degree and birth years. All matches are manually verified and we resolve 531 pairs of faculty members to the same ID. Second, because we were particularly concerned that name changes are more likely for women after a change in marital status, we match on first name, biographical information, and school affiliation alone and manually verify matches.

A.5 School Meta-Data

We augment AALS directory data with school meta-data from (a) law school rankings by U.S. News and World Reports for 2004-2010; (b) American Bar Association disclosures to identify whether a school is public or private.[15]

A.6 Sample Definition

Our main analysis sample includes active (i.e., non-retired) tenured/tenure-track faculty and excludes all other teaching and non-teaching staff (e.g., lecturers, clinical faculty, and academic librarians). We perform robustness checks involving clinical faculty and librarians in sections D and F.7, neither of which change our findings.

Our main analysis also excludes a certain number of schools. Because uncapping in 1994 was a change in American law, we focus on continental law schools in the United States. We do not include law schools in Hawaii and Puerto Rico because these are outliers in their demographic composition. We also exclude historically black colleges and universities (HBCUs) and the Judge Advocate General's School because uncapping may have affected these schools quite differently. Because we identify effects based on changes, we also exclude (a) a small number of law schools that merged or split during the observation window,[16] and (b) law schools that are members or fee-paying non-members of AALS exclusively before or after their uncapping. As we show above and F.6, our results are the same whether or not these school sample restrictions are made.

[15] ABA Required Disclosures, `https://www.americanbar.org/groups/legal_education/resources/statistics/`

[16] The mergers are comprised of: (1) Hamline University Law School and William Mitchell College of Law merging to form Mitchell Hamline School of Law in 2015; (2) Rutgers School of Law - Camden and Rutgers School of Law - Newark merging to form Rutgers Law School in 2015. The splits are comprised of: (1) Pennsylvania State University - Penn State Law splitting from Pennsylvania State University - Dickinson Law to form two distinct law schools in 2015; (2) Widener University Commonwealth Law School splitting from Widener University Delaware Law School to form two distinct law schools in 2015.

B Data Validation

We implement a range of data validation and correction measures to ensure the accuracy of our database. Most importantly, we found that AALS errors are most prevalent from 2014-17, when AALS "moved to a new data collection system."[17] Our manual validations addresses these issues in the raw data, as we detail below. We also re-run all analyses excluding data from 2012-2017, as described in Section F.3.

B.1 Accounting for Missing Volumes

AALS did not publish directories in 2008, 2012, and 2013. As such, we do not have data from the AALS directly on law teachers during these years. We use a combination of manual and automated processes to populate these missing years, which allows us to uncover time trends across different dimensions. First, we assume that faculty members who appear, within the same school, before and after the missing years remained at that school. For example, if John Doe is a professor at Stanford Law School in the years 2007 and 2009, he is assumed to have also been a professor at Stanford in 2008.[18] Next, we manually investigate faculty members who (a) appear last in the year before a missing volume or (b) appear first in year after a missing volume. For example, if John Doe's last year of appearance in our data set is 2011, we manually obtain information on his *actual* last year of association with a school. We collect information on start and end of service using a variety of sources: (a) individual faculty CVs, (b) LinkedIn profiles, (c) retirement announcements by a school, and (d) the Wayback Machine (a digital archive of web pages) to examine historical faculty pages. In total, we manually collect first and last years of service for 3,500 faculty members.

B.2 Birth Years

While we successfully extract birth years from faculty members' biographical sketches for the majority of observations, birth years are reported less regularly beginning in 2007 and cease being reported entirely by 2014. This issue of missing birth years does not affect most faculty since we can populate their birth year from a previous biographical sketch that contains this information. Overall, 15.0% of tenured/tenure-track faculty members have no birth year reported in any year.

For faculty with missing birth years, we develop a simple model that predicts birth year based on degree dates. A majority (53.8%) of faculty with missing birth years report at least one degree year in a biographical sketch. For the remaining faculty, we first attempt to collect their degree dates manually using Amazon's Mechanical Turk (MTurk). Missingness is more prevalent in recent years, making it relatively easy to locate degree years. We use two MTurkers per faculty member, with interrater agreement at 96%. The MTurk response rate was 70%, reflecting the fact that some faculty are difficult to locate online. For these remaining faculty, we collect degree dates by hand, using contemporaneous publications that sometimes list degree year, as well as archived

[17] Under this new system, deans provided AALS with a roster of their faculty and staff. Faculty members who previously appeared in the directories were then expected to log on to the online portal to make any necessary updates to their prior directory entry. New faculty were asked to create their directory entry through the online portal.

[18] Our process excludes the possibility of faculty both starting and ending a new faculty job within the missing years. This possibility seems unlikely, as it would be very rare for a faculty member to move laterally and return to the old faculty within two years.

announcements of achievements (nominations, hiring, endowments, etc.) with more detailed biographies.

We train simple regression models to predict birth years based on degree years, using faculty members who report both. We concentrate on three degree types: undergraduate degree (BA, BS, AB, etc), law degree (JD), and Bachelor of Law (LLB). In the U.S. system, law school requires an undergraduate degree and LLB's are primarily from foreign institutions. We randomly split the population of faculty with both birth and degree years observed into 80% training set and a 20% test set. We fit separate regressions for each degree type using the training set, with interactions of degree year and degree decade to account for secular trends. We give priority to predictions based on undergraduate degrees, followed by law degree, and then LLB's. After plotting the correlation between predicted birth year and reported birth year for a (random) test set, we fit a least squares line, which yields an R^2 of 0.98.

After this imputation, we are left with 2.6% of all faculty without observed or predicted birth years. These faculty members are spread uniformly across all years of observation and unlikely to create any biases in our results. We omit these faculty from our analyses involving age.

B.3 Gender

Biographical sketches include gender from 1986-2011. Of faculty who appear before 1986, 63.7% reported gender after 1986. Similarly, of faculty who appear after 2011, 75.0% reported gender before 2011.

There are three remaining sources of missingness. First, there are 2,451 faculty members who appear and leave the directories before 1986. Second, 651 faculty appear for the first time after 2011. Third, there are 1,979 faculty who appear between 1986-2011, but do not report gender at any point.

For these remaining faculty, we use name and birth-year-based methods to predict gender. We investigated several available methods. We assess the accuracy of these models on our own data, using faculty members who report a gender in their biographical sketch as the test set. We select the model based on U.S Social Security Administration baby names data because it had the highest accuracy rate: 99%.[19]

We are able to predict gender of 95.6% of faculty with missing gender. The remaining group of 222 faculty members mostly had non-traditional U.S. names. For these remaining faculty, we manually collected gender. For further validation, we examine the weakest classifications (mostly gender-neutral names such as "Taylor") and manually verify gender. We omit the 26 faculty members whose gender could not be verified online.

B.4 Minority Status

The Minority Law Teachers listing is available from 1986-2017. We rely on the minority listing because it is the only source we are aware of that provides reliable, multi-institution, multi-year data on the racial composition of tenured/ tenure-track faculty at U.S. law schools. These data are

[19]We use the R package 'gender'(Mullen, 2018) (`https://github.com/ropensci/gender`). It provides the option to use models based on historical datasets from the U.S. Social Security Administration, the U.S. Census Bureau (via IPUMS USA), the North Atlantic Population Project, or the Kantrowitz corpus of male and female names to provide predictions of gender.

commonly used in studies on the demographics of law faculty (Bell and Delgado, 1989; Redding, 2003; McCrary et al., 2016). The few studies on this topic that do not use the AALS directories rely on sources that provide only a limited number of years or institutions (Chused, 1988).

We assume that faculty members who appear in any year's minority listing are minorities in all years. Faculty members who do not appear in any minority listing are assumed to be non-minorities. For faculty in the years prior to 1986 (before the AALS begins publishing the minority listing), we are able to infer 67.5% of their minority statuses due to post-1986 service.

We consider three reasons why our data on faculty minority status may not be complete and how these issues would affect our results. First, there are 2,451 faculty members who were never asked to report their race/ethnicity to the AALS because they left legal academia prior to 1986. While we expect that a large majority of these faculty were white based on descriptive studies at the time, some of these faculty may have identified as minority had they been asked by the AALS (Chused, 1988). Because of this issue, our data may be understating faculty diversity prior to 1986. Second, minority faculty may choose not to self-identify. This issue would also imply that our data underestimates faculty diversity. Moreover, if the propensity of minority faculty to self-report changes during the observation window and is correlated with faculty age composition, this issue would confound our estimates of the effect of faculty age composition on diversification. Third, the AALS's move to the new data collection system towards the end of the observation window (2014-2017) may have increased non-response overall (see Section B). Thus, some minority faculty in recent years may not appear on the listing because they did not complete any part of the questionnaire before the end of our observation window. If non-response of minority faculty increases towards the end of the observation window, this issue might have contributed to the declines in the rate of diversification that we observe.

We address these potential concerns in several ways. First, we compare our data to the few other sources on faculty demographics across law schools that exist. These sources offer the added advantage of providing data from before 1986 (before the AALS directories included the minority listing) (concern 1) and data that is not strictly self-reported (concern 2). Specifically, we compare our data to Chused (1988), who asked administrators at AALS law schools in 1980 and 1986 to report faculty demographic information, and to a special release of data from the ABA, which collected data from administrators at ABA-approved law schools on minority tenured/tenure-track law faculty in the fall of 2013.[20] We do not find evidence that our data understate the number of minority faculty prior to 1986 or in subsequent years. Chused reports that the proportion of tenured/tenure-track professors who were minority was 3.6% in the 1980-1981 academic year, which is comparable to the proportion of tenured/tenure-track professors identified as minority our dataset in that year: 4.8%. In later years, we also find that data from external sources is consistent with ours. In the 1986-1987 academic year, Chused reports that 5.0% of tenured/tenured-track faculty were minority and our data reports 7.5% for this year. In 2013, 19.8% of tenured/tenure-track faculty were minority according to the ABA data, compared to 18.1% in our data. The small discrepancies between these benchmarks and our data likely stem from differences in the sample of schools used to generate these numbers.[21]

[20] These data were collected as part of the ABA's 2013 Annual Questionnaire. Data accessed on May 15, 2019, https://www.americanbar.org/groups/legal_education/resources/statistics/statistics-archives/

[21] For example, Chused received responses from 144 schools in 1980 and 149 schools in 1986, whereas our full sample of schools consists of 170 and 175 institutions in these years. While Chused does not provide detail on all the schools that did not respond, we expect that Chused may have reported fewer minority faculty than our data because

Second, we also consider the possibility that the propensity of minority faculty to self-identify has declined and that such patterns of self-identification may be correlated with institutions' faculty age composition (concern 2). To be sure, researchers have found a variety of individual and contextual factors shape decisions about racial self-identification (Sen and Wasow, 2016; Yoshino, 2006). However, we do not find evidence that minority self-identification has systematically declined during the observation period (Roth, 2016; Sen and Wasow, 2016). If anything, prior research suggests the reverse.[22] Even if one thinks that declines in self-identification occurred among minority individuals, it is less plausible that such declines are more acute after 1994 specifically in schools with high proportions of retirement eligible faculty. If the age composition indeed affected the propensity by faculty to racially "cover," that would itself be a notable treatment effect of uncapping (Yoshino, 2006).

Third, to address all three concerns, we use an additional measure of minority status: name-based predictions of faculty race/ethnicity. We note two limitations from the outset: (a) this method is often unable to identify minority individuals who have surnames that are not typically or exclusively associated with a minority group; and (b) this method is particularly ill-suited to identify minority women who adopt the surname of a non-minority partner (or vice versa). Notwithstanding these limitations, this method is frequently used in a range of research fields (see, for example, (Imai and Khanna, 2016; Elliott et al., 2009)). We identify four imputation methods that predict race/ethnicity from first and/or last name using models that differ based on data and learning algorithm. [23] Identifying the package with the most accurate predictions for our dataset is inherently difficult, due to the subjective nature of racial identity and the absence of ground truth (Roth, 2016). From sampling faculty predicted to be minority by each of the packages, we find that (a) models appear more accurate in predicting Hispanic and Asian or Pacific Islander (API) faculty than African American faculty and (b) the prediction package which uses 2010 census data on an LSTM model seems to produce the most accurate predictions (for API and Hispanic faculty). Therefore, we select the LSTM model and use its predictions for API and Hispanic. The minority prediction package identifies an additional 124 to 360 API or Hispanic faculty, depending on

his data included fewer predominantly minority institutions. Chused reports that five "minority-operated" institutions (HBCUs, schools in Puerto Rico and schools in Hawaii) responded to the survey in 1980 and six responded in 1986, whereas our dataset includes nine of such institutions in these years. The ABA data comes from ABA-approved law schools, which totaled 203 in 2013, whereas our full data used to generate the numbers reported above consists of 197 schools.To be sure, the discrepancy may be larger because the ABA noted there was non-response on questions used to generate the report.

[22]Researchers have found evidence of increases in the propensity of individuals to identify as American Indian/Alaska Native: "In each census since 1960, there have been hundreds of thousands of new American Indians – people who joined the population through response changes rather than birth or immigration" (Liebler and Ortyl, 2014). Some of the largest proportionate increases in American Indian/Alaska Native self-identification occurred among a subset— highly-educated adults—that encompasses our population of interest (law school faculty) (Liebler and Ortyl, 2014). While this shift may have contributed to the uptick in diversification in the early part of our observation window, it cannot explain the decline in diversification rates that we observe following uncapping: American Indian/Alaska Native self-identification continued to increase in the two censuses following uncapping (2000 and 2010) (Liebler and Ortyl, 2014). Liebler et al.'s comparison of individuals' responses in the 2000 Census to their responses in the 2010 Census also found that it was more common for individuals to shift from identifying as white in 2000 to identifying as a minority in 2010 than the reverse (Liebler et al., 2014).

[23]The python package ethnicolr (https://github.com/appeler/ethnicolr) provides 3 main methods for ethnicity prediction: (1) an long short-term memory (LSTM) model trained using census data for 2000 or 2010, (2) an LSTM model trained using Wikipedia data, and (3) an LSTM model trained using Florida voter registration data. The R package wru (https://github.com/kosukeimai/wru) uses a Bayesian prediction model trained on Census data.

prediction threshold, who are not identified as minority in the AALS. As a robustness check, we assess whether the time trends and regression results persist when incorporating these predictions (Section F.4).

Finally, we present results using data that excludes years before 1986 and after 2014 (concerns 1 and 3). See Section F.4 for results.

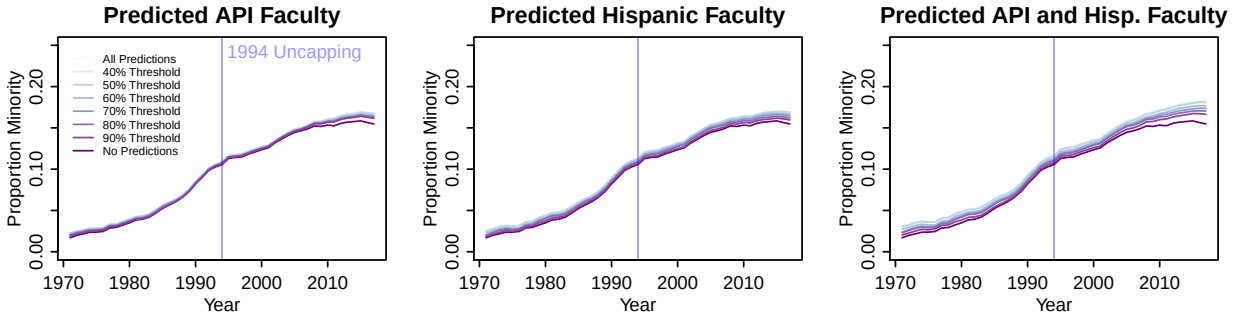

Figure A2: Minority Predictions. Proportion faculty predicted to be Asian or Pacific Islander (API), Hispanic, or either over time. An LSTM model trained using 2010 census data was used to generate probabilities of faculty members being one of white, black, API or Hispanic based on last name. The "No Prediction" line plots the proportion of all faculty members who self identify as a minority in the AALS directories across time, analogous to the right panel of Figure 8 in the manuscript. The 80% threshold line represents the proportion of faculty members who self-identified as a minority *or* were predicted with probability greater than 80% to be API, Hispanic or either (left, center and right panels respectively). Similar logic was used for all other threshold percentages. "All predictions" represents the proportion of faculty members who self-identified as a minority *or* had the highest probability (regardless of absolute value) of being API, Hispanic, or either (left, center and right panels respectively).

B.5 Titles

Some individuals provided no titles in either the school or biographical listing. This issue of missing titles primarily affects 2014-2017. While only one individual had a missing title in 2011, there were 223 such individuals in 2017. Much of this appears to be explained by the transition to AALS's new data collection system, since missing titles disproportionately appear to affect new junior faculty hired in this period. We suspect that this stems from a delay in entering biographic details to AALS, as the rate of missingness spikes in 2014 and decreases by 2017. Such a pattern of missingness could potentially lead to underestimates of the number of junior faculty hired in recent years, therefore biasing our results.

To address this issue, we identify the nearest title within a three-year bandwidth for each affected faculty member. For instance, for a faculty member missing a title in 2014, we search for titles in the three preceding and three subsequent volumes for that faculty member.[24] The most proximate title found is then assumed to be their title in 2014. With this automated process, we were able to populate the titles of more than 80% of faculty with missing information.

Second, we manually search for titles (and other biographical details as described above) for all remaining faculty members through web searches. Because most of these hires were recent,

[24]The years would be 2015-17 and 2009-11, as AALS did not publish directories in 2012-2013.

this was a straightforward way to complete the AALS title information.

B.6 Emeritus Status

Starting in 2014, we find that a number of faculty members who had recently been conferred emeritus status did not update their titles in the AALS directory to reflect this change. This issue would inflate the average faculty age, as the retirement of (usually) older faculty members would not be reflected until several years after actual retirement or not at all.

To address this issue, we perform a manual check of all faculty members over the age of 70 in the years 2014-2017 who do not report an emeritus title. As before, we conduct a series of web searches for faculty pages and personal pages, including pages that are archived on the Wayback Machine. Of 1,372 faculty members, we find that 540 had retired or taken emeritus between 2014 and 2017. We record their retirement year and treat them as retired / emeritus for all subsequent years.

C Applicant Pool

We now investigate whether the declining rate of diversification after 1994 may be attributable to changes in the applicant pool. This mechanism may be particularly relevant after the Great Recession. The financial shock may have simultaneously caused faculty members to stay active longer than anticipated and disproportionately led attorneys of color to stay at law firm positions rather than risk going on the entry-level job market. (It is common for attorneys to practice for several years before going on the legal academic job market.)

To examine this potential explanation, we compile data on the demographics of applicants for entry-level teaching positions from two sources to cover 1990-2017. First, from 1990-2008, we use information on applicants from AALS's annual Statistical Reports on Law School Faculty and Candidates. Second, because AALS ceased publishing these statistics in 2008, we rely on hand collected applicant demographic information from the AALS Faculty Appointments Register from 2000-17, which contain application forms of all applicants during these years. The overlapping period allows us to examine discrepancies due to the different measurements (e.g., for multi-racial candidates). One limitation of these data sources is that they do not consistently report statistics for minority female job applicants.

Figure A3 shows that the proportion of job applicants who are female and the proportion of job applicants who are minority has been increasing. We interpret these finding as evidence that the decline in the rate of diversification cannot be attributed to a decline in female and minority applicants for faculty positions. If anything, the proportion of female and minority applicants increased after the Great Recession, potentially due to layoffs at law firms.[25]

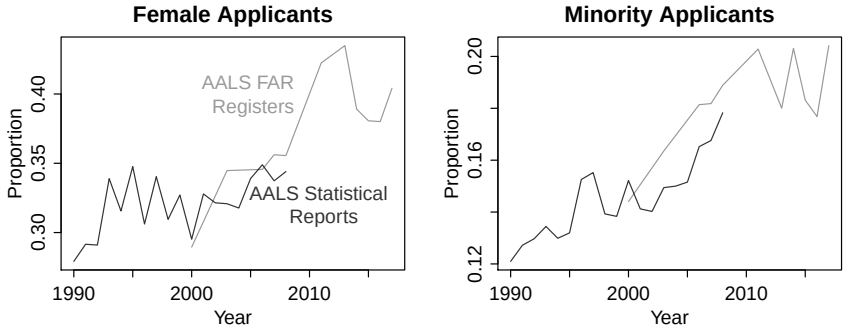

Figure A3: Applicants for faculty positions over time. The left panel plots the proportion of applicants who were female, while the right panel displays the proportion of minority applicants. The darker line reflect data from the Association of American Law Schools'(AALS) Statistical Report on Law School Faculty, which was available for the years 1990 to 2008. The lighter gray line reflects data from the AALS Faculty Appointments Register for the years 2000 to 2017.

[25] We cannot say anything about the quality of the applicant pool. It is possible, for instance, that layoffs caused an increase in less competitive applicants, but it is difficult to systematically measure the quality of the applicant pool.

D The Growth and Diversity of Clinical Faculty

Our main analyses exclude clinical faculty for two reasons. First, most clinical faculty are not on the tenured / tenure-track line, and hence unaffected by uncapping (Adamson et al., 2012). Second, even in the rare instances that clinicians are tenured / tenure-track, because directing a legal clinic can require significant day-to-day management, such positions may not be as prone to the concerns that originally motivated the exemption in 1986.

That said, we encountered some uncertainties in inferring whether a faculty member is tenured / tenure-track or a clinical faculty member based on the title. For instance, a title of "Ass't. Prof. & Dir. Clinical Educ." may be tenure track or a clinical title. We sampled faculty and found that in most cases, faculty members who are directors of clinics, but also have titles that imply they are tenured / tenure-track (as in the example above), are on the tenure line. As such, we include 544 of such faculty in our main analysis. Faculty with titles such as "Dir., Legal Aid Clinic" or "Lect. and Dir., Civil Clinical Prog.", however, are not considered tenured/tenure-track.

We assess sensitivity to this measurement here by re-estimating models excluding all individuals who are listed as directors of clinics from our sample. Row (A) of Table A1 shows results are comparable.

One concern with the elimination of mandatory retirement has been that universities shifted to contract or contingent (untenured) faculty (Ehrenberg, 2006). It is hence possible that universities have generated the appearance of greater diversity by hiring untenured faculty. In law schools, the rise of experiential education has contributed to growth in untenured positions. Since the 1960s, American law schools have expanded opportunities for students to develop professional practice skills through participation in clinics. Beginning in 2005, the ABA began adding experiential learning requirements for all students in law school accreditation standards (Adamson et al., 2008). As a result, the number of clinical faculty positions, which are most often non-tenured, has grown substantially over the past several decades (Adamson et al., 2008).

Our data on clinical faculty allow us to assess whether law schools may have diversified faculties through clinical hiring. The left panel of Figure A4 plots the proportion of faculty comprised of clinicians, demonstrating substantial grown from the 1970s to the present. This growth is consistent with historical accounts of the rise of legal education (Carey, 2002). The methodological challenge here is that a number of schools cease reporting clinical faculty after 2011. We hence focus our analysis on the 1971-2011 period. The middle and right panels of Figure A4 show time series plots of the proportion of clinical faculty that are female and minority, respectively. We observe that compared to tenured/tenure-track faculty, clinical faculty are substantially more likely to be female, but less likely to be members of a minority group. In both instances, diversification appears to slow down after uncapping. These findings do not corroborate the notion that uncapping shifted schools to diversification using clinical lines.

		Junior faculty	Female faculty	Minority faculty	Minority female faculty
(A) Clinical Definition	Prop. Over 70	-0.80^{**}	-0.53^{***}	-0.69^{**}	-1.03^{**}
		(0.32)	(0.16)	(0.27)	(0.53)
	N	7,470	7,470	7,470	7,470
(B) Linear Model	Prop. Over 70	-0.18^{**}	-0.23^{***}	-0.08^{**}	-0.04^{*}
		(0.09)	(0.05)	(0.04)	(0.02)
	N	7,470	7,470	7,470	7,470
(C) Junior Faculty	Prop. Junior		0.51^{***}	0.32^{*}	0.46^{*}
			(0.07)	(0.17)	(0.24)
	N		7,470	7,470	7,470
(D) Post-2011 Data Quality	Prop. Over 70	-0.56	-0.38^{*}	-0.79^{**}	-0.88
		(0.38)	(0.19)	(0.34)	(0.73)
	N	6,489	6,489	6,489	6,489
(E) Post-1986 Minority	Prop. Over 70			-0.44^{*}	-0.82^{*}
				(0.26)	(0.42)
	N			5,290	5,290
(F) Fully Balanced Panel	Prop. Over 70	-1.04^{***}	-0.56^{***}	-0.85^{***}	-1.04^{**}
		(0.36)	(0.16)	(0.29)	(0.53)
	N	6,256	6,256	6,256	6,256
(G) Mergers & Splits	Prop. Over 70	-0.51^{**}	-0.40^{***}	-0.54^{**}	-0.85^{*}
		(0.25)	(0.13)	(0.23)	(0.46)
	N	7,482	7,482	7,482	7,482
(H) Librarians	Prop. Over 70	-0.52^{**}	-0.32^{**}	-0.49^{**}	-0.83^{*}
		(0.25)	(0.13)	(0.23)	(0.47)
	N	7,470	7,470	7,470	7,470

Table A1: Regression Results for Additional Robustness Checks. Rows (A), (C)-(H) present (quasi-poisson) count model results and Row (B) presents linear regression results. Rows (A), (D)-(H) regress proportion faculty over 70 in the prior year on count junior, female, minority, and minority female faculty with faculty size as an offset. Row (B) regresses proportion faculty over 70 in the prior year on proportion junior, female, minority, and minority female faculty. Row (C) regresses proportion junior faculty in the prior year on proportion female, minority, and minority female faculty with faculty size as an offset. Row (A) excludes directors of clinics with titles that appear to indicate tenured/tenure-track status. Row (D) excluded directory listings from 2014-2017. Row (E) excludes directory listings from 1971-1985. Row (F) uses only schools that appear in all years from 1971 to 2017. Row (G) includes schools that were the product of mergers or that split during the observation window. Row (H) includes librarian faculty with titles that appear to indicate tenured/tenure-track status. Observations are at the school-year level. All regressions have school and year fixed effects. Standard errors are clustered at the school level. */**/*** denote statistical significance at α- levels of 0.1, 0.05, and 0.01 respectively.

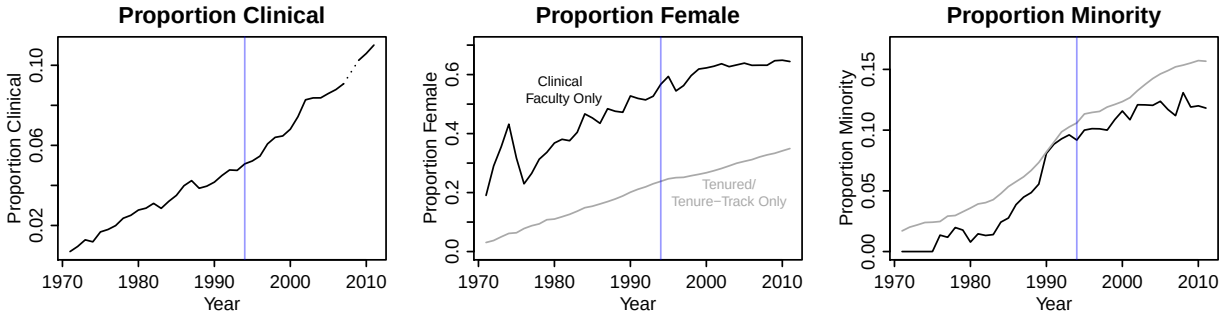

Figure A4: Clinical faculty over time. The left panel plots the proportion of all faculty that are clinical. We do not manually collect data for clinical faculty for the 2008 missing volume - this data point is extrapolated and represented using a dotted line. The middle panel shows the proportion female across time and the right panel shows proportion minority. In the middle and right panels, plots of proportions for tenured/tenure-track faculty only are added for reference. The vertical blue line in all plots shows the federal uncapping year (1994).

E Alternative Measure of Faculty Retirement

One of the challenges to our survival analysis is that faculty may leave the AALS directory for many reasons other than retirement: e.g., death or taking a non-faculty position. In the main analyses, we hence condition on a faculty member being above age 50 to construct our cohort survival analyses. We here consider one alternative measure for retirement, namely when an individual is awarded emeritus status.

At the outset, we note one principal limitation to this measure. The meaning of "emeritus" status has changed considerably over time, and may itself be affected by uncapping. During the beginning of our observation period, the status was an honorific title, conferred to the most distinguished professors (Mauch et al., 1990). Over time, conferring emeritus appears to have become more common and provide a wider array of privileges, plausibly because universities have grappled with providing incentives for retirement (Mauch et al., 1990; Burton Jr., 1987).

We nonetheless examine the age at which emeritus status is granted to faculty, plotted in Figure A5. The left panel presents the distribution pre-1994, showing a sharp spike before the age of 70. The right panel presents the distribution after 1994, which suggests a much longer right tail. These findings corroborate that uncapping has led to delayed retirement.

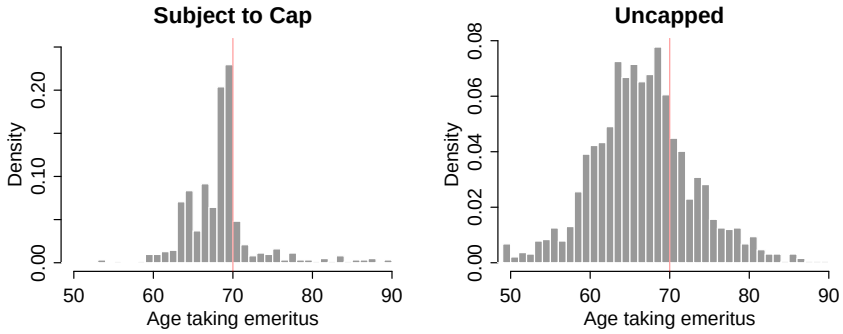

Figure A5: Age at Emeritus Status Conferral. Distribution of the age at which faculty take emeritus status for faculty who turned 70 prior to (left) and after (right) uncapping. For visibility, ages are trimmed at 50 and 90 years old.

F Robustness Checks

We now present a series of additional robustness checks.

F.1 Linear Model

First, instead of using a count model, Row (B) of Table A1 presents results from a simple linear regression that looks at the relationship between the proportion of faculty over the age of 70 in the prior year and the proportion of faculty that are junior, female, minority and minority female. Our results persist in this linear specification.

F.2 Mechanism of Junior Faculty Hiring

In our main analyses, we presented evidence that the proportion of faculty over 70 is significantly negatively correlated with the count of junior faculty and, separately, that the proportion of faculty over 70 is also significantly negatively correlated with the number of female, minority, and minority female faculty members. We supplement these findings by presenting models that directly estimate the effects of junior faculty size on the number of female and minority faculty members. We use the lagged proportion of junior faculty in the prior year as the explanatory variable to exclude the direct effect a hire in a specific year. Row (C) of Table A1 presents the results. As expected, we find that schools with a smaller junior faculty have significantly lower levels of racial and gender diversity. These results provide further evidence of the mechanism underlying the trade-off between delayed retirement of senior faculty and diversification: delayed retirements reduce schools' opportunities for hiring junior faculty, which in turn limits diversification.

F.3 Post-2011 Data Quality

As discussed in section B, we find that data quality declines in the years 2014-2017, when AALS transitioned to a new data collection system. While we thoroughly address these issues through a combination of manual and automated processes documented above, we investigate here whether our results are affected by any remaining data quality issues. We fit regressions excluding data from the post-2014 period. Because AALS did not publish any directories in 2012 and 2013, we hence use only data from 1971 - 2011. Row (D) of Table A1 presents results, which are substantively comparable, with two slight differences. The coefficients on junior faculty and minority female faculty become statistically insignificant, but point estimates remain at the same magnitude. Statistical precision of estimates is likely driven by the sharp rise in retirement-eligible faculty after the Great Recession.

F.4 Minority Robustness

As reported in Section B.4, we account for potential under-reporting in the AALS minority list by cross-referencing faculty members over time and using model-based techniques to impute ethnicity based on year and age. We now conduct two further sensitivity analyses.

First, we address the possibility that we are undercounting minority presence before AALS published the minority listing in 1986. Row (E) of Table A1 presents the minority faculty regression results excluding the years 1971-1986. Results are substantively the same. Second, after using the prediction models described in Section B.4, we plot time trends for the proportion of faculty members predicted to be API and Hispanic at different probability thresholds, as shown in Figure A2. The figure also includes those faculty members who appear in the AALS minority listing. We find that the trend of diminished minority growth after the 1994 uncapping persists with the addition of predicted API and Hispanic minority faculty, regardless of probability threshold. We also re-fit models using different probability thresholds for whether a faculty member is minority. Table A2 presents results, which are again substantively comparable.

	Minority Faculty							
	Predicted API				Predicted Hispanic			
	All Pred.	40%	60%	80%	All Pred.	40%	60%	80%
Prop. Over 70	-0.55^{**}	-0.53^{**}	-0.63^{***}	-0.62^{***}	-0.47^{**}	-0.46^{**}	-0.41^{*}	-0.44^{*}
	(0.22)	(0.22)	(0.23)	(0.23)	(0.21)	(0.21)	(0.22)	(0.23)
School FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A2: Regression Results Using Minority Predictions. Quasi-Poisson count regression of faculty self-identifying as minority or predicted as API (left columns) or Hispanic (right columns) at different classification thresholds of ethnicity prediction, with proportion faculty over 70 in the prior year as chief explanatory variable and faculty size as an offset. Observations are at the school-year level. $N = 7,470$. FE indicate fixed effects. Standard errors are clustered at the school level. */**/*** denote statistical significance at α- levels of 0.1, 0.05, and 0.01 respectively.

F.5 Fully Balanced Panel

In our main analyses, 137 of 166 schools included are observed for the full observation window (1971-2017). While the remaining 29 schools are observed before and after uncapping, they are not present for the full period. 25 of these 29 schools became members or fee-paying non-members of the AALS after 1971 (between 1972-1989) and two pairs of schools merged in 2015. We include these schools in the main analyses because they allow us to examine effects on schools existing both in the capped and uncapped schemes. Some schools might exit, for instance, if uncapping negatively affected productivity and quality of teaching. One disadvantage to our main sample, however, is that the composition of schools changes. The intensive margin (faculty composition) may be distinct from the extensive margin (exit). We hence fit models on a fully balanced panel of institutions present in the data from 1971-2017. As Row (F) of Table A1 shows, the results are substantively the same.

F.6 Splits and Mergers

We also assess sensitivity to including schools that were subject to splits and mergers during the observation window. In addition to two instances where two schools merged, two schools split. It

is possible, however, that such reorganizations are a response to uncapping. Mergers could have been partially affected by adapting to the growth in retirement-eligible faculty. And splits might have been influenced by the potential to re-build a faculty in light of demographic trends. We hence fit count models including these mergers and splits, and find comparable result as shown in Row (G) of Table A1.

F.7 Librarians

In our main analyses, we exclude librarians. Yet a minority of schools confers "faculty status" on librarians.[26] The effect of uncapping on the likelihood of academic librarians to retire remains unclear, as such status is typically conferred on the director of a law library, which is accompanied by significant managerial responsibilities. Law librarians may therefore have fewer incentives to stay long into retirement age.

We nonetheless examine whether our results are sensitive to the exclusion of law librarians. We add to our main sample all librarians whose title appears to indicate faculty status. For instance, we include individuals denoted as "Librarian and Ass't. Prof." We do not include librarians whose titles affirmatively suggest no faculty status (e.g., "Adj. Ass't. Prof. and Librarian"). Row (H) of Table A1 presents regression estimates including librarians, with comparable results. We note one additional finding, which is that a number of law schools appear to have diversified the faculty early on via the hiring of law librarians with faculty status.

[26] See Parker (2011) ("Consequently, today only between one-quarter and one-third of law librarians report holding faculty status."). This norm has admittedly changed over time. Compare Bailey and Dee (1974).