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

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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 crosscutting effects of civil rights laws: preventing age discrimination can have collateral effects on racial and gender integration.

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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 7-year exemption for faculty at institutions of higher education and mandated further research on potential effects of "uncapping" on colleges and universities. The perceived benefits of uncapping included the retention of talented faculty, increased generational diversity, and elimination of age discrimination. Some voiced concerns about fiscal pressures, innovation, and productivity (Pratt, 1989; Casper and Mac Lane, 1990). 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

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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 (Hershel and Liu, 2009; Conn, 2010; Blau and Weinberg, 2017; Ghaffarzadegan and Xu, 2018). 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 of 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 crosscutting 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 50 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. These data—rare in its scope across institutions, its comprehensiveness within institutions, and its inclusion of individual demographic detail—permit 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 10-fold after uncapping. Among faculty who would have been subject to mandatory retirement between 1994 and 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 3 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 (Clark et al., 2001; Ehrenberg et al., 2001; Clark and Ghent, 2008; Larson and Gomez Diaz, 2012; Weinberg and Scott, 2013) 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 nonprofessional 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; Bertrand, 2011; Bayer and Rouse, 2016; Nielsen et al., 2017, 2018). 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 (Donohue and Siegelman, 1990; Oyer and Schaefer, 2003) 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 face in securing protections (Delaney and Lahey,

1. See also Supplementary Appendix Figure A3.

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).[2]

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 and 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 Supplementary 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 semiautomated record linkage methods to structure the data as a relational database of faculty and schools over time. To ensure that

2. Similar tensions have been documented in other domains. For arguments that affirmative action's beneficiaries have primarily been white women, see Crenshaw (2006) and Wise (1998). For an example of external effects of employment agreements, see Starr et al. (2019).

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 semiautomated 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.[3]

Our data have 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 data set 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.[4]

Figure 1 displays the faculty age distribution for each year before uncapping (left panel) and after uncapping (right panel), demonstrating a

3. See Supplementary Appendix B4.
4. See Supplementary Appendix G6.

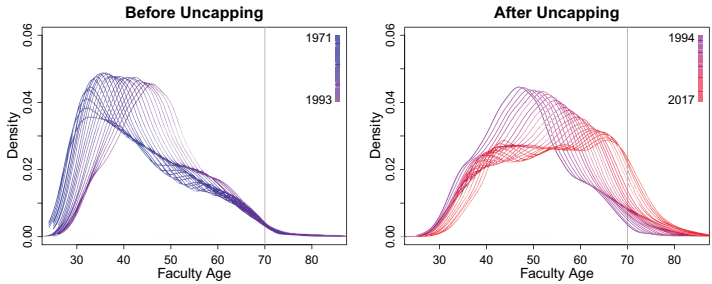

Figure 1. Kernel Density Plot of Faculty Age across All Schools for Each Year.

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.

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.[5] To place this increase in context, Figure 3 compares population demographics over time. The

5. 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

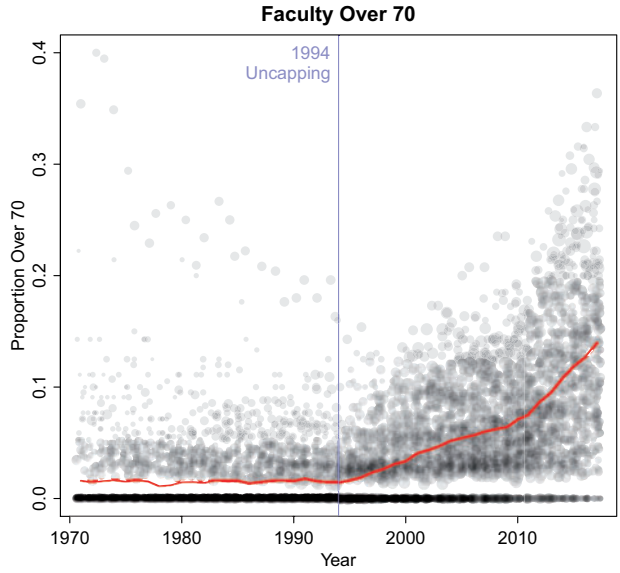

Figure 2. Proportion of Faculty over the Age of 70.
Notes: Each dot represents a school and the size of the dots represents the overall faculty size. The black line represents the average proportion across all schools for a given year.

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 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 3 years before uncapping (1991–93) with faculty turning 70 during the three years after uncapping (1995–97). Supplementary Appendix C shows that these two cohorts are balanced on observable characteristics. 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

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).

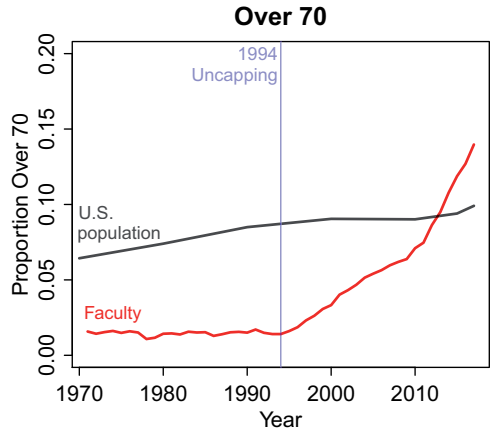

Figure 3. Proportion of Faculty over the Age of 70.
Notes: The black line represents the average proportion of faculty over 70 across all schools for a given year. The gray line represents the proportion of the U.S. population over 70 for a given year. Source for U.S. population data is the U.S. Census.

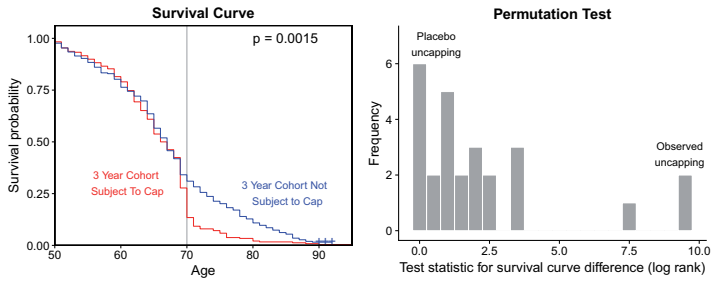

Figure 4. Survival Analysis.
Notes: Left: Kaplan–Meier survival curves for a cohort subject to the cap in light gray (i.e., tenured faculty active at the age of 50 who turned 70 in the 3 years before uncapping) and a cohort not subject to the cap in dark gray (i.e., tenured faculty active at the age 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 in 3 years before and after the observed uncapping year and placebo uncapping years (all other years between 1988 and 2013).

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 to be explained by secular demographic shifts.[6]

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 and 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

6. The attribution to uncapping would be incorrect if schools adopted different hiring strategies (e.g., making only lateral hires) around 1994 or if there were other post-1994 cohort-specific shocks independent of uncapping. We are not aware of such contemporaneous changes affecting the retirement-eligible cohort.

mandatory retirement policies. Among faculty who would have turned 70 between 1994 and 2017, 39% elected to work past age 70. As of 2017, these faculty have served past age 70 for 5 years on average and 6,815 years collectively, which is roughly equivalent to 343 "lost" faculty careers (based on average career length for those hired pre-uncapping). 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 is 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; Ehrenberg and Rizzo, 2001; Holden and Hansen, 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; Issacharoff and Harris, 1997; Ehrenberg and Rizzo, 2001; Clark and Ghent, 2008). 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. 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.

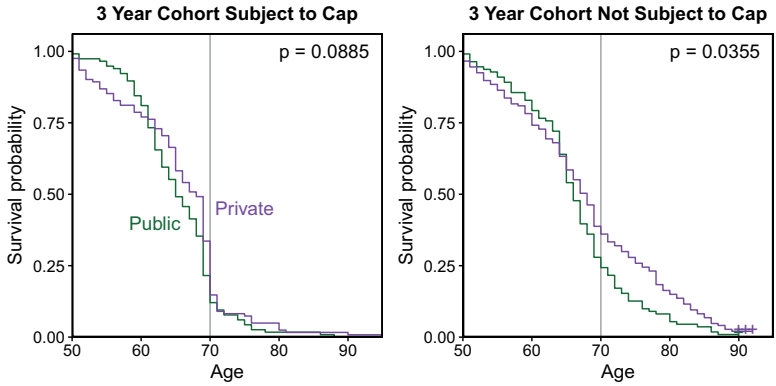

Figure 5. Survival Curves by School Type.
Notes: Kaplan–Meier survival curves for three year cohorts subject to cap (left) and not subject to cap (right) by public school (light gray) and private school (dark gray). While most faculty retire by 70 pre-1994, the survival curve shifts more substantially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

Third, DB plans at public institutions may also spur earlier retirements because they provided greater pension wealth than DC plans at private universities.[7] 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).[8]

We hence expect that fewer faculty would continue to work past age 70 at public law schools. Figure 5 presents Kaplan–Meier survival curves comparing three year cohorts subject to and not subject to the cap at public (green/light gray) and private (purple/dark gray) 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

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

8. 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.

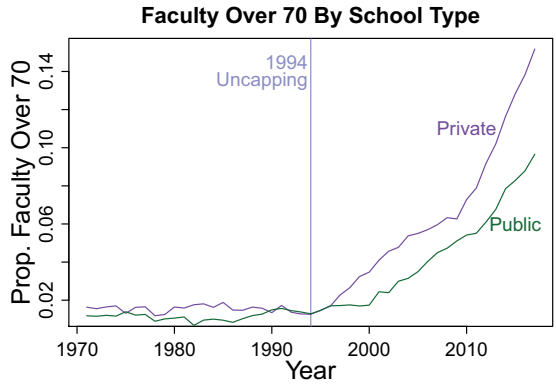

Figure 6. Faculty over 70 by School Type.
Notes: Average proportion of faculty over the age of 70 at private (dark gray) and public (light gray) law schools. The vertical line indicates the year mandatory retirement was uncapped (1994).

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 may exist between public and private schools. For instance, there may be differences in salary scales, teaching loads, and housing benefits between private and public schools that may affect retirement decisions differentially. If such benefits change contemporaneously around 1994, that would confound our inference.[9] 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*

9. Unfortunately, we are not aware of comprehensive sources of information about salaries, teaching loads, and housing benefits across our full sample of schools.

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 for 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 (light gray) and minority faculty in red (dark gray). The right column of panels displays the age distribution by gender, with male faculty in green (light gray) and female faculty in yellow (dark gray). 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.

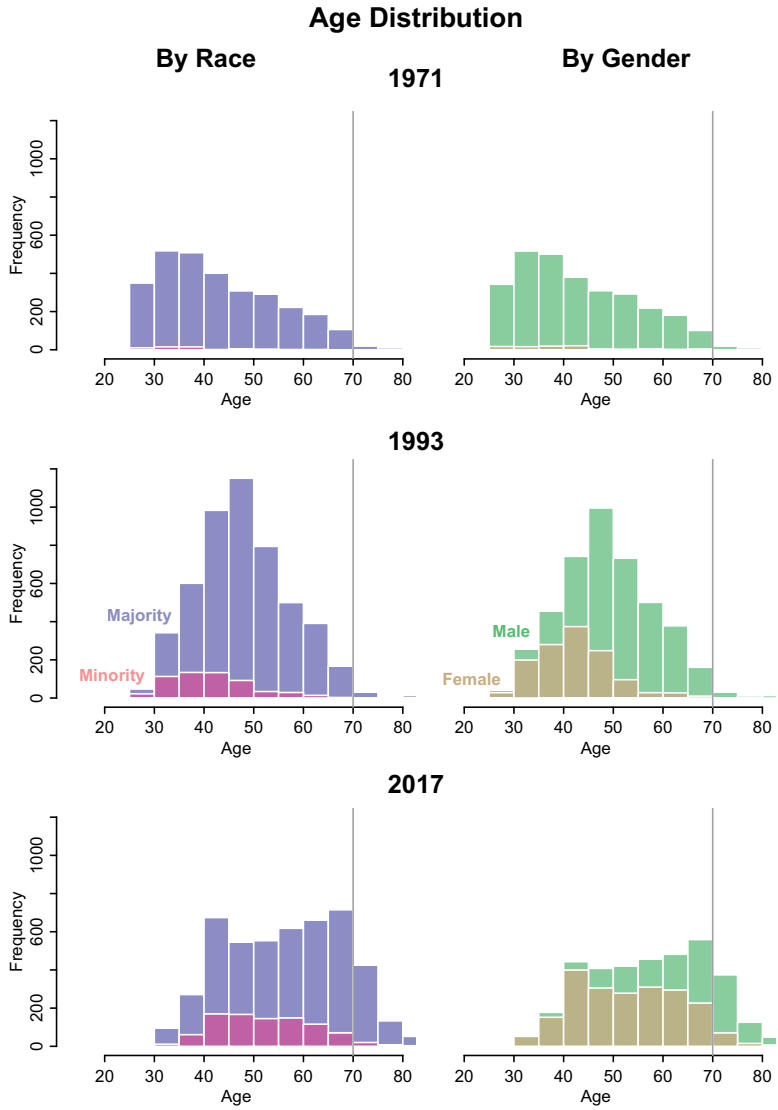

Figure 7. Age Distribution of Faculty by Race and Gender in 1971, 1993, and 2017. *Notes*: The left panel shows overlayed histograms of minority and majority faculty members in each age range, while the right panel shows overlayed histograms of female and male faculty members in each age range.

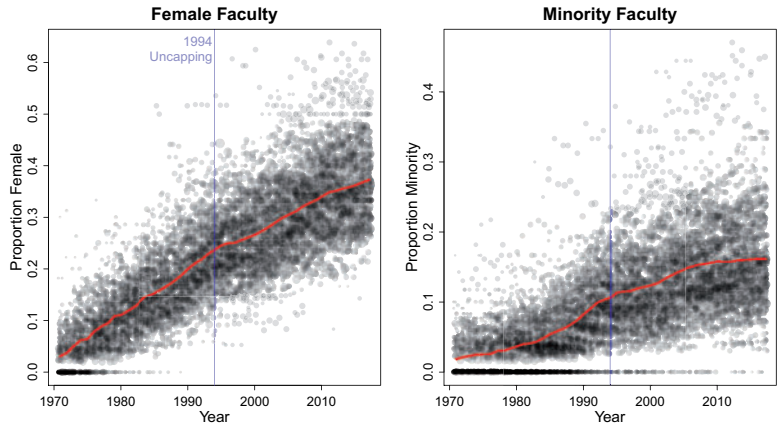

Figure 8. Female and Minority Faculty.
Notes: Proportion of faculty that is female (left panel) and minority (right panel) over time. Each dot represents a school, weighted by faculty size, and black lines plot the average proportions across all schools for each year.

The middle panel of Figure 7 shows that at the time of uncapping, faculty turning 70 within 5 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 40% of women faculty were under 40 compared to 17% of men.

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 to 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 (black) 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.[10] 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.[11] 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.

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. Figure 9A–C 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 by rank of school. The panels show that top 10 schools (purple/dark gray) appeared to be more affected by uncapping, particularly

10. Linear fits of the pre-1994 time trends generate slopes of 0.009 and 0.004 for female and minority proportions respectively. Post-1994, these slopes decrease to 0.006 and 0.002. The change in the rate of diversification is statistically significant for both female and minority proportions (with p-values < 0.01 each).

11. See Supplementary Appendix Figure A3.

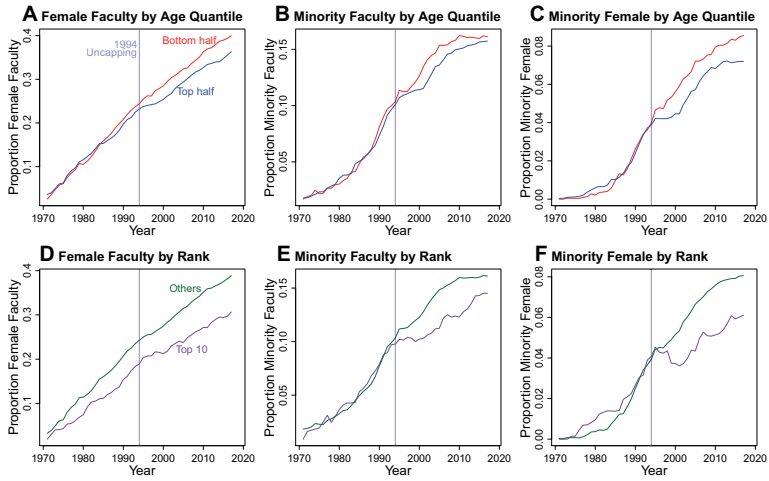

Figure 9. Diversification by Age Quantile and Rank.
Notes: Proportions of minority, female, and minority female by age quantile (top row) and by rank (bottom row). In the top row, the dark gray (light gray) line represents schools most (least) affected by uncapping. In the bottom row, the dark gray (light gray) line represents top 10 (all other) law schools.

for minority hiring.[12] 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, region, and general size) and year fixed effects to account for (school-invariant)

12. 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.

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.[13] 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.[14] 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 Figure 9F and Table 1 row (1) column (D), we also find suggestive evidence that uncapping may have

13. The exogeneity assumption could be violated if retirement decisions anticipated changes in hiring strategies. For instance, if a new Dean announced a new hiring initiative around race and diversity to begin at time t, that hiring strategy might itself affect $T_{(t-1)}$.

14. This parameter relaxes the assumption of a conventional Poisson model that mean equals variance.

Table 1. Regression Results

		(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 and 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)
	$N = 4,111$				

Notes: 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.

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 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.

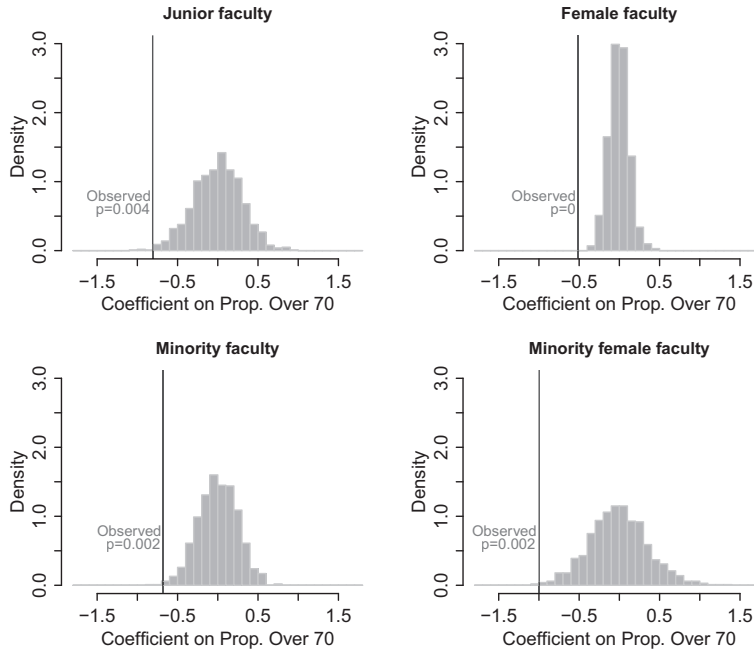

Figure 10. Permutation Test.
Notes: Permutation distribution of coefficients on proportion of faculty over 70 on junior (top left), female (top right), minority (bottom left), and minority female (bottom right) faculty.

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 retirement. 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 noncontinental 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 retirement reduced entry-level hiring in other schools, it may collaterally have assisted HBCUs and noncontinental 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 noncontinental 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 (Supplementary Appendix E). These results suggest that uncapping, if anything, also affected clinical hiring.

Last, we present a wide range of additional robustness checks in Supplementary Appendix G. 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.[15] 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 intraschool correlation. For each alternative, Figure 11 displays the number of "additional faculty" with 95% confidence intervals: 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

15. 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.

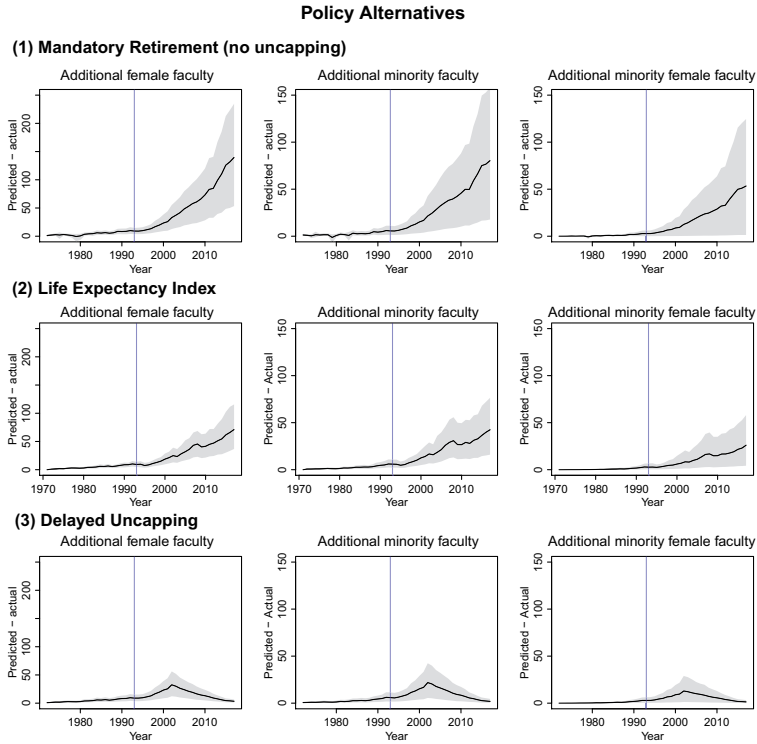

Figure 11. Policy Alternatives.
Notes: Difference in the number of female (left), minority (middle), and minority female (right) faculty predicted under three alternative policy scenarios and observed under the actual uncapping which took effect in 1994. The three scenarios include continuing mandatory 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.

in 2017, we would predict 140 more female professors and 80 more minority professors, including 53 minority female professors, which represent increases of 6.5%, 8.6%, and 12%, respectively, over 2017 levels.[16]

16. We find similar results using a simpler simulation where we replace faculty that would have been forced to retire with faculty that represent the demographics of entering cohorts.

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 (increases of 3.3%, 4.5%, and 5.8%, respectively, over 2017 levels). The jagged gains reflect the fact that the life expectancy adjustment is done on a yearly basis.

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.[17] The direction of such general equilibrium effects in our context remains 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.

17. For more general modeling efforts along these lines, see Blau and Weinberg (2017), Ghaffarzadegan and Xu (2018), and Larson and Gomez Diaz (2012).

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.[18] The relationship between uncapping and diversification stems from three conditions that have been separately documented in other academic fields, most notably in the sciences: (a) delayed retirement of incumbent faculty, (b) increasing diversity of the entry-level hiring pool, and (c) billet and budget constraints on faculty size. The aging of STEM faculty has been widely documented (Kaiser, 2008; Blau and Weinberg, 2017), 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 and 11 percentage points, and the share from under-represented 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.

18. 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.

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.

Fifth, 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 our study only points to one structural source. Nor would uncapping have the same magnitude of effects if it had been adopted in present day, when there are substantially more women and minority faculty members who would be the near-term beneficiaries.

Last, we have not examined the evidence of uncapping on law school quality. Measuring quality—based on research productivity, scholarly impact, or pedagogical impact—presents significant challenges (Brooks, 2005). This is all the more so because women and minority faculty can report disproportionately spending time on teaching and service (see, e.g., Guarino and Borden, 2017; Jimenez et al., 2019). We hence leave such assessments for future work.

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 (Schuster and Miller, 1984; Rutherglen, 1995; 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).[19] 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 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

19. 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.

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.

Supplementary material

Supplementary material is available at *American Law and Economics Review* Journal online.

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