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Estimating the Returns to Occupational Licensing: Evidence from Regression Discontinuities at the Bar Exam

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Abstract

We estimate the earnings returns to occupational licensing by exploiting regression discontinuities at the Italian bar exam. Law graduates who became lawyers after barely passing the exam earned 20,000 euros (50%) more per year than they would have in alternative occupations. This premium is not driven by monopoly rents from entry barriers. Instead, it reflects a compensating differential for the greater earnings risk inherent in the legal profession relative to the outside options available to law graduates.

JEL classification

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Keywords

labor market regulation, occupational licensing, field of study, lawyers

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1 Introduction

Occupational licensing requires workers to obtain a permit before they can practice, typically by passing an exam after meeting specific educational and training requirements. Licensing is widespread and growing in both the US and Europe. Whereas less than 5% of US workers held licenses in the early 1950s, this share had risen to 29% by 2008 (Kleiner and Krueger, 2013). In the EU, licensing currently covers about 22% of workers (Koumenta and Pagliero, 2019).[1] Although professional associations argue that licensing primarily serves the public interest, economists have long recognized two competing effects: licensing can mitigate asymmetric information (Akerlof, 1970, Leland, 1979), but it can also raise prices by restricting competition (Stigler, 1971).

Estimating the causal effect of licensing on earnings faces two challenges. First, workers self-select into licensed occupations and are then screened by an entry test, so observed earnings differences between licensed and unlicensed workers may reflect selection bias rather than a licensing effect. Second, exploiting cross-state variation in licensing requirements risks capturing general equilibrium effects, since licensing policies alter the supply of licensed workers. Identifying a credible source of exogenous variation in licensing status has thus remained an open question since Friedman and Kuznets (1945).

We address this challenge using a fuzzy regression discontinuity (RD) design that exploits a sharp threshold in access to a specific licensed occupation: the legal profession. In Italy, law school graduates must pass the bar exam to practice law. Using newly digitized records from bar exams held in Turin (Italy's fourth-largest city), we compare law graduates who scored just above and just below the passing threshold on their first attempt. Personal identification numbers allow us to link exam records to administrative labor market data, enabling us to track earnings over two decades into individuals' prime working years.

Although there is no limit on the number of times one can take the bar exam in Italy, failing on the first attempt reduces the probability of ever obtaining a license by 20 percentage points.[2] We find that law graduates who become lawyers after passing the bar on their first attempt earn

[1] Mocetti et al. (2021) find that in Italy, the setting of our study, licensing covers 24% of workers and 53% of college-educated workers.

[2] Jepsen et al. (2016) show that in situations like ours, where program participation is determined by a score on an exam that can be retaken multiple times, the score from the first exam should be used to avoid bias originating from individuals' decisions to retake the exam.

50% more than otherwise equivalent graduates who did not enter the profession after failing. This premium remains stable over the 23 years following the first attempt. At a 5% annual discount rate, the present discounted value of the earnings gain over a 23-year career amounts to 248,479 euros—indicating substantial returns to entering the legal profession relative to the alternatives available to law graduates.

This average premium masks substantial heterogeneity across the earnings distribution. Below the median, licensed and unlicensed law graduates earn similar amounts; above the 70th percentile, lawyers earn considerably more. An instrumental variable (IV) quantile regression confirms that the average 50% return is driven entirely by the upper tail.

Lawyers also face considerably greater income volatility than non-lawyers. High earnings in law are far from guaranteed: only those at the top of the distribution earn significantly more than their unlicensed peers, while many earn no more than law graduates in other occupations. The earnings premium may therefore reflect a compensating differential for this greater income risk. We corroborate this argument by showing that the standard deviation of lawyers' earnings is twice that of law graduates in alternative careers, both across individuals and over time within the same individual, a pattern confirmed by our RD estimates. We provide further evidence by computing the coefficient of relative risk aversion that would make a law graduate indifferent between the high-return, high-variance legal career and the safer outside option. Assuming a constant relative risk aversion (CRRA) utility function, we numerically solve for the coefficient that equalizes expected utility across the two career paths. The implied coefficient is 1.7, well within the range reported in the literature (Guiso et al., 2005). This result suggests that income risk is a plausible explanation for the observed earnings premium. It also explains why not all law graduates persist in retaking the bar until they pass: the potential for higher earnings comes bundled with substantial uncertainty.

These results are difficult to reconcile with monopoly rents, which would raise earnings across the board. Although the earnings volatility evidence and the implied risk aversion both point toward a compensating differential, one cannot rule out monopoly rents without confronting them directly. If entry barriers allow lawyers to extract rents from clients, these rents—not risk—could be the source of the premium. Three pieces of evidence argue against this interpretation: aggregate lawyers' revenues do not decline as the number of lawyers in a local market increases, suggesting that competition does not erode markups; Italy has an exceptionally high number of lawyers relative to comparable European countries, pointing to weak entry barriers; and the bar exam, although selective, does not pose an insurmountable barrier, since candidates can retake it indefinitely and most who fail initially go on to pass. Together, these facts suggest that the earnings premium stems from factors other than monopoly rents.

Finally, we examine two additional mechanisms: the sheepskin effect of holding the license and differences in working hours.

A subtle distinction exists between holding a license and actively practicing law. The license is a prerequisite for legal practice, yet some law graduates who pass the bar never become practicing lawyers. For these individuals, the license could serve as a signal of quality—a *sheepskin effect*.[3] To test this, we compare earnings among law graduates not practicing law, with and without the license. We find no premium in this subgroup, indicating that the earnings advantage arises entirely from practicing law rather than from holding the credential.

Lawyers may also earn more simply because they work longer hours. According to the Italian Labor Force Survey (LFS), lawyers work 13% more hours than law graduates in other occupations. Although this partly accounts for the premium, it falls far short of explaining the entire gap.

These patterns are hard to reconcile with monopoly rents, working hours, or sheepskin effects, and instead suggest that the premium is a compensating differential for the greater income risk inherent in the legal profession. Our results speak against the common belief that licensing necessarily generates monopoly rents, and imply that removing the bar exam would not eliminate the earnings gap between lawyers and non-lawyers.

Our study contributes to the extensive literature on occupational licensing and its effects on earnings.[4] Previous studies have documented a licensing wage premium using cross-sectional regressions (Ingram, 2019, Kleiner and Krueger, 2010, 2013, Kleiner and Vorotnikov, 2017, Koumenta and Pagliero, 2019) or worker fixed-effects designs (Gittleman et al., 2018, Gittleman

[3] The sheepskin effect refers to the phenomenon where possessing a formal credential yields higher wages than having an equivalent level of education without the credential.

[4] There is an extensive literature on occupational licensing and its effects on various outcomes. Previous studies have examined the impact of licensing on prices and wages (Kawaguchi et al., 2014, Kleiner and Kudrle, 2000, Kleiner et al., 2016, Pagliero, 2011, Thornton and Timmons, 2013), labor supply (Blair and Chung, 2019), statistical discrimination (Blair and Chung, 2022), mobility (Cassidy and Dacass, 2021, Johnson and Kleiner, 2020), firm location and employment (Plemmons, 2022), labor shortages (Blair and Fisher, 2022), the quality of services offered by licensed professionals (Anderson et al., 2020, Bhai and Mitchell, 2022, Bowblis and Smith, 2021, Farronato et al., 2024), and the welfare consequences of occupational licensing for workers and consumers (Kleiner and Soltas, 2023).

and Kleiner, 2016, Zhang, 2019). Relative to these approaches, our design ensures that individuals are comparable in all dimensions—including human capital—except licensing status. Other work has exploited cross-state variation in licensing requirements within a difference-in-differences framework (Kleiner and Soltas, 2023, Pizzola and Tabarrok, 2017, Timmons and Thornton, 2019). Blanchard et al. (2026) use a Uruguayan reform that licensed already-employed students without additional training to isolate licensing rents from human capital, finding that incumbent accountants' earnings fell about 9%. These studies estimate a general equilibrium effect: what happens to the equilibrium wage when an entire occupation becomes licensed or unlicensed. By contrast, our results capture a partial equilibrium effect: what happens to an individual's earnings when they obtain a license, holding all else equal. Ours is the first estimate of the returns to licensing that holds human capital constant and isolates the pure licensing effect from the impact of regulatory differences on prices.[5]

Understanding this partial equilibrium effect matters for at least two reasons. First, entry barriers—such as the bar exam—may create rents for incumbents by limiting competition. In that case, the earnings premium represents a cost borne by consumers for regulating entry. Quantifying this cost requires a partial equilibrium estimate that isolates the earnings difference between licensed professionals and otherwise equivalent individuals in unregulated occupations. The general equilibrium effect—the main focus of the existing literature—confounds this with additional channels, such as changes in workforce composition and service quality. Our findings indicate that, while licensing generates a substantial earnings premium, the premium does not stem from reduced competition but instead compensates for the greater earnings risk faced by lawyers.

Second, partial equilibrium estimates can inform policies aimed at reducing wage inequality by quantifying how much occupational sorting contributes to pay disparities. For example, if a policy could alter the sorting of women out of the legal profession, how much would it narrow

[5]The most recent estimate by Kleiner and Soltas (2023) shows that shifting an occupation in a state from entirely unlicensed to entirely licensed increases state average wages in the licensed occupation by 15%. Part of this wage increase stems from the negative labor supply shock induced by licensing an occupation. In contrast, in our setting, there is no effect on equilibrium wages, as we compare individuals with and without a license while the supply of licensed workers remains constant. This distinction explains the difference between our estimates and those found in the literature. One limitation of the existing licensing literature, explicitly acknowledged by Kleiner and Soltas (2023), is that constructing a counterfactual of non-licensed workers for many high-skilled occupations—such as lawyers and physicians—is not possible because these occupations are licensed everywhere in the U.S. Our approach addresses this limitation, and our results are informative specifically about high-skilled occupations.

the gender pay gap? Answering this question requires knowing the causal return to entering the occupation, net of selection.[6]

Our paper also closely relates to the literature on the returns to different fields of study. Leveraging lottery-based admissions to medical and dental schools, Ketel et al. (2016) and Ketel et al. (2019) estimate the returns to studying medicine and dentistry in the Netherlands. Similarly, Grosz (2020) estimates the labor market returns to a nursing degree using comparable lotteries in the U.S. Other studies, such as Kirkeboen et al. (2016), Daly et al. (2022), Dahl et al. (2020), and Hastings et al. (2013), exploit discontinuities in centralized admission systems in secondary schools and universities in Denmark, Sweden, Norway, and Chile to assess the returns to various fields of study. In a similar spirit, Bleemer and Mehta (2022) analyzes GPA cutoffs for majoring in Economics at a U.S. college to estimate the wage returns to studying economics.

The empirical strategies and estimated effects in these papers are similar to ours, but a key difference emerges. Earnings differences across fields of study reflect two bundled effects: (1) human capital accumulated during the study program and (2) access to occupations that require a specific degree. The existing literature identifies only their combined effect. Our setting isolates the second component—the returns to occupation, holding human capital fixed—because all individuals in our sample completed the same degree (law) and the same apprenticeship required to sit for the bar.

Several studies have examined occupational licensing in the Italian legal profession. Bamieh and Cintolesi (2021) show that familism at the bar exam accounts for a large share of the intergenerational transmission of the profession in certain Southern Italian courts. Pellizzari et al. (2011) find that liberalizing price floors and other anti-competitive regulations weakens selection among lawyers by encouraging exits from the upper part of the quality distribution. Basso et al. (2021) show that having relatives in the profession raises the probability of passing the entry exam and boosts subsequent earnings, especially for those who performed poorly in law school.

[6]Similarly, Jardina et al. (2023) find that racial occupational segregation—the unequal distribution of racial groups across occupations—is a key factor explaining wage inequality between Black and White workers.

2 Institutional background

With 19% of its workforce in a licensed profession, Italy sits at the median in the EU distribution. Germany has the highest share at 33%, while Denmark has the lowest at 14%.[7] This makes Italy an ideal setting for studying the returns to professional licensing.

Becoming a lawyer in Italy requires meeting specific legal requirements. First, candidates must complete a five-year law degree.[8] Second, law graduates must complete a two-year apprenticeship.[9] Only after completing this apprenticeship are they eligible to take the bar exam, which consists of two stages: a written part, followed by an oral part for those who pass the first stage. Each stage has a minimum grade threshold, and candidates obtain a license to practice law only after passing both.

The bar exam is held simultaneously across Italy's 26 courts of appeal, and candidates must take it at the court where they completed their apprenticeship. The written test consists of three essay questions, administered over three consecutive days. These questions, identical across all jurisdictions, are prepared in advance by the Ministry of Justice, but each district has its own grading commission. Candidates must write two legal briefs—one on civil law (contracts and torts) and one on criminal law—as well as a court brief on a subject of their choice from civil, criminal, or administrative law. Written tests are graded anonymously, and successful candidates proceed to the oral test, conducted before a five-member panel. The panel consists of one judge, three lawyers, and one law professor, each asking one question across six fields of law. Compared to the written test, which has a pass rate of only 40%, the oral test has an 86% pass rate.[10]

In the written test, each brief is graded on a scale of up to 50 points and carries equal weight. Candidates must score at least 90 points to advance to the oral test. Those who score below this threshold must wait until the following year to retake the exam, while those who pass have a high likelihood of becoming lawyers, given the oral test's high pass rate. In the oral test, each question is graded on a scale of up to 10 points and is equally weighted. To pass, candidates must score at least 180 points; those who fail must retake the written test the following year.

[7] Koumenta and Pagliero (2019).

[8] Law schools also offer shorter three-year programs, but a five-year degree is a prerequisite for becoming a lawyer.

[9] The terms *apprenticeship*, *internship*, and *traineeship* are sometimes used interchangeably. We follow the European Commission and the European Center for the Development of Vocational Training (CEDEFOP) in using the term *apprenticeship*.

[10] For descriptive statistics on pass rates across jurisdictions in the late 1990s and early 2000s, see Table 2 in Buonanno and Pagliero (2018).

Since nearly a year separates the written and oral tests, the entire process takes over a year to complete. For example, candidates who passed the written test in 1999 were only admitted as licensed lawyers to the Italian Bar Association at the end of 2000.

Candidates who fail the bar on their first attempt may choose to retake it the following year, with no limit on the number of attempts. Alternatively, they may forgo retaking the exam and pursue open competitions to become judges or notaries, both of which also require a law degree. Others may opt for careers in sectors where legal training—though not mandatory—is valuable, such as public administration or finance. Given these outside options, extending the apprenticeship while waiting to retake the bar is unattractive, as apprentices earn low wages, making entry into a different occupation a more lucrative choice.

3 Data

We use newly collected data from the National Archives of Turin, Italy.[11] The archive preserves records of bar exams held in Turin before 2001, including each candidate's social security number, written test scores, and—for those passing the written test—oral test scores. We digitized five complete exam sessions (1996–2000) and partially digitized the 1994 and 1995 sessions, collecting only the data necessary to identify first-time exam takers in the 1996–2000 cohorts.

The main digitization challenge concerns social security numbers. For the 1994 and 1995 sessions, we recorded only names and grades; for 1996–2000, we also recorded social security numbers. We identify repeaters by cross-referencing names across sessions. Two potential concerns arise. First, we may misclassify a candidate as a first-timer if they skipped sessions (e.g., took the exam in 1993 and again in 1996). However, skipping is extremely rare—occurring in less than 1% of cases—because candidates either abandon the exam or retry in consecutive years, and the apprenticeship credential expires after five years. Second, namesakes could introduce errors, but the share of individuals with identical names who are distinct persons is negligible (0.09%).

In our sample, 86% of individuals who pass the written test also pass the oral test, while the written test itself has a pass rate of 40%. Since the written test serves as the true barrier to obtaining a license to practice law, we focus exclusively on this stage. To preserve the validity

[11] All documents used in this study can be requested at the Archivio di Stato di Torino, https://archiviodistatotorino.beniculturali.it/.

of our RD design, we follow Jepsen et al. (2016) and consider only scores from the first attempt at the bar. Jepsen et al. (2016) show that in settings like ours, where program participation is determined by a score on an exam that can be retaken multiple times, the score from the first exam should be used to avoid bias originating from individuals' decisions to retake the exam.[12] By focusing on the written test, the first stage of the bar exam, and restricting the analysis to first-time candidates, we eliminate any selection bias induced by prior written test outcomes.[13] Thus, our final sample includes all law school graduates who took the bar for the first time between 1996 and 2000. For these individuals, the running variable in our RD design is the written test score from their first bar exam, regardless of whether they retook the exam in later years.

The bar exam data were linked by the Italian Social Security Agency (INPS) to its administrative archives. These records contain earnings information of all Italian private and public sector workers, as well as self-employed individuals, including licensed professionals and entrepreneurs.[14] Combining bar exam data with INPS records allows us to track the earnings of all law school graduates for 23 years following their first attempt at the bar exam.

We separately linked candidates who took the bar exam between 1996 and 2000 to the archives of the University of Turin's law school. During this period, 66% of bar exam takers graduated from this university. While not essential to our analysis, these data allow us to validate our identification strategy by showing that important pre-determined variables, such as graduation grade and age at graduation, are balanced at the bar exam's passing grade cutoff. For the full sample of bar exam takers, including those who did not graduate from the University of Turin, we can still provide a balancing test using gender, place of birth, and age at their first bar exam.

Finally, we use two methods to determine whether an individual holds a license to practice law. First, we identify those who passed both the written and oral tests of the bar exam between 1996 and 2000. Second, we check the registry of the Italian Bar Association, known as

[12]In our setting, this bias could originate from individuals who fail with low scores and choose not to retake the exam. The narrower distribution of those trying again, combined with first-time candidates from subsequent years, can introduce a discontinuity in the predetermined characteristics of the candidates' distribution, invalidating the continuity assumption of the RD design.

[13]Since individuals in the Netherlands can participate in the medical school admission lottery an unlimited number of times, Ketel et al. (2016) adopt our same approach and restrict their analysis to first-time lottery applicants.

[14]This data can be accessed through the VisitINPS Scholars Program, https://www.inps.it/dati-ricerche-e-bilanci/attivita-di-ricerca/programma-visitinps-scholars.

the *Consiglio Nazionale Forense* (CNF), for any remaining individuals. After passing the bar exam, all law graduates who wish to practice law must register with the CNF. Therefore, anyone listed in the CNF registry must have passed the bar exam at some point.[15]

Between 1994 and 2000, a total of 6,053 bar exams were taken by 3,508 distinct individuals. Using data from 1994 and 1995, we identify 2,174 first-time exam takers between 1996 and 2000. We then requested the Social Security Agency to link these individuals to their archives, successfully matching 1,972 of them. Table A.1 outlines the steps in constructing our sample, from the initial pool of bar exam takers to the final estimation sample.

Table 1 compares the characteristics of law graduates with and without the license to practice law. Graduates who eventually pass the bar exam earn more, but they also have higher graduation grades and complete their degrees more quickly than those who never pass. Additionally, women are marginally less likely than men to obtain the license, and this difference is not statistically significant. Given these differences, a simple mean comparison of earnings between licensed and non-licensed law graduates would be biased. Instead, comparing marginal passers to marginal failers allows us to isolate the effect of licensing from other confounding factors, such as differences in underlying ability.

[15]CNF data is publicly available online: https://www.consiglionazionaleforense.it/

Table 1: Characteristics of law graduates with and without the license

	Non-licensed	**Licensed**	**p-value**
Age at bar exam	31	29	0.000
Share females	0.63	0.60	0.228
Share born in Turin	0.37	0.44	0.003
Connected family name	0.13	0.17	0.036
Observations	582	1,592	
Graduation grade	100	103	0.000
Age at graduation	27	26	0.000
Observations	321	1,108	
Earning (mean over 23 years after bar exam)	30,799	40,034	0.000
Observations	453	1,519	

Note: This table compares law graduates who eventually pass the bar exam, obtaining the license to practice law, to those who do not. Means of some important variables and p-values of t-tests are reported. The sample size for the two variables "graduation grade" and "age at graduation" is smaller than for the remaining variables because only 66% of individuals in our sample graduated from the law school at the University of Turin. The sample sizes when considering earnings is smaller than the original sample size because 200 individuals could not be linked to social security (INPS) archives.

4 Empirical design

We aim to measure the effect of professional licensing on earnings over the entire career of a worker. A longstanding challenge in the occupational licensing literature is that a comparison between licensed and non-licensed workers is confounded by unobserved factors such as ability, preferences, opportunities, and background. To address this issue, we exploit a fuzzy regression discontinuity design based on the minimum score required to pass the bar exam. Candidates who score below 90 points on the written test are not admitted to the second and final stage (the oral test) and must wait one year before retaking the exam.

We are interested in estimating the lifetime returns to the legal profession for law graduates. This can be captured by the following regression model:

$$Y_{it} = \beta_t L_i + X_i' \theta_t + \varepsilon_{it}, \tag{1}$$

where Y_{it} denotes the labor market outcome of interest—such as earnings—measured t years after the year in which individual i first took the bar exam (for example, $t = 1$ refers to 2001 for individuals who took the exam in 2000, and to 2000 for those who took it in 1999); L_i is an indicator equal to 1 if the individual obtains a license to practice law at any point in time—not necessarily on their first attempt; the vector X_i includes calendar year fixed-effects, gender, and the year of birth; and ε_{it} is the error term. The coefficients of interest are β_t, which capture the returns to the legal profession t years after the first attempt at the bar exam. Equation (1) is estimated separately for each year t following the exam. In addition, we estimate the same equation using average annual earnings, Y_i, over the 23 years following the first bar exam attempt.

If high-ability individuals are more likely to pass the bar exam, the OLS estimator of β_t from equation (1) will be biased. We solve this endogeneity problem by exploiting the sharp discontinuity in the Italian bar exam: individuals scoring below 90 points on the written test are not admitted to the second, and final, phase of the bar exam—the oral test. Passing the oral test is a necessary condition for obtaining a license to practice law. We use the written test result as an instrument for L_i in equation (1). More specifically, following Imbens and Lemieux (2008), Lee and Lemieux (2010), we estimate a first-stage equation of the form,

$$L_i = \gamma P_i + X_i'\delta + f(g_i) + \nu_i, \tag{2}$$

restricting the analysis to individuals who took the bar for the first time between 1996 and 2000 and use the test results from their first attempt. P_i is a dummy variable equal to 1 if the individual scored 90 points or more on the written test; $f(g_i)$ is a polynomial in the written test score used to define P_i; and ν_i is the error term.

The parameter γ captures compliance, defined as the difference in the probability of obtaining a license to practice law between those who pass and those who fail the bar on their first attempt.[16] Compliance may be imperfect because exam failers can retake the exam, which is administered once a year, and because a small share of candidates (14%) fails the oral test.

This setting yields a fuzzy regression discontinuity design, where the second-stage is given by

$$Y_{it} = \beta_t \hat{L}_i + X_i'\theta_t + f(g_i) + \varepsilon_{it}, \tag{3}$$

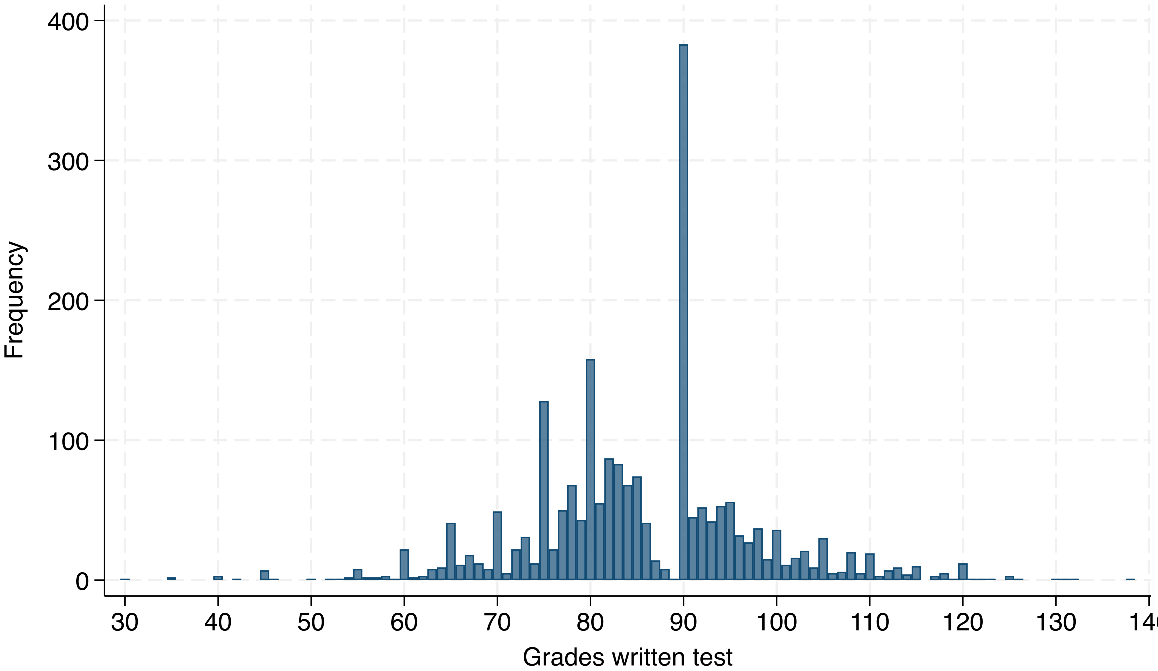
[16]Following the LATE interpretation of instrumental variables, γ measures the share of compliers.

and $\hat{L}_i$ denotes the fitted values from the first-stage equation. This design allows us to examine how the earnings differential between lawyers and non-lawyers evolves over the first 23 years following their initial attempt at the bar. Rather than including all individuals who took the bar between 1996 and 2000, we focus on first-time exam takers. Besides the considerations outlined before to obtain unbiased estimates (Jepsen et al., 2016), this restriction yields a homogeneous group of individuals who have just completed their two-year apprenticeship. Focusing on first-time candidates facilitates the interpretation of our life-cycle estimates, as each year t after the first bar attempt corresponds to the same stage in the professional life of all individuals, regardless of the year they sat for the exam.

5 Testing the validity of the RD design

Figure 1 displays the distribution of written test scores. While there is a clear spike at the minimum passing grade (90), because this test is anonymous, we have strong reasons to believe that this spike reflects rounding by examiners—i.e., rounding up all scores that fall slightly below 90—rather than sorting by examiners, which would imply selectively awarding additional points to push only certain candidates above the threshold. The written test consists of three essay questions, and the nature of essay grading inherently allows for discretion. Examiners are likely to avoid assigning scores just below the passing mark, opting instead to round up. Consistent with this, Figure 1 shows that rounding generally occurs at multiples of 5, supporting the interpretation that the spike at 90 results from rounding rather than selective manipulation of test scores. Moreover, selective leniency is unlikely: to ensure fairness, the written test is anonymous, and examiners do not know the identity of candidates until all exams have been graded.

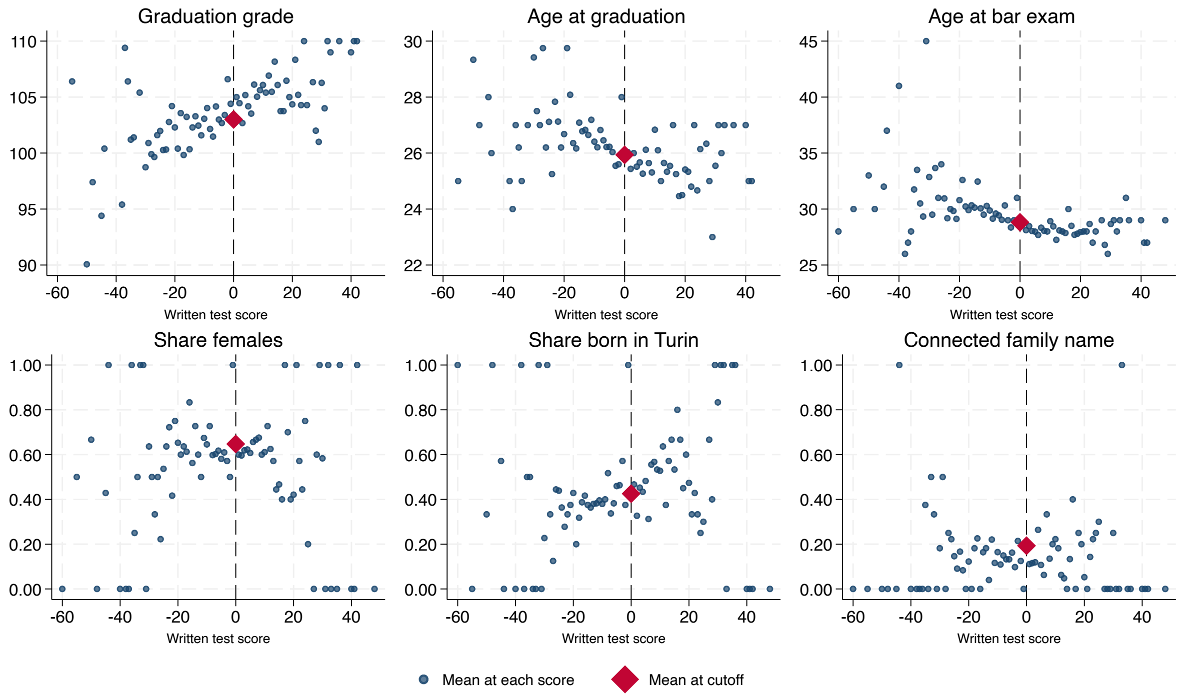
Figure 1: Grades distribution at the bar exam



Notes: This figure reports the distribution of written test scores from the bar exam in the years 1996–2000.

A distinctive feature of our setting is the large mass of candidates scoring exactly at the passing threshold of the written bar exam. This concentration raises the natural concern that candidates may be able to sort precisely at the cutoff, which would invalidate the RDD identifying assumption. To address this, we present our balancing tests in a way that isolates the mass point as a separate group. Specifically, Figure 2 plots the conditional mean of each pre-determined covariate at every integer value of the running variable, excluding individuals exactly at the cutoff, whose mean is displayed as a distinct marker. If sorting were driving the mass point, we would expect these individuals to differ systematically from those just above and just below the threshold—for instance, by being older, higher-achieving, or more likely to come from connected families. Instead, the figure shows that the cutoff mean falls squarely in line with the pattern traced by observations on either side, and the estimated discontinuities in pre-determined covariates reported in Table A.3 are small and statistically insignificant. This evidence suggests that the mass point reflects mechanical features of the grading process rather than strategic manipulation, and that the individuals at the threshold are comparable to those in its vicinity.

Figure 2: Balancing test around the passing threshold



This figure shows the relationship between predetermined characteristics and written test scores on candidates' first attempt at the bar exam. Each dot represents the mean of the corresponding variable for candidates with a given written test score. The red diamonds denote candidates at the passing threshold, that is, those generating the spike in Figure 1.

Furthermore, to confirm that the written test is anonymous, we constructed a measure indicating whether a candidate shares a surname with a lawyer already registered in Turin; we call these "connected" candidates. If family connections influenced test outcomes, connected candidates might benefit from relatives already in the profession who know members of the examining commission and could exert pressure to help them pass. However, the written test is anonymous precisely to prevent such unfair practices. Consistent with this, the bottom right panel of Figure 2 shows no correlation between written test scores and connected status, particularly around the passing threshold.[17]

Nonetheless, to dispel any concerns about the spike at the threshold in Figure 1, we conduct two more tests confirming that the observed bunching is not endogenous. First, following the literature (Canaan and Mouganie, 2018, Dahl et al., 2014, Zinovyeva and Tverdostup, 2021), we assess the robustness of our IV estimates by implementing a donut-RD design: we exclude all

[17] Bamieh and Cintolesi (2021) document that in some district courts in Italy, lawyers have used their influence to help family members pass the bar exam. However, their analysis also shows that the court in Turin is not affected by these practices.

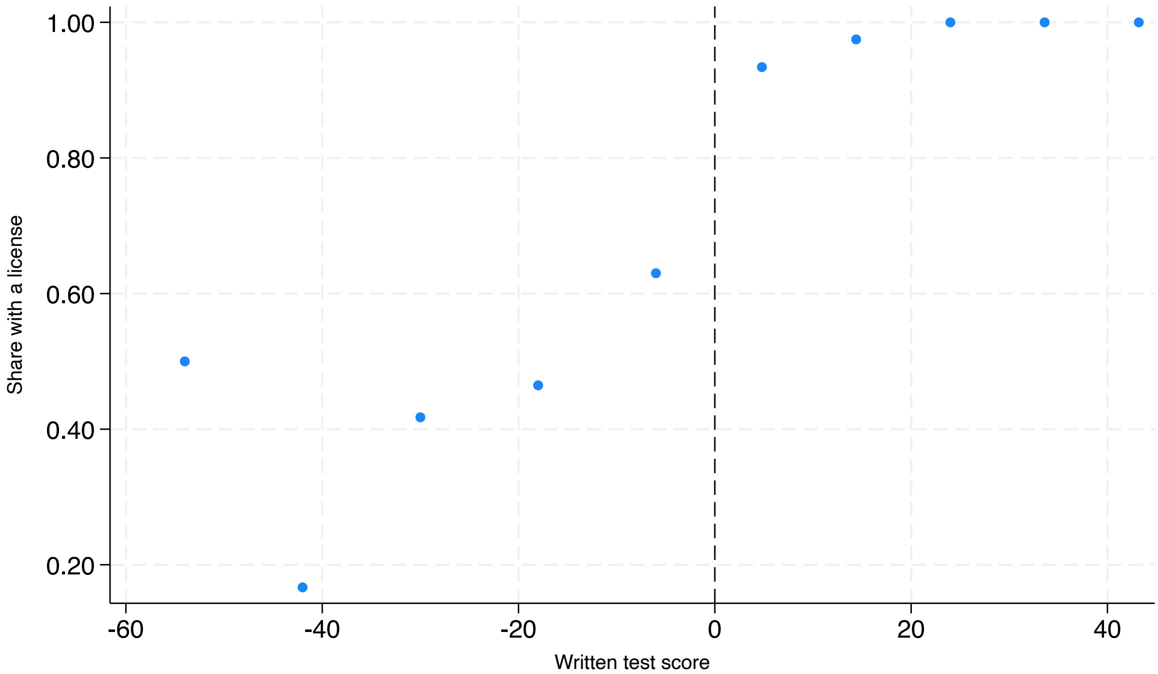
individuals who scored exactly 90 points, as well as those scoring just below the threshold; our point estimates remain stable under this specification. Second, we report the average earnings throughout the period considered (as well as at specific years) as a function of written test scores. One potential concern is that candidates who are not rounded up to 90 may differ systematically from those who are. However, Figure A.7 shows that this is not the case: earnings are neither unusually low just below the passing threshold for the marginal failers (those scoring between 80 and 90), nor unusually high at the threshold itself.

6 Returns to the legal profession

Figure 3 plots the relationship between the probability of ever obtaining a license to practice law and the written test score on the first attempt at the bar. Among first-time exam takers, nearly all individuals who pass the written test eventually obtain the license, while a substantial share of those who fail never do. Not all candidates who pass the written test obtain the license, as approximately 10% fail the oral test; conversely, about 60% of those who fail the written test on their first attempt succeed in later attempts.

Crucially, Figure 3 reveals a clear discontinuity in the probability of obtaining the license at the passing threshold: individuals who pass the written test on their first attempt are 20 percentage points (a 40% increase) more likely to obtain the license than those who fail. To corroborate this graphical evidence, the last row of Table 2 reports the first-stage estimates, corresponding to γ in equation (2). This estimate is robust to alternative bandwidth choices and polynomial specifications in the running variable.

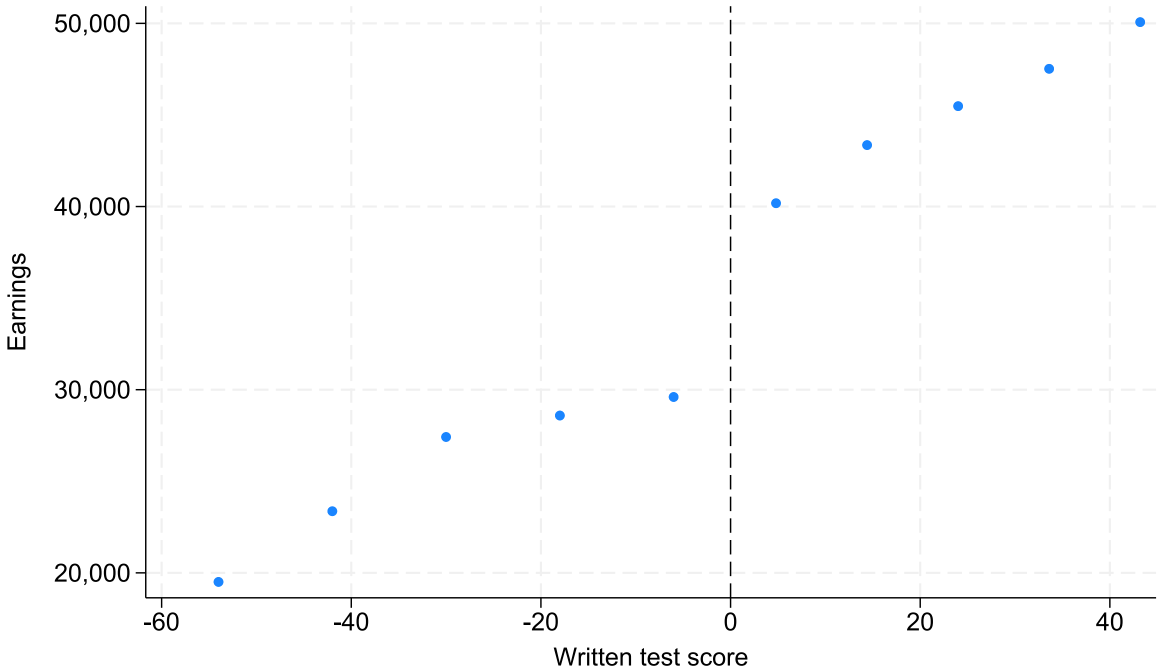
Figure 3: Passing the bar on the first attempt increases the likelihood of becoming a lawyer



Notes: The figure shows that passing the bar exam on the first attempt increases the probability of ever holding a license to practice law. Each dot represents the share of individuals holding a license within a grade interval from their first bar exam. Intervals are constructed to contain approximately equal numbers of observations.

As a counterpart to Figure 3, Figure 4 shows a discontinuity in earnings: individuals who barely pass the bar exam earn more than those who barely fail. This figure reports earnings measured nine years after the first bar exam attempt. Figure A.2 presents analogous results for different years.

Figure 4: Passing the bar on the first attempt increases earnings



Notes: The figure shows that passing the bar exam on the first attempt increases earnings (measured nine years after the exam). Each dot represents the average earnings of individuals within a grade interval from their first bar exam. Intervals are constructed to contain approximately equal numbers of observations.

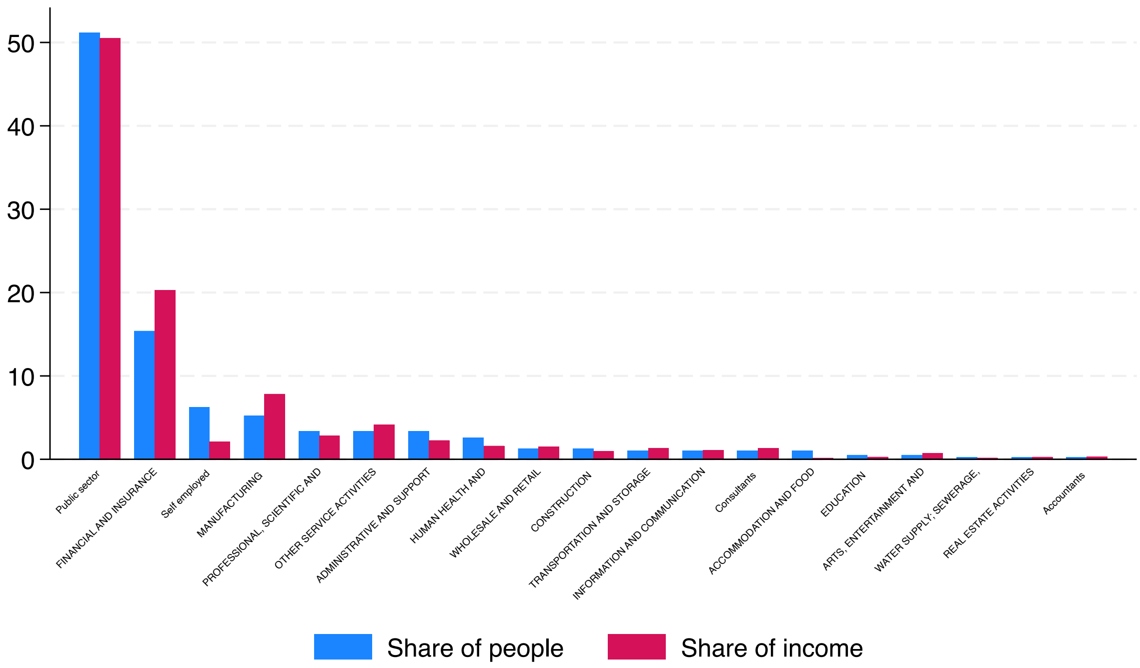
Our IV estimates should be interpreted as local average treatment effects (LATE, Imbens and Angrist (1994)). In our context, this captures the effect of holding a license to practice law for those who became lawyers because they (barely) passed the bar exam on their first attempt—the so-called *compliers*. According to our first-stage estimates, compliers represent approximately 20% of the sample. One might ask whether this group is systematically different from the broader population of bar exam takers.

To address this, we follow Abadie (2003) and Almond and Doyle Jr (2011) to compute the average characteristics of compliers. Overall, compliers appear quite similar to the full sample of bar exam takers. The main difference is that the compliers have slightly higher university GPAs (2.9% higher), while other characteristics are broadly comparable. Notably, the share of women is identical across groups, approximately 60%. Detailed results are reported in Table A.2.

Moreover, our estimates of the returns to working as a lawyer should be interpreted relative to the alternative occupations available to law graduates. This raises a natural question: what are law graduates who do not hold a license to practice law doing instead? Social security data allow us to observe all sources of earnings—not only income from legal practice, but also earnings from employment in the private or public sector, as well as from self-employment.

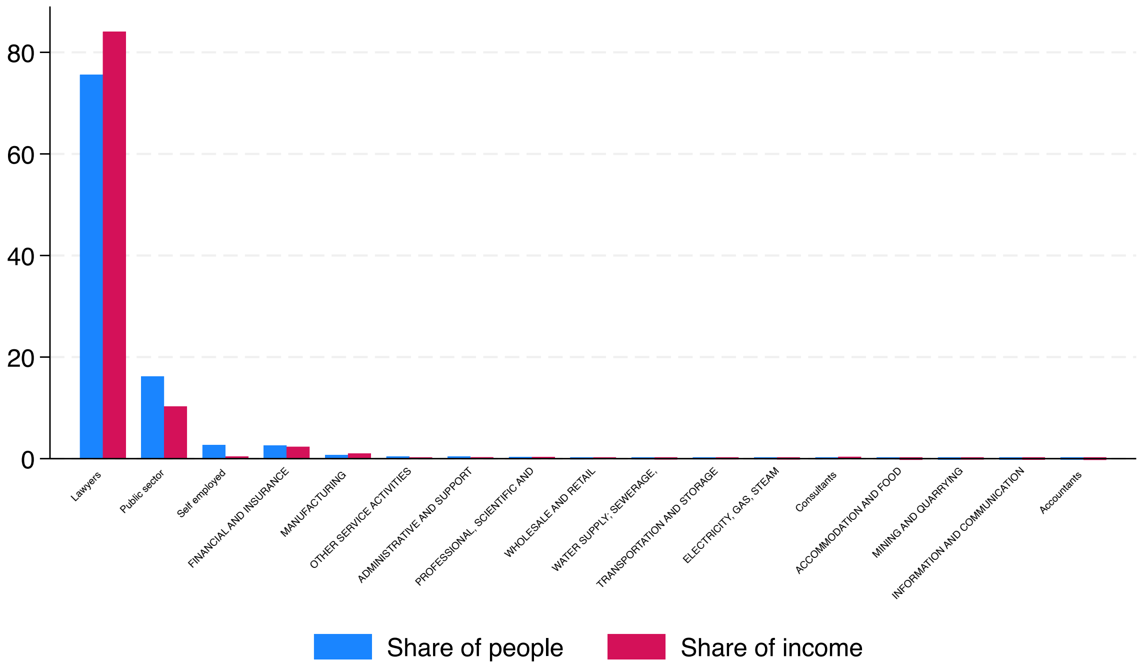
Figure 5 shows that nearly 50% of law graduates who never passed the bar exam are employed in the public sector. In contrast, Figure 6 shows that law graduates holding a license to practice law earn 80% of their income from practicing law. A small share of licensed lawyers earn part of their income in the public sector, consistent with the rule that practicing attorneys cannot simultaneously hold a regular job—the only exception being employment in the public sector.

Figure 5: Sectors where law graduates without the license are employed



Notes: This figure reports the share of income earned in different sectors among individuals who do not hold a license to practice law. It also reports the share of individuals earning any income in each of these sectors. All sectors follow the NACE Rev. 2 classification.

Figure 6: Sectors where law graduates with the license work

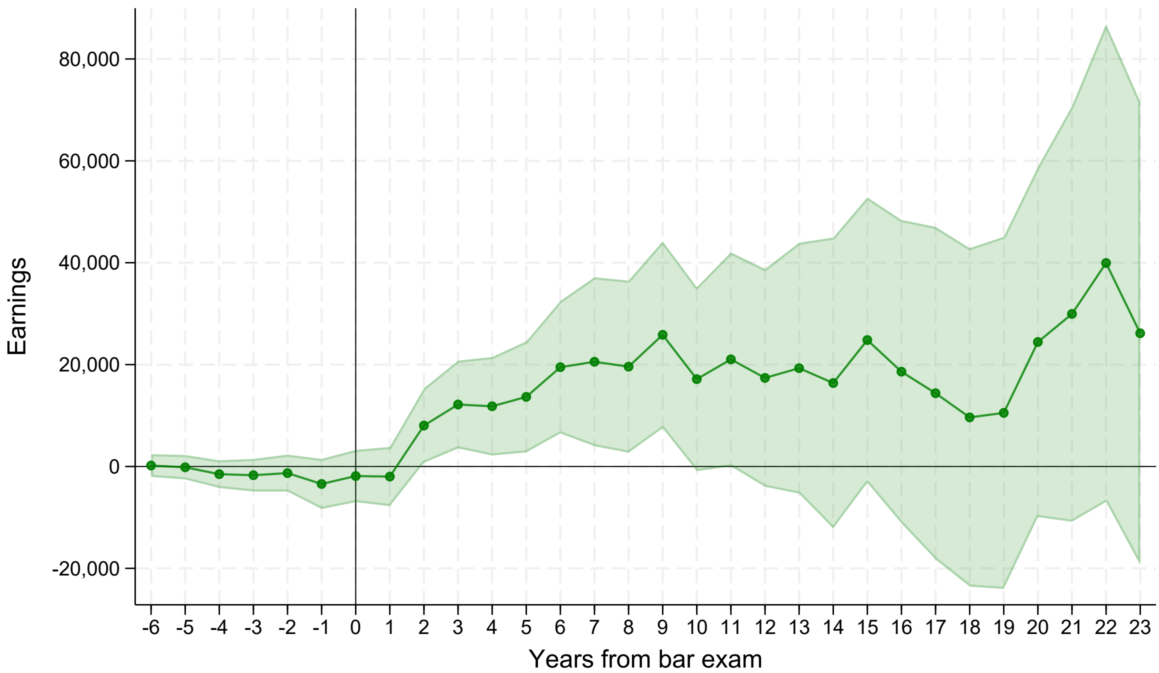


Notes: This figure reports the share of income earned in different sectors, including from practicing law, by individuals holding a license to practice law. It also reports the share of individuals earning any income in each of these sectors. The sector "Lawyers" refers to the registry of lawyers in our social security data, while all other sectors follow the NACE Rev. 2 classification.

Figure 7 reports the instrumental variable (IV) estimates β_t from equation (3) for each year t before and after the year of the first bar exam. Confirming the validity of our RD design, there are no differences between licensed and non-licensed law graduates before the first exam. Instead, we find significant returns to the legal profession from the beginning of the career, which gradually increase over the first six years after the bar exam, eventually stabilizing at around 20,000 euros per year. The sample size of 1,972 individuals remains nearly constant throughout the period considered, with minimal attrition—only 46 individuals by year 23, due to deaths and early retirements. The larger standard errors in the later years reflect the fact that the standard deviation of earnings increases over time, as shown in Figure A.3.[18]

[18]Figure A.4 reports the yearly intention-to-treat (ITT) estimates as a counterpart to the IV estimates reported in Figure 7. As expected, the two estimates are equivalent up to the rescaling due to the first-stage.

Figure 7: Returns to licensing—IV estimates earnings



Notes: The figure reports the differences in earnings, obtained from the IV estimates based on equation (1), between individuals with and without a license to practice law. The shaded area denote 95% confidence intervals.

The effect kicks in only two years after the written test because the grading process is lengthy, usually taking around six months. The oral test cannot begin until the results of the written test are available, as passing the written test is a prerequisite for admission to the oral test. The interview calendar for the oral test spans from September to December. Since the written test typically takes place in December (e.g., December 2000), it is not unusual for a candidate to obtain the license only one year later (e.g., December 2001) and begin practicing in the following calendar year (e.g., January 2002), resulting in a two-calendar-year gap between the written test and the start of legal practice.

To get a sense of the size of these effects, we compute the predicted earnings for an average individual with and without a license. Expected earnings are estimated using:

$$Y_{it} \times L_i = \beta_{1,t} L_i + X_i' \theta_t + \varepsilon_{it} \tag{4}$$

$$Y_{it} \times (1 - L_i) = \beta_{0,t} (1 - L_i) + X_i' \theta_t + \varepsilon_{it}, \tag{5}$$

where both L_i in equation (4) and $1 - L_i$ in equation (5) are instrumented using the results from the first bar exam. The coefficients $\beta_{1,t}$ and $\beta_{0,t}$ represent the average potential outcomes with treatment (with the license) and without treatment (without the license) for compliers. Results are reported in Figure 8.

Figure 8: Earnings of treated and non-treated compliers based on IV estimates

Notes: the figure reports the earnings of treated and non-treated compliers based on equations (4) and (5).

Depending on career stage, law graduates practicing law earn between 20% and 70% more than those in other occupations, with an average gap of 50%. Earnings rise steadily for both groups, but lawyers' earnings exhibit greater year-to-year variation—consistent with the higher uncertainty faced by self-employed professionals relative to salaried employees, particularly those in the public sector.

Table 2 summarizes these results by reporting average IV estimates over the full period (years 2–23) and over two subperiods. In absolute terms, the returns are smaller in the first eleven years, but as a share of non-treated compliers' earnings, they are stable at roughly 50% across all windows.

Table 2: Aggregate estimates—IV and ITT

	Years from bar exam		
	(1) 2-12	(2) 13-23	(3) 2-23
Earnings (IV)	16,908	22,999	19,825
	(6,300)	(16,074)	(10,188)
Mean earnings non-treated compliers	29,975	49,509	39,195
Earnings (ITT, reduced form)	4,171	5,514	4,891
	(1,468)	(3,833)	(2,471)
Mean earnings left cutoff	23,182	41,680	32,265
License (ITT, first-stage)	0.2467	0.2397	0.2467
	(0.0279)	(0.0279)	(0.0279)
Mean license left cutoff	0.5974	0.6017	0.5974
Observations	1,972	1,958	1,972

Note: This table reports separate IV and ITT estimates in different time windows since the bar exam and in the whole period available. Column (1) considers only the years between year 2 and year 12 since the bar. Column (2) considers years between year 13 and 23. Column (3) considers all years available.

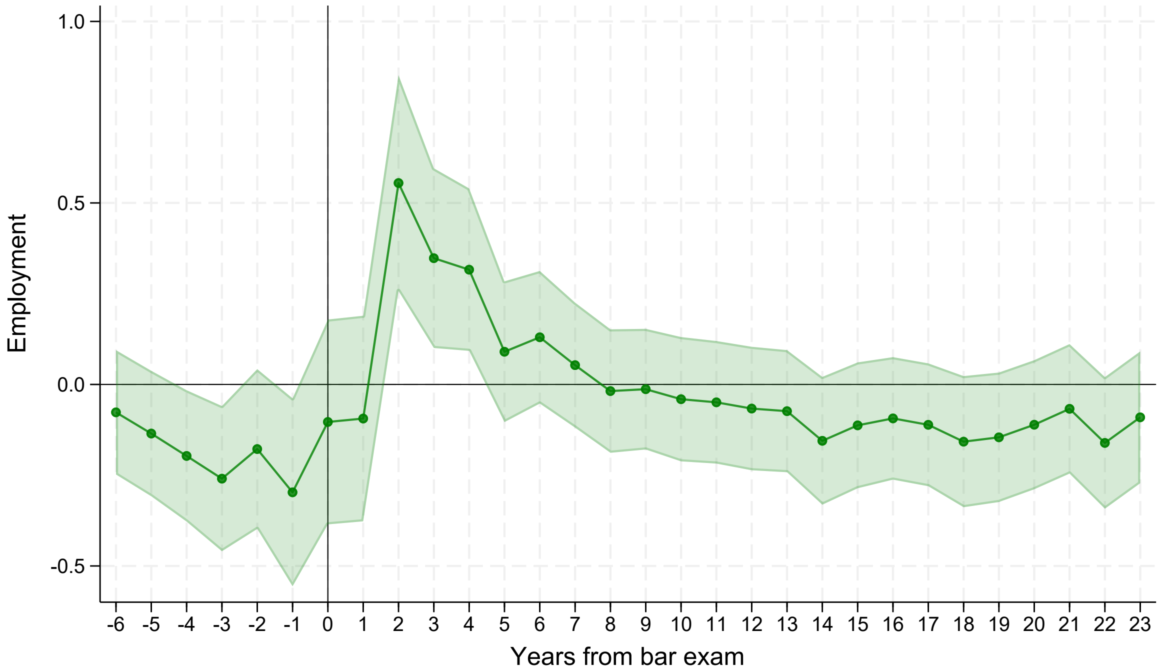
An alternative summary statistic is the present discounted value (PDV) of the estimated earnings differences from Figure 7. At a 5% annual discount rate over 22 years, the PDV amounts to 248,479 euros.

Since RD designs rely on local comparisons, one might worry that using the full sample is inappropriate. Figure A.5 shows that the results are robust to alternative bandwidth choices, polynomial orders, and the inclusion of individual controls.

Moreover, to dispel any concerns about the spike at the threshold in Figure 1, Figure A.6 shows that our results are robust to donut holes of different sizes—that is, by omitting individuals who scored 90 points (the passing grade) and a few points below.

We also examine whether the earnings effect is driven by the extensive margin—that is, whether licensed graduates are more likely to work. We define employment as having positive earnings in the social security data. A caveat is that apprentices at law firms often earn too little to appear in social security records, so we may misclassify some as non-employed. Figure 9 reports IV estimates with an employment indicator as the outcome. In the first few years after the bar, marginal passers are significantly more likely to be employed than marginal failers—likely because failers continue as (unrecorded) apprentices while retaking the exam. In later years, the employment gap vanishes, suggesting that those who do not obtain a license eventually abandon the exam and transition into other occupations.

Figure 9: IV estimates employment



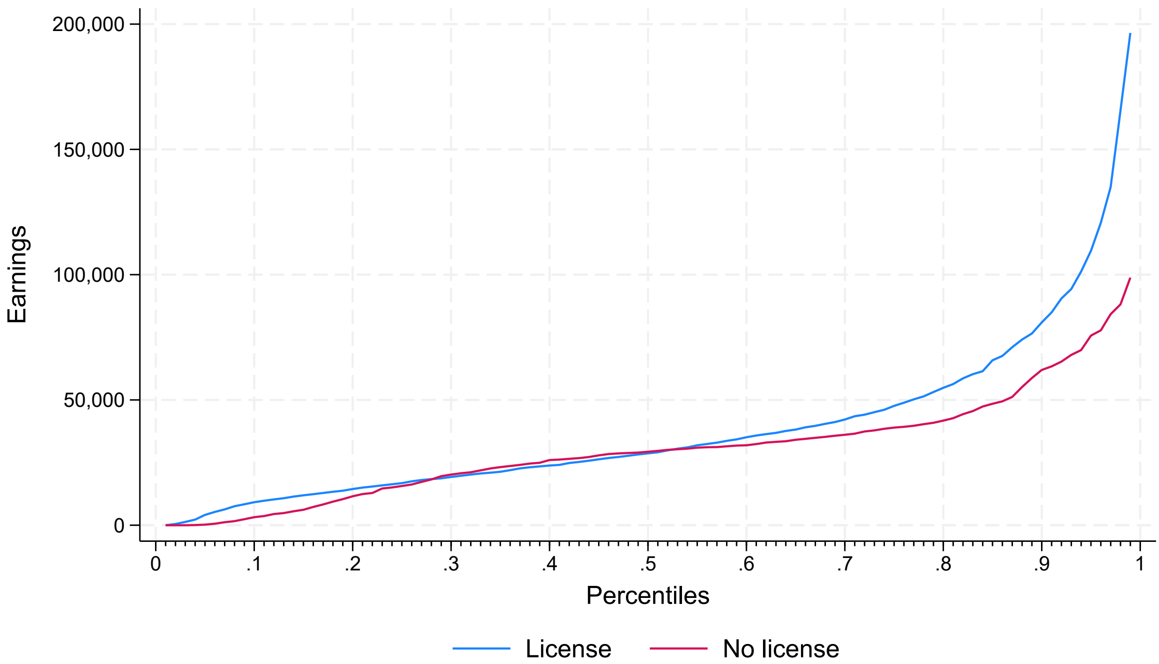
Notes: This figure reports the differences in employment status (defined as having earnings greater than 0), based on IV estimates from equation (1), between individuals with and without a license to practice law.

6.1 Returns across the earnings distribution

The average effects in Figure 7 and Table 2 may mask substantial heterogeneity. Figure 10 plots the inverse cumulative distribution function (CDF) of earnings for licensed and unlicensed law graduates.[19] The two distributions are nearly identical below the median but diverge sharply in the upper tail: the average earnings gap is driven entirely by top-earning lawyers.

[19]The inverse CDF reports, for each percentile, the corresponding earnings level. For instance, at the 90th percentile it gives the earnings threshold above which only 10% of individuals earn.

Figure 10: Inverse cumulative distribution function (CDF) of earnings



Notes: This figure reports the inverse cumulative distribution functions of annual earnings for licensed (lawyers) and non-licensed (non-lawyers) workers. The sample includes 1,972 observations.

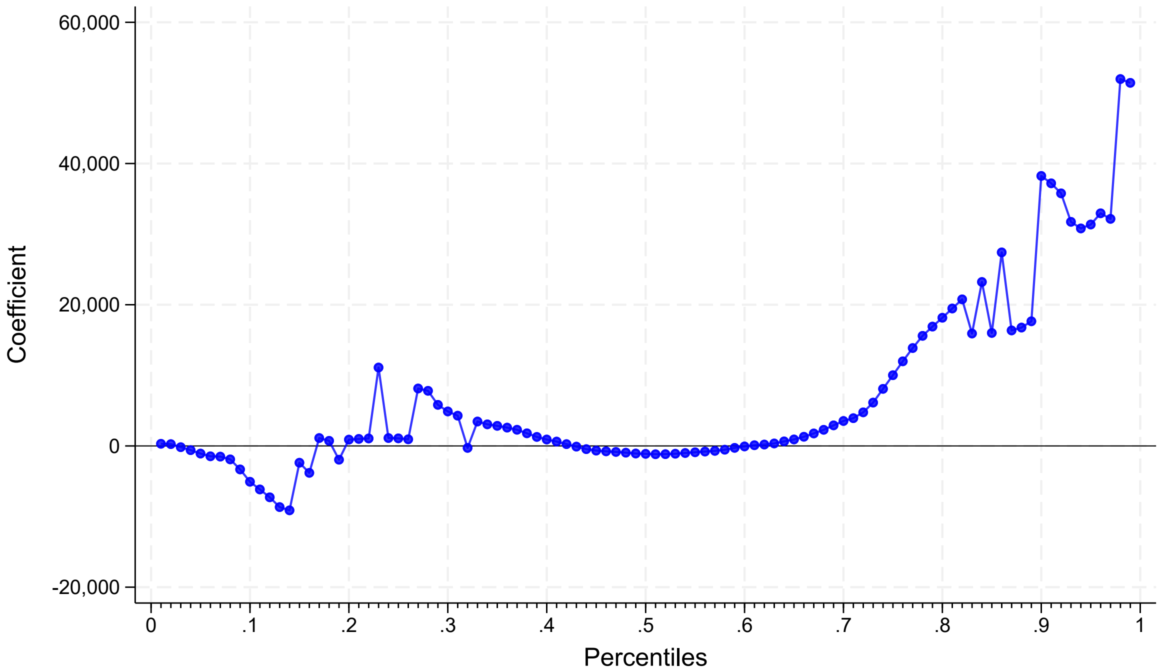
To corroborate the evidence presented in Figure 10, we report instrumental quantile regression results using the estimator proposed by Kaplan and Sun (2017) in Figure 11.[20] Figure 11 reports estimates for all percentiles. Because some estimates are imprecise, and to make the figure legible, we omit confidence intervals; however, Table 3 reports both point estimates and standard errors at selected percentiles. Table 3 also reports the earnings of non-treated compliers (non-lawyers) at these percentiles. Using these values to calculate the relative differences in earnings between the two groups confirms that earnings differences between lawyers and non-lawyers at the top of the distribution are larger than those in the lower part—not only in absolute terms, but also in relative terms.

These results show that the average effects reported in Figure 7 and Table 2 are driven by individuals at the top of the distribution, beginning around the 75th percentile. Below that, returns to licensing are smaller relative to the effects found at the top of the earnings distribution. This suggests that the benefits of practicing law, as opposed to pursuing other professions for law

[20]Kaplan and Sun (2017) propose an estimator for instrumental quantile regression (IQR) that addresses endogeneity when estimating quantile treatment effects. This approach generalizes the standard IV framework to a quantile setting, thereby allowing for heterogeneous effects of an endogenous regressor across the distribution of the outcome variable.

graduates, accrue primarily to highly successful lawyers at the upper end of the distribution. In contrast, there are no clear differences in earnings when comparing less successful lawyers with law graduates in other occupations.

Figure 11: Quantile IV regression



Notes: This figure reports point estimates from an instrumental quantile regression at each percentile from the 1st to the 99th. The estimation sample includes 1,972 observations.

Table 3: Quantile IV regression

	Percentiles				
	(1) 10th	(2) 25th	(3) 50th	(4) 75th	(5) 90th
Earnings	-5,073	1,080	-1,117	10,022	38,247
	(14,743)	(32,324)	(4,796)	(9,676)	(10,379)
Earnings of non-treated compliers	11,600	24,700	33,000	46,400	75,500

Notes: This table reports point estimates and standard errors from an instrumental quantile regression at selected percentiles, along with the corresponding earnings levels of non-treated compliers. The estimation sample includes 1,972 observations.

The results reported in Figure 10 and 11, and Table 3, show that earnings inequalities arising from access to the regulated legal profession widen along the earnings distribution. Below the top percentiles, practicing law does not yield higher returns than working in other occupations, whereas at the upper end, substantial returns to practicing law become evident.

7 Mechanisms

The existence of an earnings premium for licensed lawyers raises the question of its source. We consider four potential explanations: (i) monopoly rents from entry barriers that shield lawyers from competition; (ii) non-monetary job attributes—such as longer working hours or lower flexibility—that justify compensating wage differentials; (iii) a risk premium for the greater earnings volatility inherent in legal practice; and (iv) a sheepskin effect, whereby the license signals quality and commands higher wages even though it does not augment human capital (all individuals in our sample hold the same law degree).

We discuss each mechanism below and conclude that a compensating differential for income risk is the most plausible explanation.

7.1 Monopoly rents and entry barriers

The seminal work of Leland (1979), building on Akerlof's concept of "the market for lemons" (Akerlof, 1970), investigates the conditions under which quality standards imposed by a profession are socially optimal, and whether these standards are invariably set too restrictively to secure monopoly rents. When a professional group is permitted to establish minimum quality standards, these standards may be set excessively high to extract monopoly rents for incumbents.

The Italian government imposes no quota on the number of new lawyers, but there are reasons to suspect that part of the substantial earnings premium we observe reflects monopoly rents generated by entry barriers. The examining commission for the bar exam consists of five members: three lawyers nominated by the local bar association, and one judge (usually retired) and one university professor, both nominated by the Ministry of Justice. Crucially, the president of the commission must be chosen from among the three lawyers. Therefore, the lawyers on the commission may have an incentive to limit competition by applying particularly stringent grading standards during the exam.

However, several facts suggest that rents are unlikely to be important in this context. First, because law graduates can retake the bar indefinitely, any potential rents are eroded over time.

In our sample, 60% of first-time failers eventually passed, contributing to a steady influx of new entrants. By 2010, Italy had the highest number of registered lawyers among comparable European countries—217,000, versus 161,000 in Spain, 153,000 in Germany, and 50,000 in France—and this gap has persisted.[21] Second, we directly test for monopoly rents by examining the relationship between lawyers' revenues and the intensity of competition.

Each lawyer's revenue equals the market price of legal services times the quantity they provide. Under perfect competition, prices equal marginal costs; under imperfect competition, a markup drives a wedge between the two. Markups decline as competition increases. We assume no supplier-induced demand—that is, aggregate demand for legal services does not depend on the number of lawyers.

Under these assumptions, a negative relationship between total revenues and the number of lawyers would signal declining markups, while insensitivity would imply the absence of rents. Because we observe individual revenues, we can implement this test.

Formally, for each lawyer i, their revenues, s_i, are:

$$s_i(N) = d_i(N)P(N), \tag{6}$$

where P is the equilibrium price of legal services, and d_i is the demand for legal services captured by lawyer i. As standard, P is equal to marginal costs and, potentially, a markup:

$$P(N) = MC + markup(N). \tag{7}$$

Markups decrease with the number of lawyers, N, as competition increases; the equilibrium price of legal services then approaches marginal costs. The demand for legal services captured by lawyer i, $d_i(N)$, is also decreasing in N because, for a given total demand for legal services, as the number of lawyers increases, each lawyer captures a smaller share. However, under the assumption of no supplier-induced demand, the total demand for legal services in the economy does not depend on N. To see this, we define total revenues, $S(N)$, as the sum of equation (6) over all lawyers i:

$$S(N) = \sum_{i=1}^{N} s_i(N) = P(N) \sum_{i=1}^{N} d_i(N) = P(N)D \tag{8}$$

[21] Source: official statistics of the Council of Bars and Law Societies of Europe (CCBE).

where D does not depend on N: the total demand for legal services in the economy, D, does not depend on the number of lawyers.[22] Finally, total revenues $S(N)$ can be written as:

$$S(N) = [MC + markups(N)]D, \tag{9}$$

which decline with the number of lawyers solely through the effect on markups. Even without observing prices or markups directly, this allows us to test for the presence of markups indirectly by examining the relationship between total revenues and the number of lawyers. In the absence of monopoly rents, there are no markups, and prices equal marginal costs—implying that total revenues do not depend on the number of lawyers. But with imperfect competition, total revenues and the number of lawyers should be inversely related. We empirically test this hypothesis by examining the correlation between the number of lawyers and total revenues across Italian Commuting Zones (CZs).[23]

We use CZ-level data from 2006 to 2019 to assess the relationship between competition and total revenues. For each of the 611 CZs in Italy, we calculate lawyers' total revenues per capita by dividing total revenues by the number of inhabitants. We regress this variable on the number of lawyers per inhabitant, controlling for the level of economic activity, employment rate, and labor force participation. To account for aggregate shocks and CZ-specific characteristics, we include year and CZ fixed-effects. Additionally, to capture differences in the demand for legal services across CZs, we add CZ-specific linear time trends. Specifically, our empirical model is as follows:

$$\frac{\sum_{i=1}^{N} Rev_{ict}}{Pop_{ct}} = \beta \frac{L_{ct}}{Pop_{ct}} + \gamma X_{ct} + \tau_t + \phi_c + t \times \phi_c + \varepsilon_{ct} \tag{10}$$

where Rev_{ict} represents the revenues of lawyer i in CZ c in year t; Pop_{ct} denotes the population; L_{ct} indicates the number of lawyers; and X_{ct} includes control variables such as the labor force participation rate, employment rate, and unemployment rate. τ_t represents year fixed effects, ϕ_c denotes CZ fixed effects, and $t \times \phi_c$ captures CZ-specific linear time trends, which account

[22] This assumption would be violated if the supply of lawyers increased in response to higher demand for legal services. This is an important point that we explicitly address empirically.

[23] Commuting zones are geographic areas defined by commuting patterns—that is, by where people live and where they work. These zones group together neighboring municipalities (or other administrative units) that are economically integrated through daily commuting flows. The goal is to identify labor market areas that reflect the real economic geography, rather than relying strictly on administrative boundaries. In Italy, the National Institute of Statistics (ISTAT) defines commuting zones, known as "Sistemi Locali del Lavoro" (SLL), which translates literally as Local Labor Systems.

for variation in the demand for legal services. Our primary focus is on the coefficient β, which captures the extent to which total revenues in a CZ decline as the number of lawyers increases. According to our simple theoretical framework, $\beta < 0$ would suggest the presence of monopoly rents, which are eroded as competition among lawyers increases. By contrast, $\beta = 0$ would imply that markups have already been competed away and are equal to marginal costs, implying the absence of monopoly rents.

The estimates of β from regression (10) are reported in Table 4, which show no evidence of a negative correlation between competition (measured as the number of lawyers per capita) and total revenues across different CZs. If anything, column (1) suggests a positive relationship between the number of lawyers and total revenues, which could potentially be driven by a demand effect: as the demand for legal services increases, the number of lawyers rises, thereby driving total revenues up. To account for this response in the supply of lawyers driven by demand, we include CZ-specific linear trends to control for demand effects within each CZ. Incorporating these trends results in a precisely estimated zero effect of lawyers' density on total revenues ($\beta = 0$), as reported in column (2). A one standard deviation increase in lawyers' density leads to only a 0.7% increase in total revenues relative to the mean. These results challenge the notion that lawyers' earnings premium reflects monopoly rents generated by entry barriers.

Table 4: No effect of lawyers' density on total revenues across CZs

	(1)	(2)
$\frac{\text{Lawyers}}{\text{Population}}$	20,876	876
	(5,333)	(2,006)
Labor force participation rate	10,764	-4,307
	(7,870)	(6,114)
Empolyment rate	-9,481	5,459
	(8,958)	(7,127)
Unemployment rate	-15,660	1,549
	(4,243)	(2,678)
Observations	8,376	8,376
Years fixed-effects	✓	✓
CZ fixed-effects	✓	✓
CZ trends		✓

Note: The table reports weighted least squares (WLS) estimates of equation (10), estimated at the CZ level and weighted by the number of lawyers in each CZ. Italy is divided into 611 CZs, and we use data from the years 2006-2019. The mean and the standard deviation of the outcome variable (lawyers' total revenues per capita) are 240,239 and 179,170, respectively; the mean and the standard deviation of lawyers' density, $\frac{\text{Lawyers}}{\text{Population}}$, are 4.11 and 1.85. Standard errors are clustered at the CZ level.

7.2 Non-monetary aspects of practicing law

Earnings are only one dimension along which the legal profession differs from the alternatives; lawyers also work longer hours and enjoy less flexibility.

Social security data lack information on hours worked, so we turn to the Labor Force Survey (LFS),[24] which contains self-reported hours and education. Among law graduates in Piedmont (the region containing Turin), lawyers work 13% more hours than those in other occupations. Lawyers are self-employed and receive no paid leave, which could inflate their reported hours; however, LFS interviews are conducted outside the holiday season, mitigating this concern. While longer hours partly explain the premium, a 13% gap in hours cannot account for a 50% gap in earnings.

Another form of compensating differentials is the lack of flexibility. As shown by Goldin (2014), lawyers tend to have many clients, maintain frequent contact with them, and have little

[24] https://www.istat.it/en/tag/labour-force/

discretion over how to organize their time. This lack of time flexibility may also help justify their high earnings.

7.3 Lawyers face more uncertainty

Another factor that may explain the returns to the legal profession is the compensating differential for the higher uncertainty faced by law graduates practicing law relative to those in other occupations. Lawyers are self-employed, and their earnings are proportional to the quantity and quality of the cases they handle. In contrast, the wages of employees, especially in the public sector, are subject to less volatility. Table 5 shows the within and between standard deviations of earnings for licensed and non-licensed workers. Lawyers' earnings are twice as volatile as those of law graduates working as employees, considering both variations within an individual over time and variations across individuals. These results suggest that the higher earnings enjoyed by lawyers may be justified as a compensating differential for the higher risk they face.

Table 5: Earnings volatility for lawyers and non-lawyers

	Non-licensed	**Licensed**
Mean	30,799	40,034
Standard deviation overall	27,531	59,677
Standard deviation between	22,062	44,581
Standard deviation within	16,463	39,665
Observations	9,773	33,299
Individuals	453	1,519

Note: This table reports the mean earnings and the overall, between-, and within-standard deviations of earnings for licensed and non-licensed workers. The sample includes 1,972 individuals and 43,072 observations.

7.3.1 Regression discontinuity design (RDD) estimates of earnings volatility

To corroborate the findings reported in Table 5, we also consider our RDD estimate using earnings volatility as the outcome. Following Bordignon et al. (2016), we measure the volatility of earnings in two ways. First, we examine the *intertemporal* variation in earnings. To do this, we calculate the unconditional standard deviation of earnings across years for each individual. For each person, we compute this unconditional standard deviation across all years, obtaining one observation (i.e., one measure of volatility) per person. Second, we analyze the *cross-sectional* variation in earnings within bins of individuals with the same bar exam grades, which serve as the running variable in the RDD. Specifically, for each year after the bar exam and for each bin defined by grades, we compute the unconditional variance of earnings, obtaining a cross-sectional standard deviation for each bin.

In addition to these measures, we consider the difference between the 90th and the 10th percentile of the cross-sectional earnings distribution within each bin and year. This measure allows us to reduce the influence of outliers that may disproportionately affect the standard deviation. Finally, since workers tend to be more affected by losses than by equivalent gains, we also adopt a downside-oriented measure of volatility, defined as the probability that earnings decline by 5% or more relative to the previous year.

These RDD results are reported in Table 6 for all measures of volatility. Although imprecisely estimated, the intertemporal standard deviation is 80% larger for lawyers. Similar results, but more precisely estimated, hold for the cross-sectional standard deviation. In line with these findings, results are confirmed when using alternative measures of earnings volatility. In particular, when we consider the difference between the 90th and the 10th percentile of the earnings distribution, we find a significant increase in volatility for lawyers. Moreover, using a downside risk measure based on the probability of an earnings decline of at least 5%, we find that this probability rises from about 10% for non-treated compliers to 39% for treated individuals. Here, all estimates are calculated using weighted least squares (with weights based on the frequency of individuals in each bin) to account for heteroskedasticity and to accommodate for the differing accuracy in the estimation of the standard deviation across bins of varying sizes. Taken together, all measures consistently indicate that lawyers face earnings volatility at least twice as high as that of non-lawyers.

Table 6: RDD estimates variation of earnings

	earnings standard deviation		90th–10th Percentile Gap	P(Earnings Drop >5%)
	Intertemporal	**Cross-sectional**		
IV estimates	11,706	27,924	58,561	0.29
	(7,714)	(11,793)	(21,555)	(0.14)
Mean non-treated compliers	14,979	23,194	48,845	0.10
Observations	1,971	2,310	2,310	2,637

Note: This table reports RDD estimates for the *intertemporal* variation (the standard deviation across years) and the *cross-sectional* variation (the standard deviation across individuals within bins defined by the grade on the bar exam) in earnings. Additional measures of earnings volatility are the 90th–10th percentile of earnings gap within each bin-year cell and the probability of an earnings decline of at least 5% relative to the previous year. Estimates are obtained by weighted least squares, with weights given by the inverse of the number of observations in each bin, when using the last three measures of earnings volatility.

Overall, these results confirm that lawyers face greater uncertainty, as reflected in higher earnings volatility, both in terms of variation within an individual over time and variation across individuals. These findings align with those presented in section 6.1, where we show that the earnings premium for lawyers is not guaranteed but is concentrated among those at the top of the earnings distribution. Taken together, our evidence suggests that the higher earnings observed among lawyers may reflect a compensating differential for the greater risk inherent in their profession.

7.3.2 Estimated risk aversion with CRRA utility function

To substantiate the claim that the higher earnings of lawyers compensate for the greater uncertainty inherent in their profession, we estimate the coefficient of relative risk aversion required to equate the expected utility derived from working as a lawyer to that of the next best occupation. To model utility under uncertainty, we use the Constant Relative Risk Aversion (CRRA) utility function, defined as $\frac{w_t^{1-\rho}}{1-\rho}$, where the parameter ρ captures the degree of risk aversion. We compute the sample analogue of the expected CRRA utility for two groups of individuals: lawyers and non-lawyers. Finally, we solve for the coefficient ρ such that the expected utilities of the two groups are equal.

We estimate a coefficient of risk aversion (ρ) equal to 1.7, which suggests a relatively moderate degree of risk aversion consistent with the literature. This result indicates that the observed choices of our law graduates—to enter the legal profession or to work in other occupations, such as the public sector—are consistent with rational decision-making. Individuals exhibit a reasonable degree of risk aversion and are indifferent between low-risk careers outside the legal field and the high-risk legal profession. Thus, our findings lend further support to the argument that the earnings premium associated with working as a lawyer compensates for the higher risks inherent in the legal profession.

Finding a reasonable coefficient of risk aversion also explains why we observe a first-stage: not everyone who barely fails the bar exam tries again later and eventually succeeds. Consider a recent law graduate who has just missed the passing threshold by a few points; their choice is to wait and try the bar exam again next year or to pursue a different career path. Waiting is risky because it is not certain that the graduate will pass the exam the following year. Moreover, even if they pass, high earnings in the legal profession are not guaranteed, as the previous sections have shown: the earnings premium for lawyers is concentrated at the top of the distribution, and the legal profession is characterized by substantial risk, as indicated by the lawyers' high earnings volatility.

7.4 Sheepskin effect

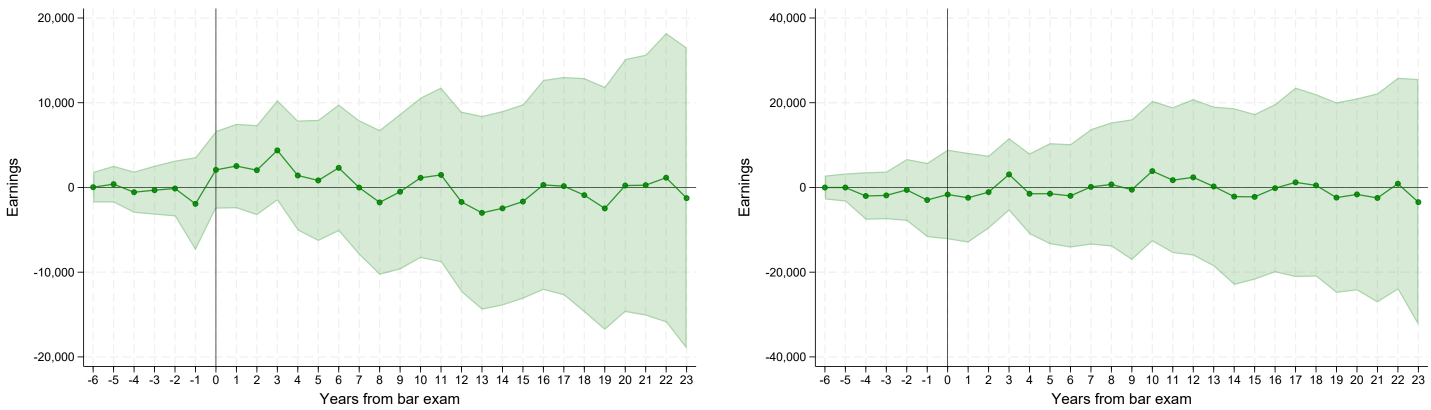
The *sheepskin effect* is the phenomenon whereby people possessing a completed academic degree earn more than people with an equivalent amount of education (i.e., human capital) who do not possess the degree. This effect suggests that the academic degree (historically, degrees were written on sheepskins) serves as a signal of productivity or quality to employers.

In our context, the sheepskin effect implies that licensed workers may earn higher wages than non-licensed workers, not necessarily because they work as lawyers, but rather because holding the license to practice law serves as a signal of quality. To investigate this, we repeat our RDD estimates as detailed in equation (1) for the subgroup of law graduates who do not work as lawyers. Specifically, we compare the earnings of law graduates with and without the license, restricting the sample to individuals working outside the legal profession. Figure 12 reports these estimates. Panel (a) considers all law graduates not working as lawyers, regardless of the sector in which they work; panel (b) further restricts the subgroup to law graduates working in the public sector, who still do not practice law.[25] Both figures reveal no significant earnings

[25]We classify a person as not being a lawyer if the income earned from working as a lawyer is never greater than 10% of their total income.

differences between law graduates with and without the license to practice law.

Figure 12: No sheepskin effect—IV estimates earnings



(a) Non-lawyers (all) (b) Non-lawyers (public sector workers only)

Notes: The figure reports the differences in earnings between individuals with and without a license to practice law, obtained from the IV estimates based on equation (1), restricted to law graduates who do not work as lawyers. Panel (a) depicts all individuals not working as lawyers (regardless of the sector of employment), while panel (b) focuses on the subset employed in the public sector. The shaded area denotes the 95% confidence intervals.

These findings rule out a sheepskin effect: the license has value only insofar as it grants access to the legal profession. Outside legal practice, it confers no additional economic benefit. This reinforces the interpretation that the earnings premium reflects a feature inherent to the profession itself—most plausibly, a compensating differential for earnings risk.

8 Conclusions

Quantifying the returns to professional licensing has long been a central question in labor economics, one that has grown more pressing as licensing requirements have expanded dramatically in recent decades.

Existing work has relied on cross-sectional comparisons or cross-state difference-in-differences designs that capture general equilibrium effects, or on admission discontinuities that bundle the returns to occupation with major-specific human capital accumulation.

In this paper, we exploit the sharp discontinuities in the Italian bar exam that create quasi-random variation in the probability of obtaining the license to practice law. Because the supply of lawyers remains unchanged and all candidates taking the bar exam have already attended law school, we identify the returns to occupational licensing net of general equilibrium effects and major-specific human capital accumulation.

We find that marginal passers earn 50% more than marginal failers, a premium that remains stable over 23 years and is concentrated at the top of the earnings distribution. Crucially, this premium does not appear to reflect restricted competition. Instead, it is best explained by the greater income uncertainty that lawyers face: a compensating differential for the risk inherent in self-employed legal practice.

Consistent with previous findings for high-skilled professionals, we find substantial returns to working in specific regulated occupations. Yet, because this earnings premium stems from the higher income uncertainty inherent in the legal profession rather than from monopoly rents created by entry barriers, it should not be interpreted as a social cost. This distinction carries important policy implications: reforms aimed at relaxing entry barriers to the legal profession, such as lowering the bar exam's passing threshold or eliminating it altogether, would not be expected to reduce the earnings gap between lawyers and non-lawyers. The earnings premium we document compensates lawyers for the higher risk inherent in self-employment within the legal profession, and this risk differential would persist regardless of changes to licensing requirements. More broadly, our findings suggest that policymakers evaluating the costs and benefits of occupational licensing should carefully distinguish between earnings premia driven by restricted competition and those reflecting compensating differentials for occupational risk.

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Appendix

Table A.1: Sample construction

Database	Individuals	Exams
Bar exam in 1994-2000	3,508	6,053
Bar exam in 1994-1995	1,263	1,617
Bar exam in 1996-2000	2,815	4,436
First time exam takers in 1996-2000	2,174	2,174
Linked to Social Security Agency	1,972	1,972

Note: the table summarizes the databases at each step of the data linkage between the data from bar exams and social security database. The sample of first time exam takers in 1996-2000 is also linked to the database of the University of Turin, out of 2,174 individuals, 1,429 could be matched.

Table A.2: Characteristics of compliers

	Compliers mean	Overall mean	Observations
Female	0.62	0.61	2,174
Age at bar	28	29	2,174
Born Torino	0.48	0.43	2,174
Connected family name	0.17	0.16	2,174
Graduation grade	105	103	1,429
Age at graduation	25	26	1,429

Note: compliers are individuals who become lawyers because they pass the first attempt at the bar exam, but would not have become lawyers had they not passed their first attempt. The estimated fraction of compliers is 20%.

Table A.3: Individual characteristics are balanced around the cutoff grade

	(1) Graduation grade	(2) Age at graduation	(3) Share females	(4) Share born in Turin	(5) Age at bar exam	(6) Connected family name
Full sample						
Pass bar exam	-0.230	-0.256	0.012	0.008	-0.340	0.025
	(0.540)	(0.199)	(0.034)	(0.035)	(0.295)	(0.025)
Mean outcome left cutoff	102	27	.604	.398	30	.147
Observations	1,429	1,429	2,174	2,174	2,174	2,174
Full sample, 2nd order poly						
Pass bar exam	-0.416	0.064	0.035	-0.070	0.062	0.036
	(0.817)	(0.330)	(0.050)	(0.052)	(0.481)	(0.036)
Mean outcome left cutoff	102	27	.604	.398	30	.147
Observations	1,429	1,429	2,174	2,174	2,174	2,174
Donut						
Pass bar exam	0.669	-0.396	0.024	0.026	-0.689	0.001
	(0.630)	(0.232)	(0.042)	(0.043)	(0.314)	(0.030)
Mean outcome left cutoff	102	27	.604	.398	30	.147
Observations	1,161	1,161	1,791	1,791	1,791	1,791
Donut large						
Pass bar exam	0.648	-0.463	0.016	0.037	-0.765	-0.003
	(0.661)	(0.247)	(0.043)	(0.044)	(0.341)	(0.031)
Mean outcome left cutoff	102	27	.604	.393	30	.148
Observations	1,116	1,116	1,727	1,727	1,727	1,727
Bandwidth 30						
Pass bar exam	-0.289	0.036	0.022	-0.027	-0.057	0.026
	(0.617)	(0.257)	(0.038)	(0.038)	(0.359)	(0.028)
Mean outcome left cutoff	102	27	.608	.396	30	.147
Observations	1,400	1,400	2,127	2,127	2,127	2,127
Bandwidth 15						
Pass bar exam	-1.126	0.136	0.016	-0.058	0.001	0.036
	(0.828)	(0.400)	(0.054)	(0.054)	(0.528)	(0.038)
Mean outcome left cutoff	103	27	.609	.404	30	.147
Observations	1,168	1,168	1,765	1,765	1,765	1,765
Bandwidth 10						
Pass bar exam	-0.565	-0.030	0.106	-0.078	-0.202	0.045
	(1.127)	(0.366)	(0.077)	(0.078)	(0.684)	(0.056)
Mean outcome left cutoff	103	26	.623	.418	30	.144
Observations	917	917	1,367	1,367	1,367	1,367

Note: the table reports results from the following model:

$$Y_i = \gamma P_i + X_i'\delta + f(g_i) + \nu_i$$

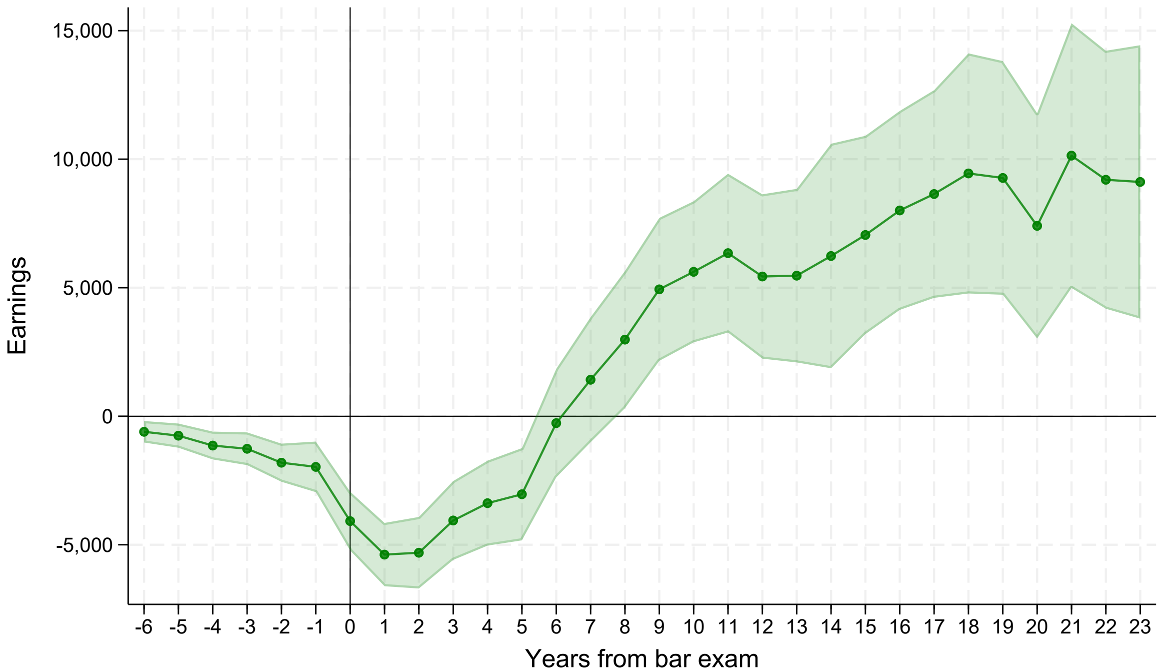
where each column refer to a separate regression with different outcomes denoted by Y_i; P_i is a dummy variable taking value 1 if the individual scores 90 or more at the his or her first attempt at the bar exam; and $f(g_i)$ is a polynomial in the grades at the first bar exam; X_i is a vector of controls including year of the exam fixed-effects, gender and the year of birth of the individual; ν_i is the error term. "Donut" refers to the subsample removing candidates scoring 90 points; "Donut large" refers to the subsample removing candidates scoring 86, 87, 88, 89, and 90 points.

Table A.4: Aggregate estimates—OLS

	Earnings		
	(1)	(2)	(3)
License	9,068	8,473	4,770
	(1,542)	(1,478)	(1,467)
Year of exam fixed-effects	✓	✓	✓
Gender		✓	✓
Year of birth fixed-effects			✓

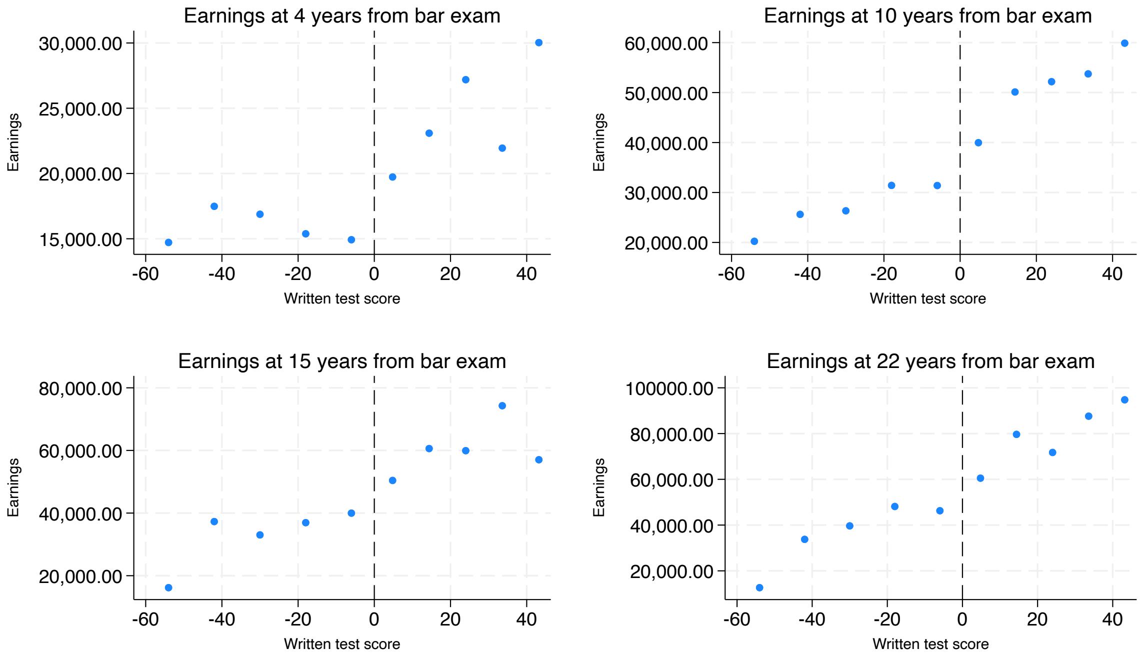
Note: 1,972 observations. The average earnings of law graduates without a license are 30,655.

Figure A.1: OLS earnings



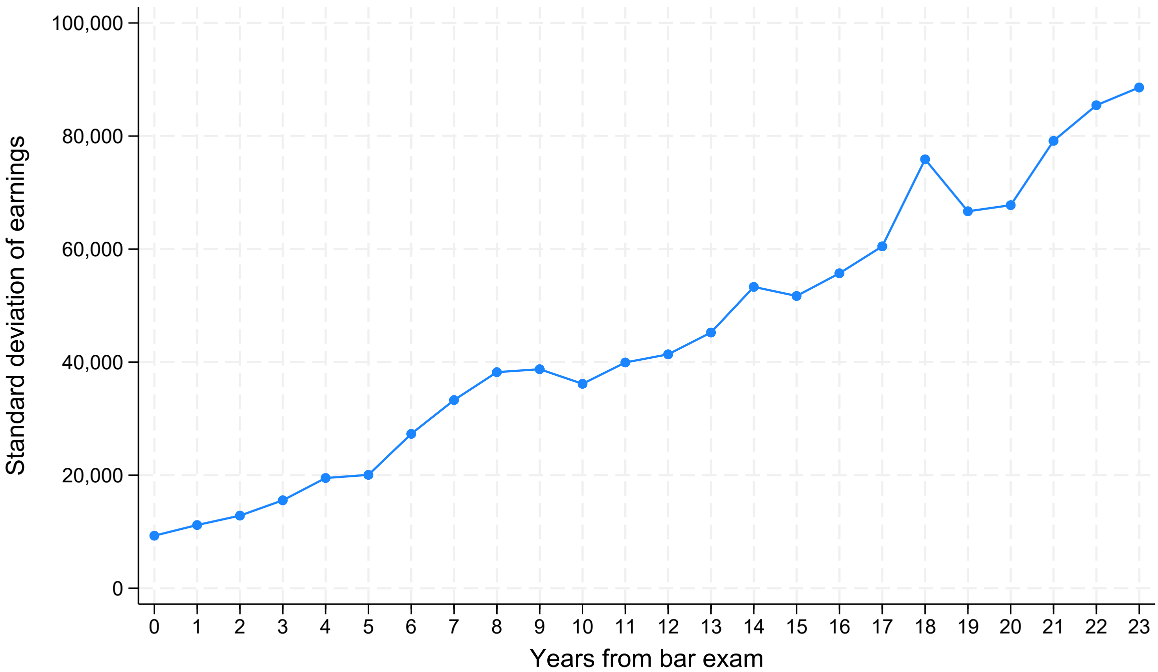
Notes: controls include gender and year of birth dummies.

Figure A.2: Passing at the first attempt at the bar increases earnings



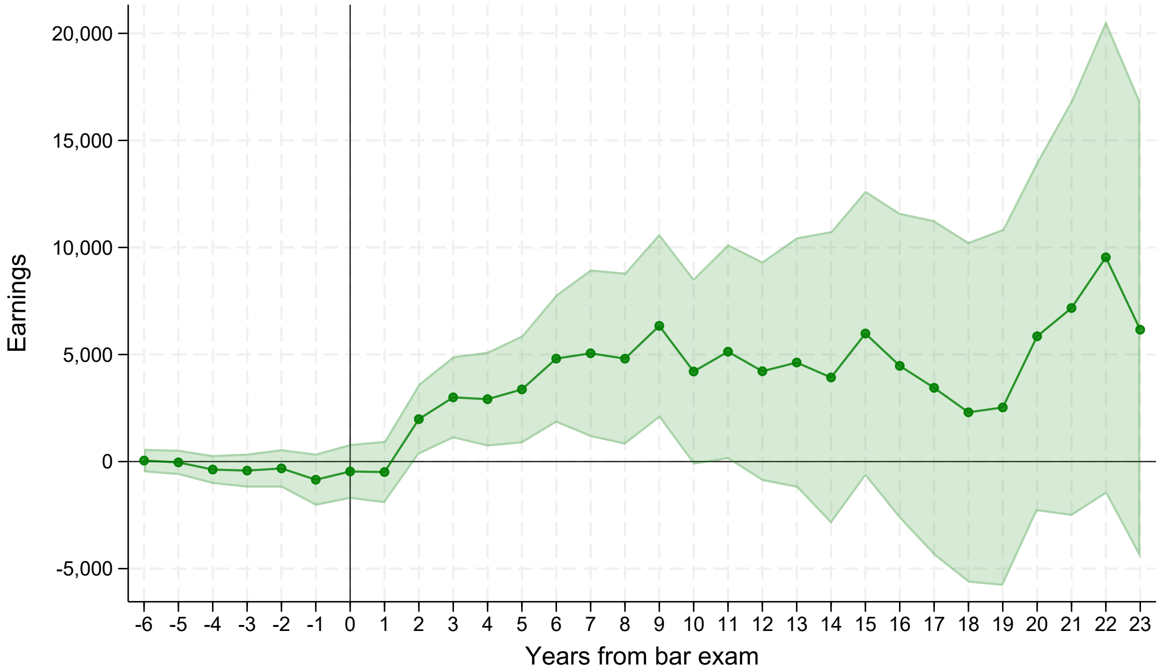
Notes: The figure shows that passing the bar exam on the first attempt increases earnings. Each dot refers to the earnings of individuals within an interval of the grade received at the first bar exam. These intervals contain roughly the same number of observations.

Figure A.3: Standard deviation of earnings



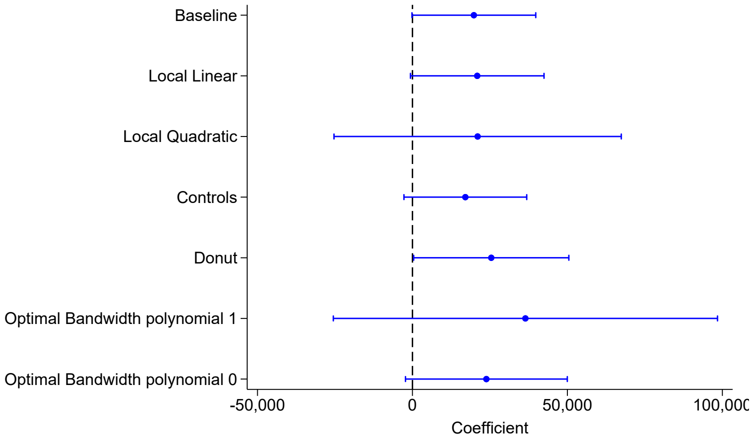
Notes: the figure reports the standard deviation of earnings in each year from the first bar exam.

Figure A.4: Intention-to-treat (ITT) estimates

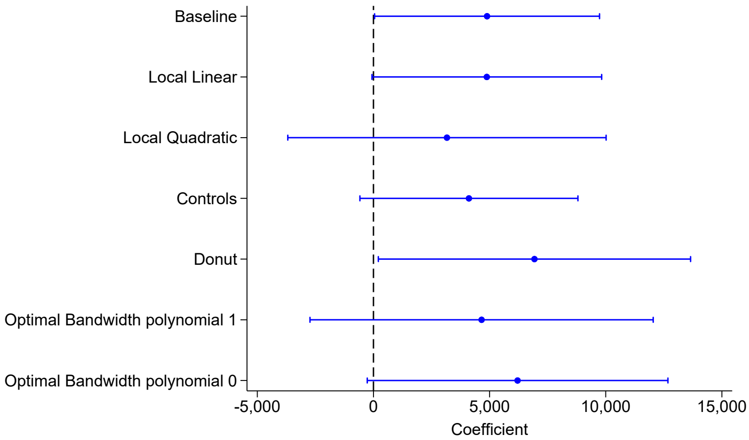


Notes: Full sample and linear polynomial.

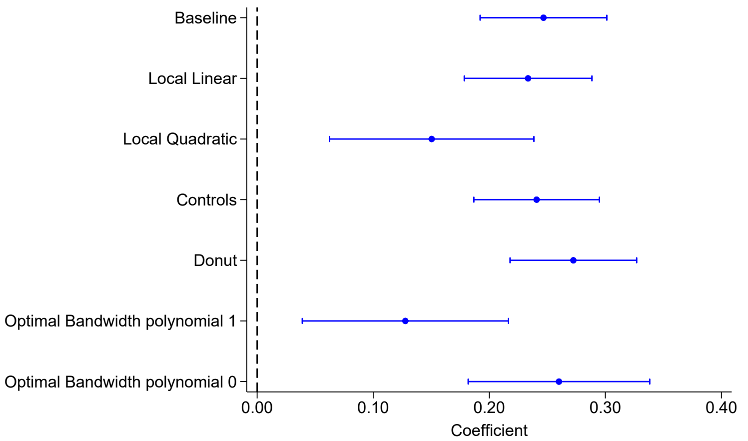
Figure A.5: Robustness to bandwidth choice and polynomials



(a) Instrumental variable estimates



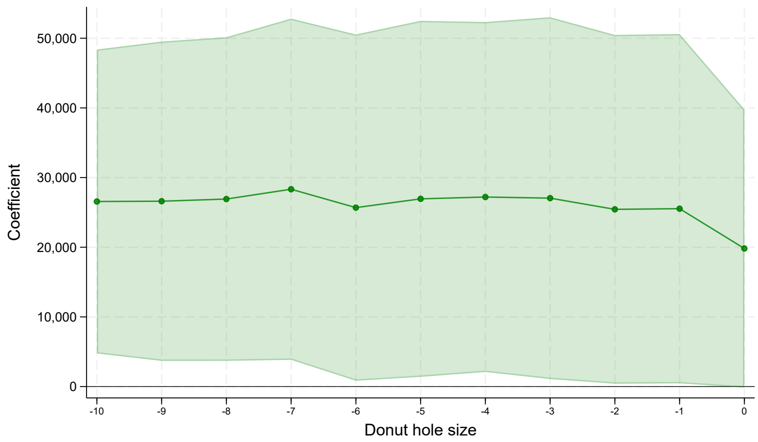
(b) Reduced form estimates



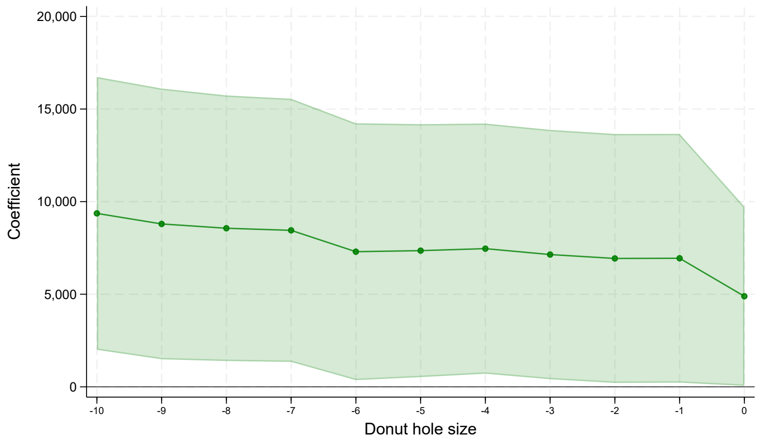
(c) First-stage estimates

Notes: The figure reports point estimates and 95% confidence intervals over the entire period. The optimal MSE (Mean Square Error) bandwidth is 16 with a 1st order polynomial and 5 with a 0 order polynomial. The donut removes all individuals scoring exactly at the passing threshold and one point below it. Additional controls are gender and age at the bar exam. Local linear and quadratic regressions use the epanechnikov kernel. All estimates control for year of exam fixed-effects.

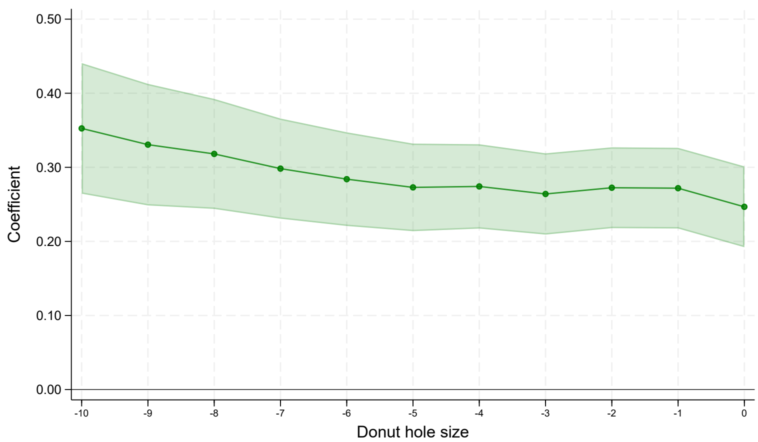
Figure A.6: Donut hole—second to twenty-third years after the bar



(a) Instrumental variable estimates

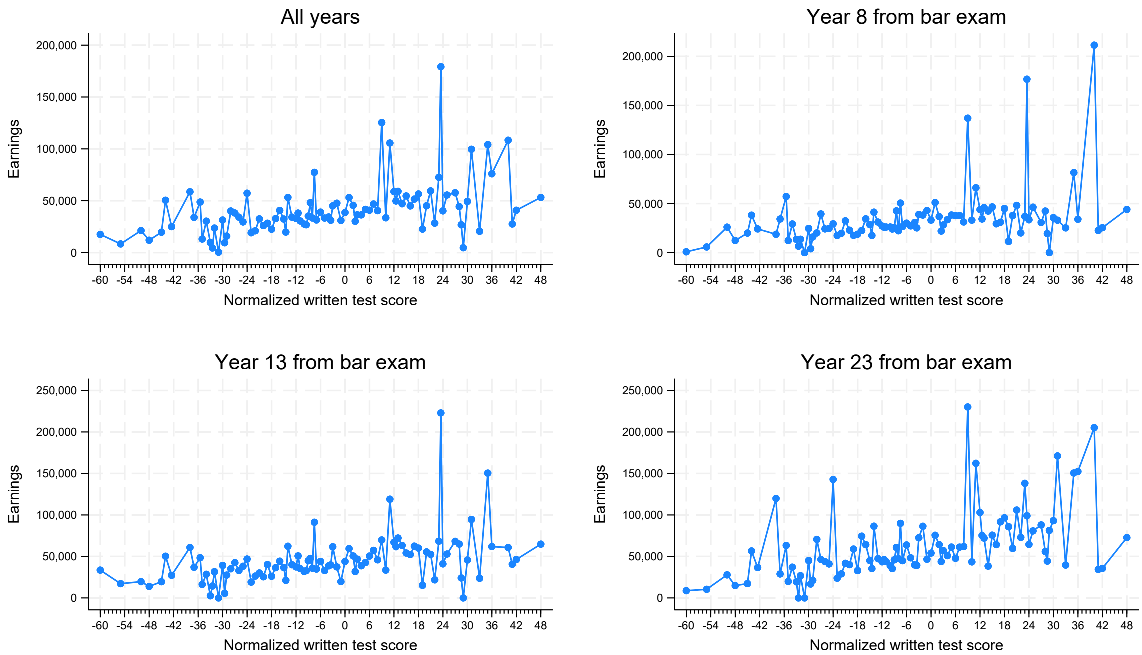


(b) Reduced form estimates



(c) First-stage estimates

Figure A.7: Earnings by grade at the written test



Notes: This figure reports earnings at different years since the first bar exam as a function of the grades at the bar. It clearly shows that mean earnings do not fall sharply in the range of marginal-fail scores.