
INPS
Istituto Nazionale Previdenza Sociale

maggio 2026 – numero 71

WorkINPS *Papers*

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

Omar Bamieh

Andrea Cintolesi

Mario Pagliero

April 2026

Aggiornamento Workinps papers n. 71 – (ottobre 2023)

ISSN 2532 -8565

Lo scopo della serie WorkINPS papers è quello di promuovere la circolazione di documenti di lavoro prodotti da INPS o presentati da esperti indipendenti nel corso di seminari INPS, con l'obiettivo di stimolare commenti e suggerimenti.
Le opinioni espresse negli articoli sono quelle degli autori e non coinvolgono la responsabilità di INPS.

The purpose of the WorkINPS papers series is to promote the circulation of working papers prepared within INPS or presented in INPS seminars by outside experts with the aim of stimulating comments and suggestions.
The views expressed in the articles are those of the authors and do not involve the responsibility of INPS.



*In copertina: uno storico "Punto cliente" a Tuscania*
*INPS, Direzione generale, Archivio storico*

## I WORKINPS PAPER

Le basi dati amministrative dell'*INPS* rappresentano una fonte statistica unica per studiare scientificamente temi cruciali per l'economia italiana, la società e la politica economica: non solo il mercato del lavoro e i sistemi di protezione sociale, ma anche i nodi strutturali che impediscono all'Italia di crescere in modo adeguato. All'interno dell'Istituto, questi temi vengono studiati sia dai funzionari impiegati in attività di ricerca, sia dai ***VisitInps Scholars***, ricercatori italiani e stranieri selezionati in base al loro curriculum vitae e al progetto di ricerca presentato.

I **WORKINPS** hanno lo scopo di diffondere i risultati delle ricerche svolte all'interno dell'Istituto a un più ampio numero possibile di ricercatori, studenti e policy markers.

Questi saggi di ricerca rappresentano un prodotto di avanzamento intermedio rispetto alla pubblicazione scientifica finale, un processo che nelle scienze sociali può chiedere anche diversi anni. Il processo di pubblicazione scientifica finale sarà gestito dai singoli autori.

**Agar Brugiavini**

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


**Omar Bamieh**

(Institute for Economic Policies, Universidad Andres Bello (Santiago, Chile))

**Andrea Cintolesi**

(Department of Economics and Statistics, Bank of Italy - Tuscan Regional Division, Italy)

**Mario Pagliero**

(University of Turin, Collegio Carlo Alberto, and CEPR)



Aggiornamento Workinps papers n. 90 – Febbraio 2025

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

Omar Bamieh* Andrea Cintolesi† Mario Pagliero‡

April 2026

**Abstract**

We investigate the earnings returns to obtaining a license to practice law by leveraging discontinuities at the bar exam. Law graduates who became lawyers after barely passing the bar exam earned 20,000 euros (50%) more in annual wages than they would have in a career outside the legal profession. These returns are not attributable to monopoly rents created by entry barriers. Instead, they represent a compensating differential for the higher earnings risk associated with the legal profession, relative to alternative occupations for law graduates.

Keywords: labor market regulation, occupational licensing, field of study, lawyers.

JEL codes: J08, J44, L84, L50.

*Address: Institute for Economic Policies, Universidad Andres Bello (Santiago, Chile). Contact: Omar Bamieh (omar.bamieh@univie.ac.at).

†Department of Economics and Statistics, Bank of Italy - Tuscan Regional Division, Italy.

‡University of Turin, Collegio Carlo Alberto, and CEPR.

Thanks to Giacomo Caracciolo, Brett A. McCully, Yagan Hazard, Jeanne Lafortune, Giovanni Mastrobuoni, Timea Laura Molnar, Paolo Naticchioni, Michele Pellizzari, Alessandra Voena, Lennart Ziegler, Nadine Ketel, Daniel Garcia, Fabiano Schivardi, Gordon Dahl, Andrés Barrios Fernández, Benjamín Villena Roldán, and seminar at participants at Collegio Carlo Alberto, the University of Linz, the University of Vienna, Andrés Bello University, the 21th Brucchi Luchino Labour Economics Workshop, the 29th Annual Meeting of the Society of Labor Economists (SOLE), the Mario Pagliero Memorial Workshop, the 3th Workshop on Public Polices at the University of Barcelona, the COMPIE 2024 conference, the 37th Annual Conference of the European Society for Population Economics (ESPE), the 2nd Workshop in Policy Evaluation at Universidad de Chile, the Bank of Italy regional divisions seminar, the 38th conference of the Italian Association of Labour Economics (AIEL), the EALE 2023 Conference, and the 1st Vienna Applied Micro Economics Workshop for helpful comments. We also thank the staff of the Visitinps program at Italian Social Security Administration (Istituto nazionale della previdenza sociale, INPS), particularly Monia Monachini.

# 1 Introduction

Occupational licensing regulations require workers to obtain a permit to work, typically issued after passing an exam and meeting specific educational and training requirements. Licensing is a widespread and growing phenomenon in both the US and Europe. While less than 5% of US workers were in occupations covered by licensing laws in the early 1950s, this share had risen to 29% by 2008 (Kleiner and Krueger, 2013). In the EU, licensing currently affects about 22% of workers (Koumenta and Pagliero, 2019).[1] Although professional associations argue that the primary goal of occupational licensing is to protect the public, economists have long held two opposing views on the subject; licensing helps mitigate asymmetric information (Akerlof, 1970, Leland, 1979), but it can also drive up prices by restricting competition (Stigler, 1971).

Estimating the causal effect of licensing on earnings is challenged by two factors. First, individuals self-select into licensed occupations, and are then selected into the profession via a test. As a result, observational differences in earnings between workers in occupations that require a license and those that do not may reflect selection bias. Second, leveraging cross-state differences in licensing requirements and changes over time may capture general equilibrium effects, as licensing policies influence the supply of licensed workers. Thus, finding a credible instrument for occupational licensing has remained an open question since Friedman and Kuznets (1945).

We address this challenge by using a fuzzy regression discontinuity (RD) design that exploits a discontinuity in access to a specific licensed occupation: the legal profession. After completing law school, graduates must pass the bar exam to practice law. We use information about individuals taking the bar exam in Turin (Italy's fourth-largest city). Our empirical design compares law school graduates who score just above and just below the passing threshold. Personal identification numbers allow us to link exam data to labor market outcomes over two decades, capturing individuals in their prime working years.

Although there is no limit on the number of times one can take the bar exam in Italy, individuals who fail on their first attempt are 20 percentage points less likely to obtain a license to practice law at any point in their careers.[2] We find that law school graduates who practice law after passing the bar exam on their first attempt earn 50% more than otherwise equivalent law school graduates who did not become lawyers due to failing the exam on their first attempt. These earnings returns remain stable over the 23-year period following the first attempt at the bar. At a 5% annual discount rate, the present discounted value of practicing law over a 23-year career amounts to 248,479 euros. Our findings indicate substantial returns to entering the legal profession, relative to alternative occupations after completing law school.

We find substantial heterogeneity in this earnings premium across the earnings distribution. Below the median, there are no significant earnings differences, while above the 70th percentile, lawyers earn

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

[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.

considerably more than non-lawyers. This pattern is confirmed by an instrumental variable (IV) quantile regression, which shows that the average 50% earnings return is driven by quantile estimates at the top of the earnings distribution. Similarly, the variance in earnings among law graduates practicing law is significantly higher than that of individuals in other occupations, suggesting that the returns to the license to practice law may represent a compensating differential for the higher risk inherent in the legal profession.

Another explanation for the earnings gap between licensed and unlicensed workers may be the lack of competition resulting from entry barriers, as licensing increases the price of legal services. However, at least three factors suggest that monopoly rents are unlikely to be the main driver of the earnings premium we identify.

First, if monopoly rents were driving the earnings premium, lawyers' revenues should decline as competition increases. Under the assumption that the demand for legal services is independent of the number of lawyers (i.e., no supplier-induced demand), greater competition among lawyers should reduce the rents they can extract from clients, leading to lower prices. A unique feature of our data is that it allows us to observe not only earnings but also revenues, which—in the absence of supplier-induced demand—serve as a reliable proxy for prices. Our results show that as the number of lawyers in a commuting zone (CZ) increases, prices (proxied by revenues) remain unchanged, suggesting that lawyers do not earn monopoly rents.

Second, the number of lawyers in Italy is exceptionally high when compared to other continental European countries of similar size. There are 217,000 lawyers in Italy, compared to 161,000 in Spain, 153,000 in Germany, and 50,000 in France. These figures suggest that entry barriers to the legal profession in Italy are relatively weak. Attesting to this high level of competition, the earning distribution reveals that many lawyers do not earn significantly more than law graduates who do not work as lawyers. This suggests that intense competition among lawyers makes it unlikely that the earnings premium is due to restricted access to the profession.

Third, although the bar exam is selective (with a pass rate of 40%), it does not pose an insurmountable barrier, as candidates can retake it indefinitely (once per year). In our sample, 60% of those who retook the exam after failing initially eventually passed. If significant rents existed, one would expect more candidates to persist in retaking the exam rather than settling for other occupations. These three observations suggest that the earnings premium may stem from factors other than monopoly rents.

Having ruled out monopoly rents as the source of our estimated returns to licensing for lawyers, we examine three alternative mechanisms that could explain these earnings returns: the sheepskin effect associated with holding the license to practice law, differences in working hours, and the earnings risk inherent in the legal profession.

First, in our setting, a subtle distinction arises between holding a license to practice law and actively working as a lawyer. These concepts are closely related, as the license is a prerequisite for practicing law. However, some law graduates do not practice law despite having passed the bar. For these individuals, the license may hold intrinsic value as a signal of quality, operating through a *sheepskin* *effect*.[3] To test for this sheepskin effect, we focus on law school graduates who do not practice law and compare those with and without the license. We find no earnings premium associated with the license to practice law within this subgroup of non-lawyers, suggesting that the earnings premium arises entirely from working as a lawyer rather than merely holding the license. In other words, we find no evidence in support of the sheepskin effect.

Second, lawyers could earn more than non-lawyers 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. While this partly explains the earnings premium that lawyers enjoy, it is insufficient to account for the entire difference.

Finally, the greater income volatility faced by lawyers compared to non-lawyers could partly explain the earnings premium observed in the legal profession. High earnings in law are not guaranteed; only lawyers at the top of the earnings distribution earn significantly more than non-lawyers, while many others do not earn more than law graduates employed in other occupations. More generally, lawyers experience greater earnings volatility than non-lawyers, even in the lower part of the earnings distribution. We find that the standard deviation of lawyers' earnings is twice as high as that of law graduates in other occupations—both across individuals and over time within the same individual. This result is confirmed by our RD estimates. Therefore, the earnings premium enjoyed by lawyers could reflect a compensating differential for the higher income risk inherent in the profession.

We further elaborate on this point by estimating the coefficient of risk aversion required for law school graduates to be indifferent between becoming lawyers—with higher but more volatile expected returns—and pursuing alternative occupations with lower but less volatile earnings. If the estimated coefficient of risk aversion exceeds the values found in the literature, the risk premium could only partly explain the earnings premium we observe. Conversely, if the coefficient falls within a plausible range, it could potentially account for the observed risk premium. Specifically, we assume a constant relative risk aversion (CRRA) utility function in earnings and numerically estimate the coefficient of risk aversion that equalizes the expected utility of law graduates working as lawyers and those employed in other occupations. We find a coefficient of risk aversion of 1.7, which lies within the range of values reported in the literature (Guiso et al., 2005). This result suggests that the higher uncertainty faced by law graduates practicing law, compared to those in other occupations, is a plausible explanation for the earnings premium in the legal profession.

The greater variance in earnings faced by lawyers explains why law graduates do not persist in retaking the bar exam until they pass. Although passing the bar exam can potentially lead to significantly higher earnings, it comes at the cost of increased uncertainty. Contrary to the common belief that licensing necessarily leads to monopoly rents, our findings suggest 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 earn-

[3] The sheepskin effect in economics refers to the phenomenon where individuals who complete a degree earn significantly higher wages than those who have nearly the same amount of education but lack the formal credential. This effect underscores the importance of educational credentials as signals to employers about a candidate's abilities and perseverance.

ings.[4] Previous studies have documented a wage premium associated with occupational licensing, using cross-sectional wage regressions (Ingram, 2019, Kleiner and Krueger, 2010, 2013, Kleiner and Vorotnikov, 2017, Koumenta and Pagliero, 2019) or a worker fixed-effects design (Gittleman et al., 2018, Gittleman and Kleiner, 2016, Zhang, 2019). Compared to these studies, our paper improves identification through an empirical design where individuals are comparable in all dimensions—including human capital—except for their licensing status. Other existing works have explored the relationship between wages and licensing by comparing U.S. states with varying licensing policies in a difference-in-differences framework (Kleiner and Soltas, 2023, Pizzola and Tabarrok, 2017, Timmons and Thornton, 2019). In contrast to our paper, 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 the salary of an individual when they obtain a license, holding all else equal. This is the first estimate of the returns from licensing that holds constant human capital and isolates the pure effect of holding a license from the effect that different licensing regulations have on prices.[5]

Understanding this partial equilibrium effect is relevant for at least two reasons. First, entry barriers—such as the bar exam—may create rents for incumbents due to a lack of competition. In such cases, the earnings premium can stem from the price consumers bear to regulate entry and uphold minimum quality standards; quantifying this markup allows us to assess the potential cost of licensing. This requires an estimate of the partial equilibrium effect of licensing, which captures exclusively the earning differences between individuals in a regulated profession and otherwise equivalent individuals—law graduates with the same ability—working in non-regulated professions. The general equilibrium effect, which has been the main focus of the literature, captures additional factors, such as the influx of less-skilled individuals into the profession, potentially lowering service quality and, in turn, wages. Our findings indicate that while licensing generates a substantial earnings premium, it does not appear to result from reduced competition. Instead, it reflects compensation for the greater earnings risk faced by lawyers.

Second, partial equilibrium estimates can inform policies aimed at reducing wage inequality by assessing the extent that occupational sorting contributes to wage disparities. For instance, in the case of law graduates, we might ask: if a policy could alter the sorting of women out of the legal

[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).

[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.

profession, how much would it reduce the gender pay gap? Understanding this dynamic is crucial for designing policies that address pay disparities by reshaping occupational distribution rather than focusing solely on wage adjustments within occupations.[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 strategy and estimated effects in these papers are similar to ours; however, despite their close connection, a key difference emerges. Earnings differences across fields of study reflect the sum of two effects: (1) the human capital accumulated during a study program and (2) access to occupations that require specific fields of study. The literature on the returns to different fields of study identifies only the combined effect of these two components. In contrast, our setting allows us to isolate the second component—the returns to occupation, holding human capital fixed—since all individuals have completed the same field of study, law, along with the apprenticeship required to take the bar.

Occupational licensing in the legal profession in Italy has been previously studied by Bamieh and Cintolesi (2021), Basso et al. (2021), Pellizzari et al. (2011). Bamieh and Cintolesi (2021) show that familism during the exam accounts for a significant share of the intergenerational transmission of the profession in certain courts in Southern Italy. Pellizzari et al. (2011) find that liberalizing price floors and other anti-competitive norms weakens the selection of lawyers by encouraging outflows from the upper part of the quality distribution. Finally, Basso et al. (2021) show that having relatives in the profession significantly increases the probability of passing the entry exam and boosts earnings—particularly for those who performed poorly in law school.

## 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]

[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.

[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 *trainee-ship* are sometimes used interchangeably. We follow the European

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 a 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. 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 the city of Turin, Italy.[11] This archive preserves records of bar exams held in Turin before 2001. For each exam session, it contains the social security number of each candidate, their written test scores, and, for those passing the written test,

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).

[11] All the documents used to construct the data used in this study can be requested at the National Archives of the city of Turin (Archivio di Stato di Torino), https://archiviodistatotorino.beniculturali.it/.

their oral test score. Our resources allowed us to digitize five exam sessions from 1996 to 2000. We also partially digitized the 1994 and 1995 sessions, but for these years, we only collected data necessary to identify candidates' first bar exam attempt in the 1996–2000 sessions.

The primary challenge in digitizing our data is the social security number of exam takers. For the 1994 and 1995 exam sessions, we digitized only the names and grades, while for the 1996-2000 sessions, we also recorded social security numbers. Therefore, we identify repeaters in 1996-2000 using names and cross-referencing data from 1994 and 1995. Aside from potential errors due to namesakes, we may fail to identify a candidate's first bar attempt if they skipped sessions. For instance, if someone first took the exam in 1993 and retook it in 1996, we would mistakenly classify them as a first-timer. However, our data indicate that failing the first attempt and skipping one or more years before retrying is extremely rare—occurring in less than 1% of cases between 1994 and 2000. This confirms that candidates either give up or continue trying in consecutive years, as the two-year apprenticeship remains valid for only five years. Lastly, the proportion of individuals with identical names who are not the same person 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 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 was 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

[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.

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 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 $\varepsilon_{it}$ is the error term. The coefficients of interest are $\beta_t$, 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 $\beta_t$ 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 $\nu_i$ is the error term.

The parameter $\gamma$ 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

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

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 1997 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.

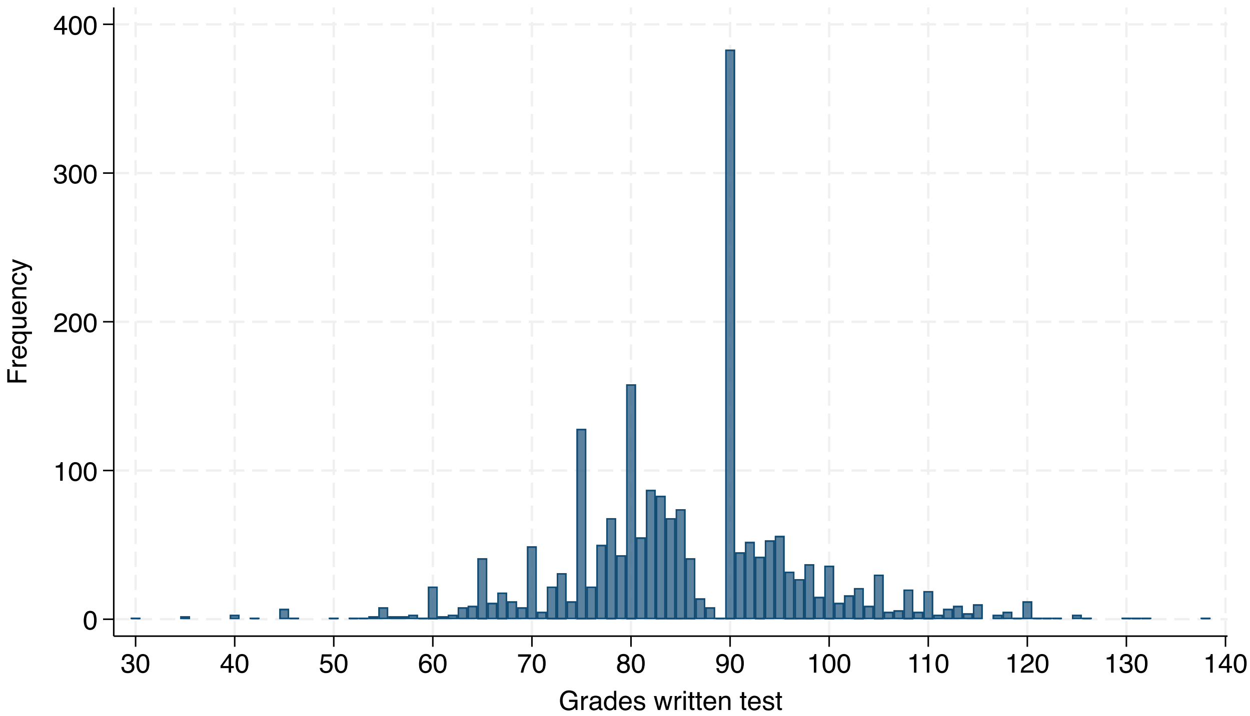
# 5 Testing the validity of the RD design

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

[16] Following the LATE interpretation of instrumental variables, $\gamma$ measures the share of compliers.

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.

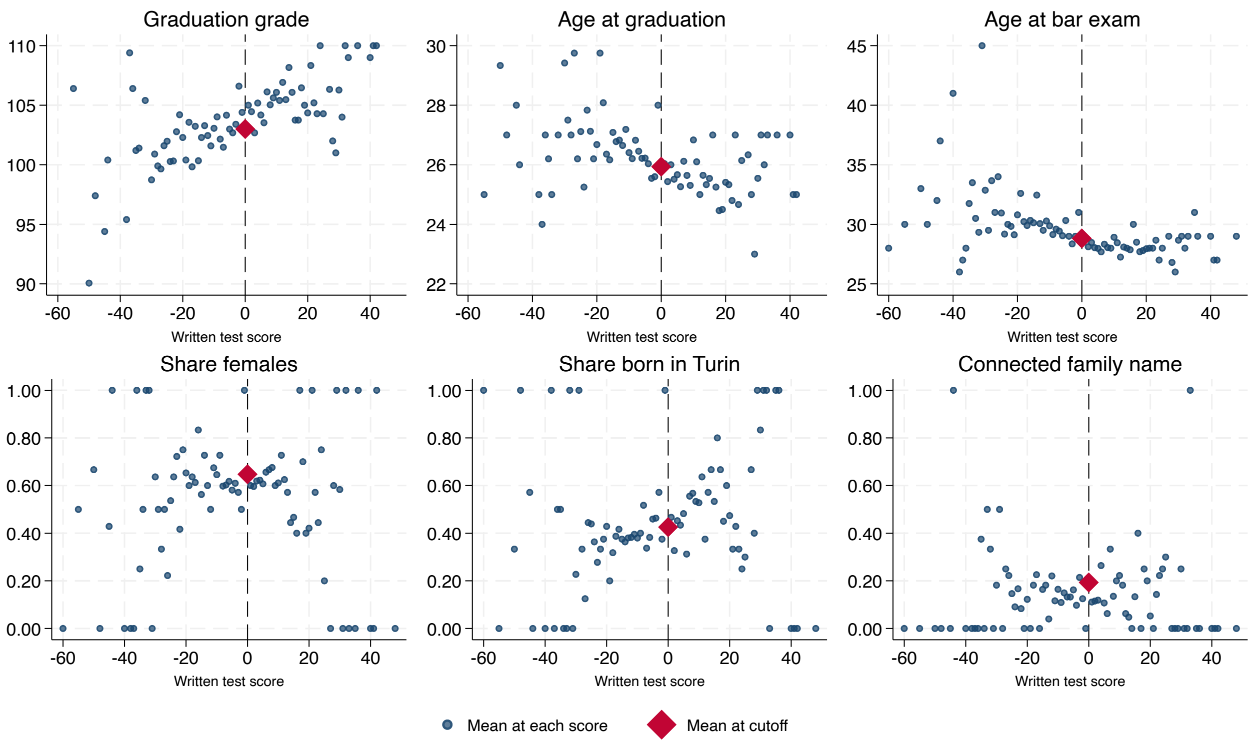
Figure 1: Grades distribution at the bar exam




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 individuals who scored exactly 90 points, as well as those scoring just below the threshold; our point estimates remain stable

[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.

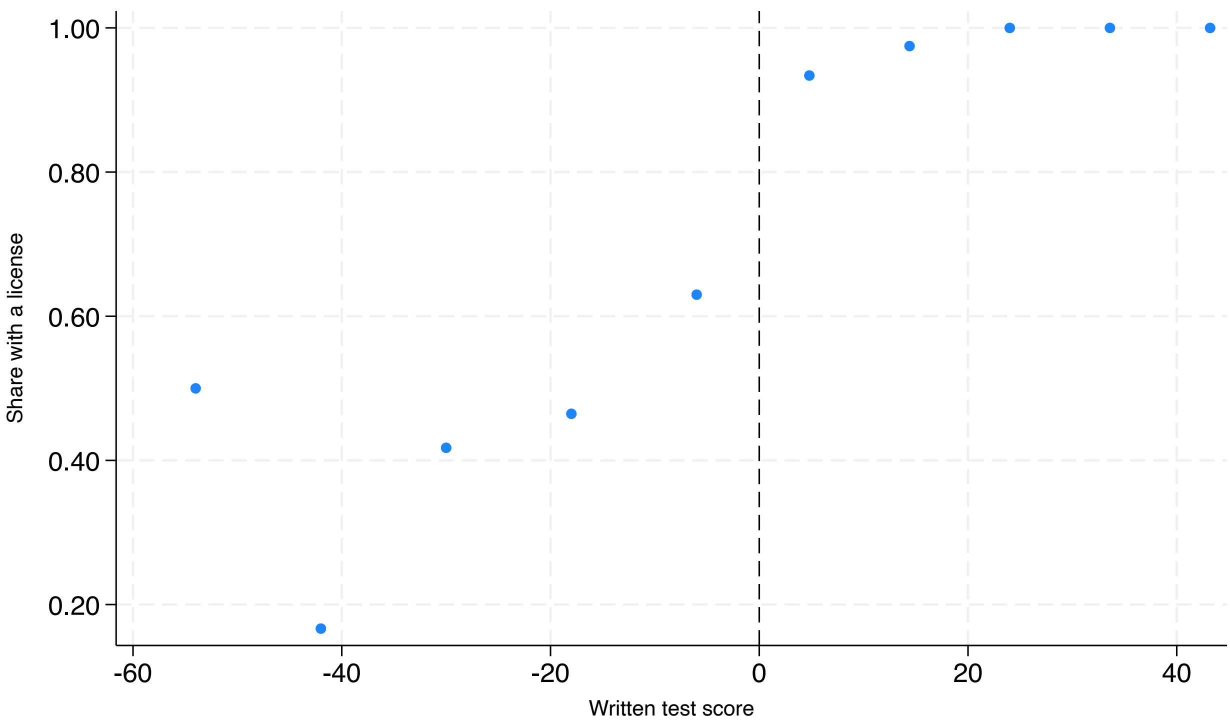
under this specification. Second, we report the average earnings throughout the period considered (as well as at specific years) as a function of written test scores. One potential concern is that candidates who are not rounded up to 90 may differ systematically from those who are. However, Figure A.7 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 $\gamma$ in equation (2). This estimate is robust to alternative bandwidth choices and polynomial specifications in the running variable.

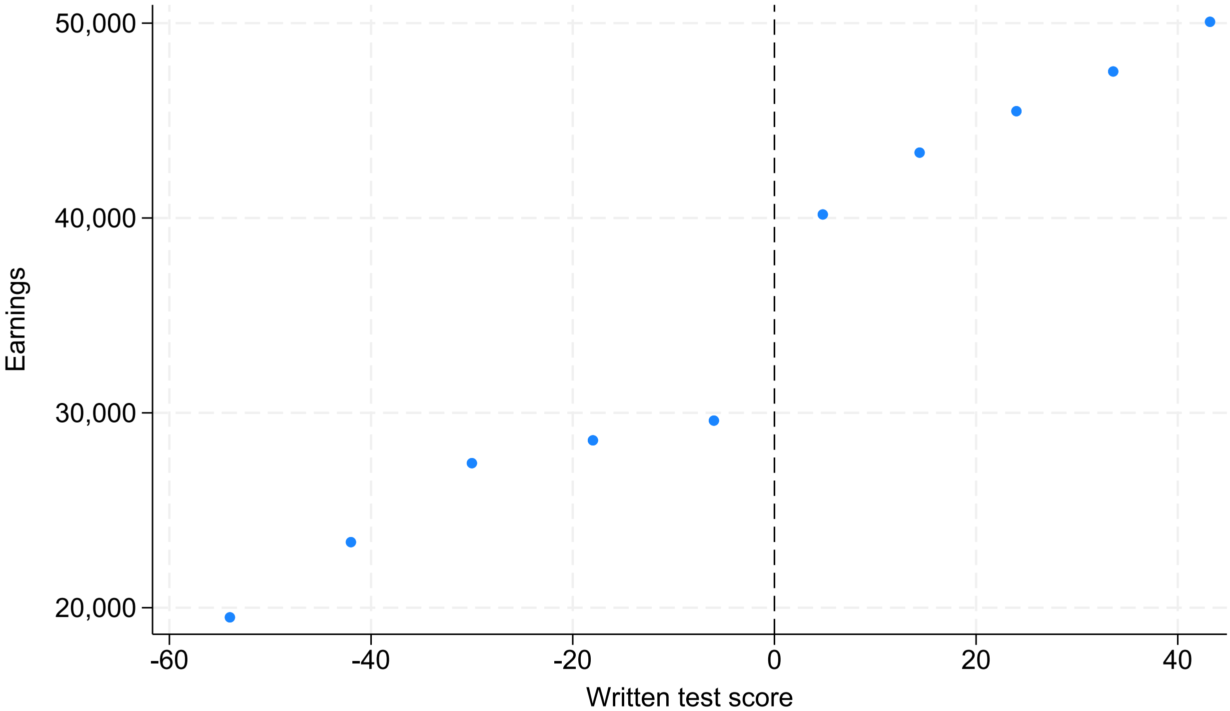
Figure 3: Passing the bar on the first attempt increases the likelihood of becoming a lawyer




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.

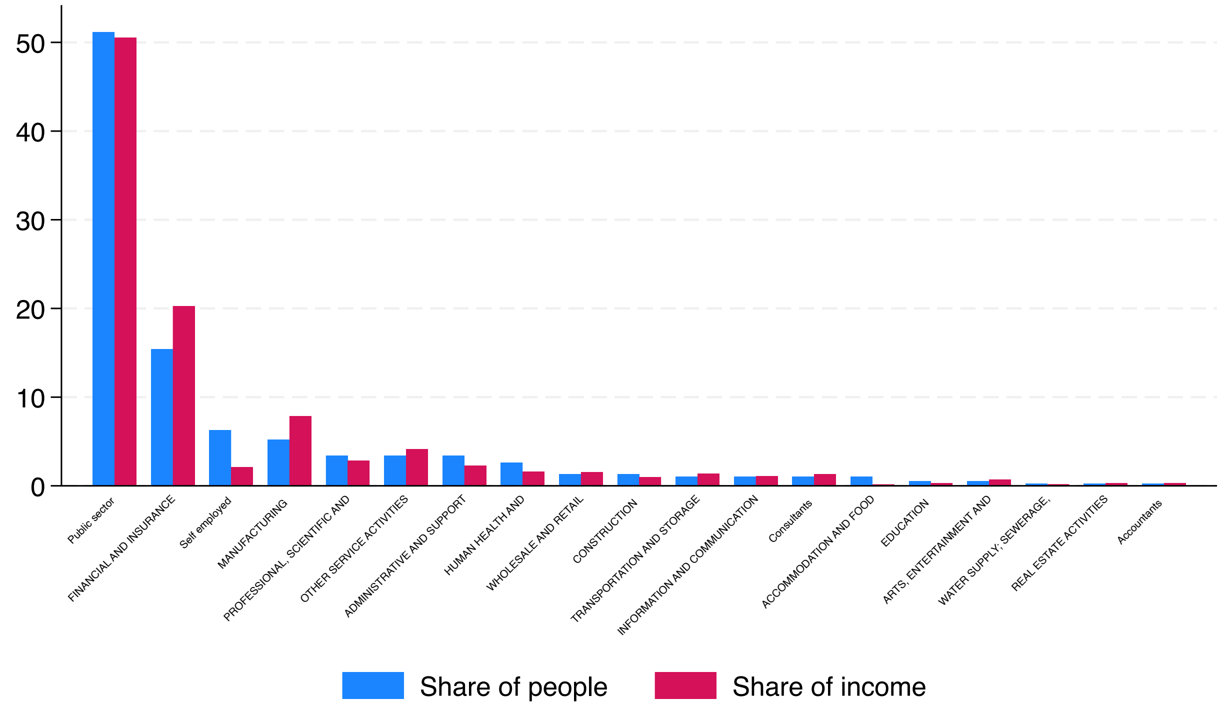
Our IV estimates should be interpreted as local average treatment effects (LATE, Imbens and Angrist (1994)). In our context, this captures the effect of holding a license to practice law for those who became lawyers because they (barely) passed the bar exam on their first attempt—the so-called *compliers*. 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.

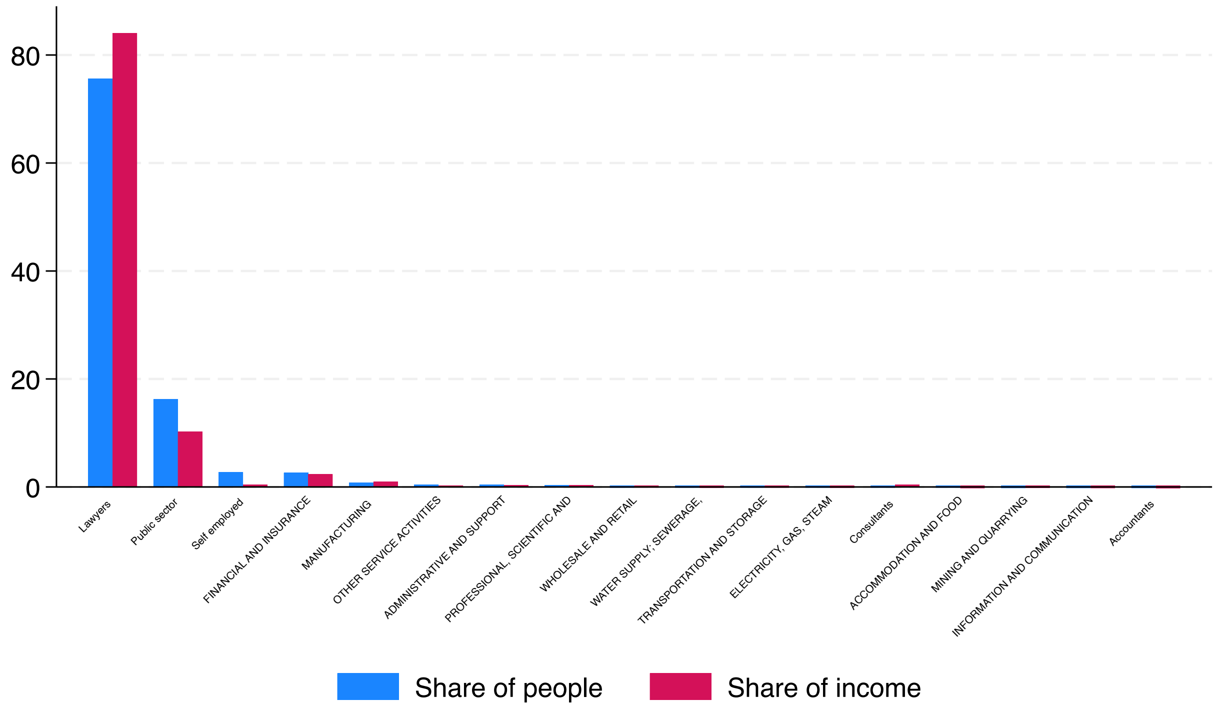
Figure 5 shows that nearly 50% of law graduates who never passed the bar exam are employed in the public sector. In contrast, Figure 6 shows that law graduates holding a license to practice law earn 80% of their income from practicing law. A small share of licensed lawyers earn part of their income in 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

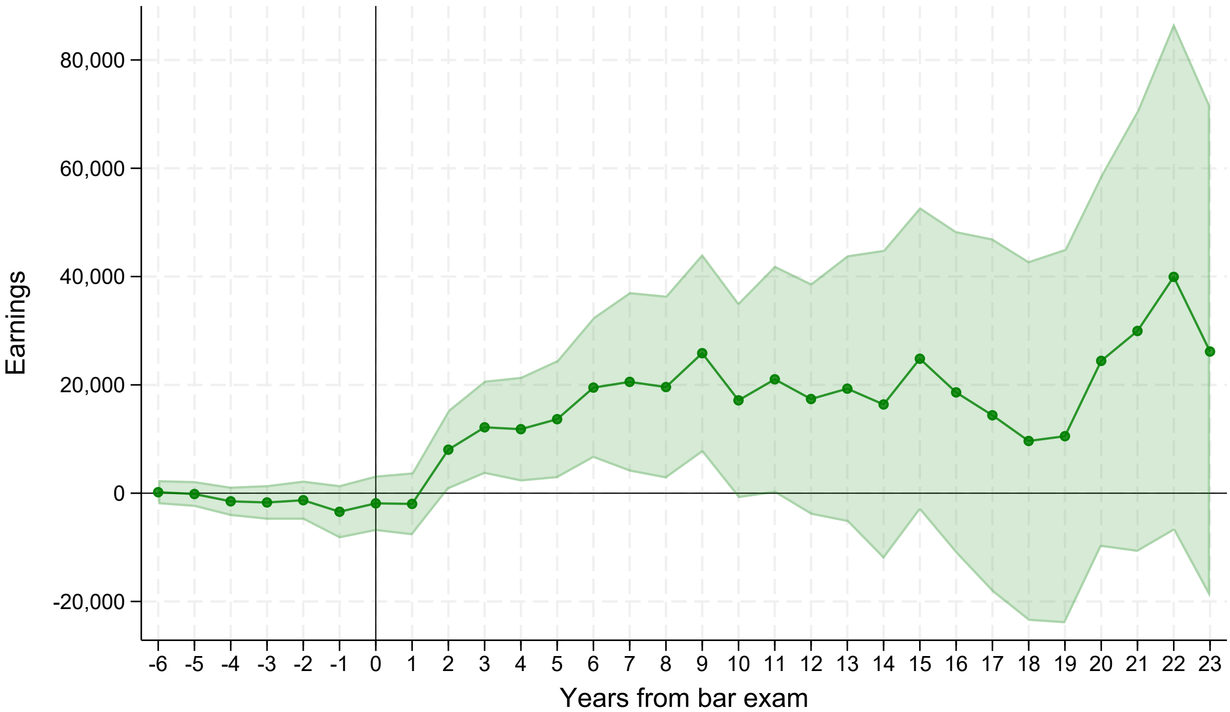



Notes: This figure reports the share of income earned in different sectors, including from practicing law, by individuals holding a license to practice law. It also reports the share of individuals earning any income in each of these sectors. The sector "Lawyers" refers to the registry of lawyers in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

Figure 7 reports the instrumental variable (IV) estimates $\beta_t$ 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 the number of years into the career, law graduates working as lawyers earn between 20% and 70% more than those in other occupations, with an average difference of 50%. Earnings for both lawyers and non-lawyers increase steadily throughout their careers. However, the earnings of lawyers exhibit greater year-to-year variation, confirming that, as self-employed workers, lawyers face greater uncertainty than regular employees, particularly those in the public sector.

To summarize the results presented in Figure 7 and Figure 8, Table 2 reports average instrumental variable estimates for the entire period considered, from the second to the twenty-third year following the bar exam. We also consider different time windows—specifically, the first eleven years and the subsequent eleven years after the bar exam. In absolute terms, the earning returns are smaller in the first eleven years; however, when rescaled by the earnings of non-treated compliers, these returns are comparable across all time periods, averaging approximately 50%.

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 report 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.

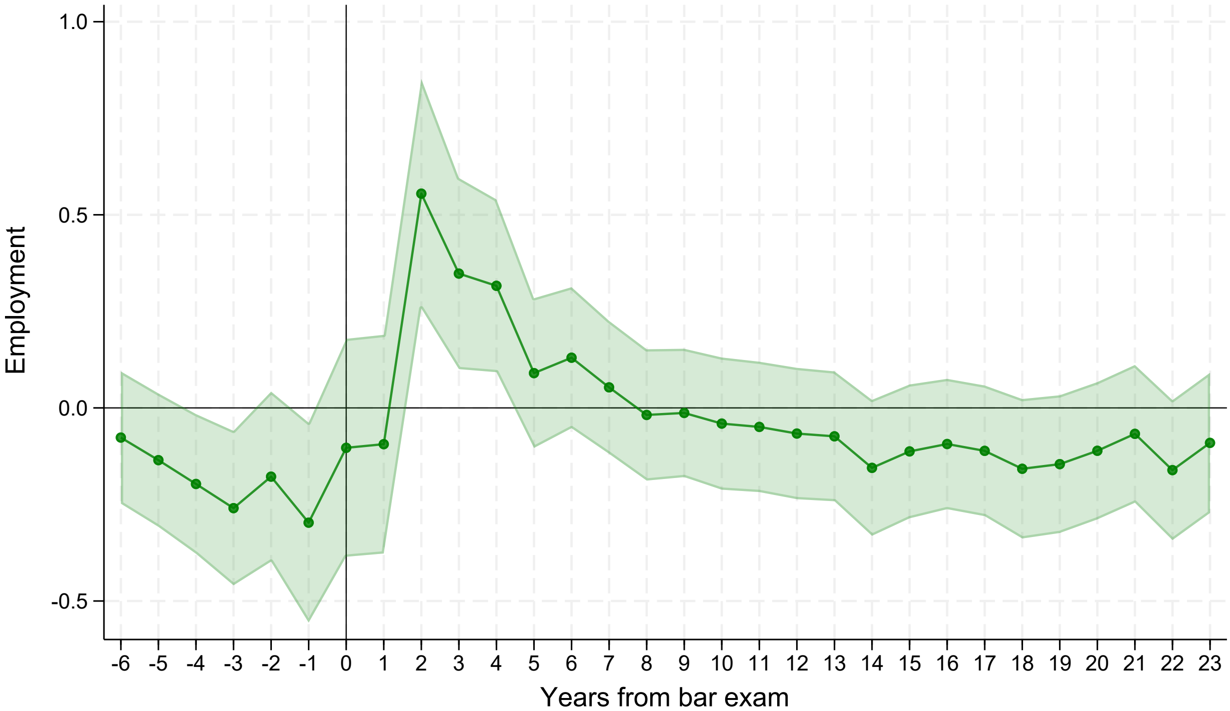
Besides examining the average effects over the 22 years since the exam, another way to summarize the returns to the license to practice law is to consider the present discounted value (PDV) of all the estimated earnings differences reported in Figure 7. Using these estimates, and applying an annual discount rate of 5% over 22 years, the PDV of the earnings return is 248,479 euros.

Given that RD designs are often motivated as local randomization, one might be concerned that using the full sample is inappropriate. Figure A.5 illustrates that the results are not sensitive to the choice of bandwidth, the degree of the polynomial in the running variable, or 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 are also interested in understanding if this earning effect is driven by the extensive margin—that is, by individuals holding a license being more likely to work than those who do not hold a license to practice law. We define employment as having positive earnings in the social security data. Importantly, the earnings of apprentices at law firms often do not appear in social security records, confirming anecdotal evidence that apprentices are paid so poorly that they do not even reach the minimum income required to pay social security contributions. As a result, we might misclassify apprentices as not working. Figure 9 reports the IV estimates when the outcome is an indicator for employment. In the first few years following their first attempt at the bar, law graduates who obtained their licenses by barely passing are significantly more likely to work than those who barely failed. These substantial effects in the early years can be attributed to those narrowly failing the bar and attempting it again in subsequent years, while continuing to work as apprentices—and thus being incorrectly classified as not working. In the later years, however, there is no discernible difference between licensed and non-licensed law graduates. This suggests that those who do not obtain their licenses eventually stop trying and transition into other occupations.

Figure 9: IV estimates employment



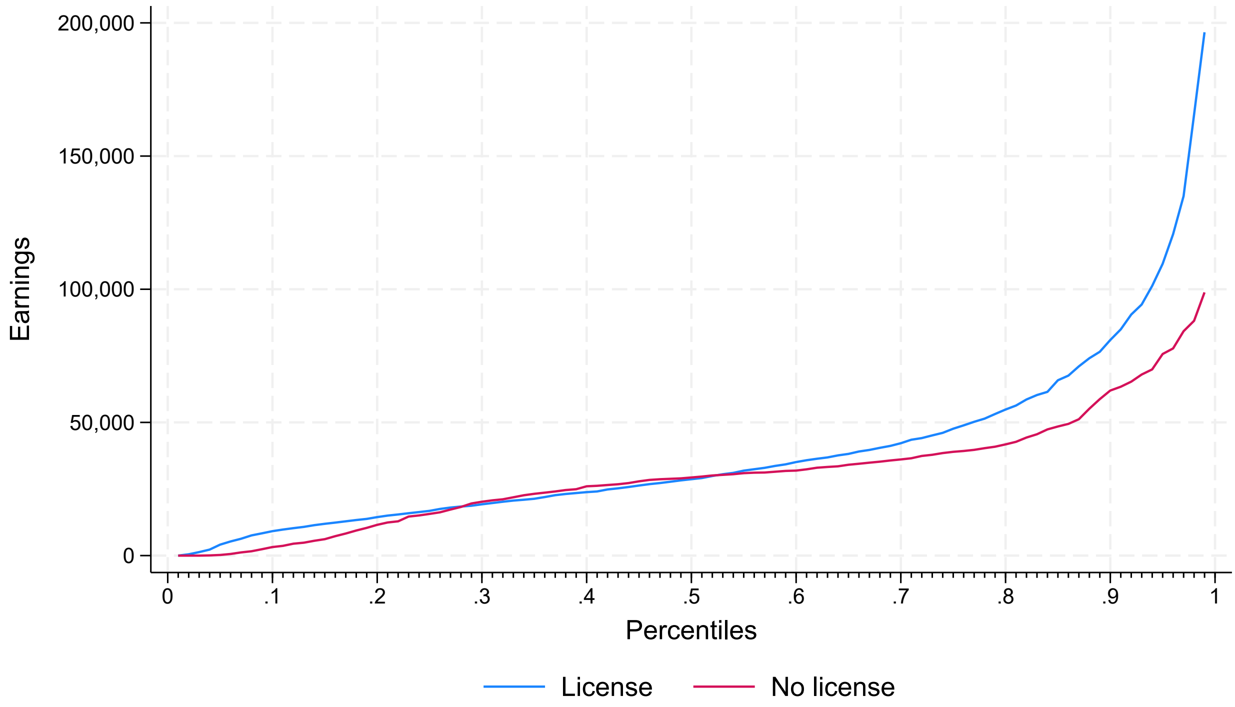

Notes: This figure reports the differences in employment status (defined as having earnings greater than 0), based on IV estimates from equation (1), between individuals with and without a license to practice law.

### 6.1 Returns across the earning distribution

The average effects reported in Figure 7 and Table 2 could mask substantial heterogeneity across the earnings distribution. To investigate this heterogeneity, Figure 10 reports the inverse cumulative distribution function (CDF) of earnings for licensed and non-licensed law graduates.[19] These distributions are similar below the median, while in the upper part, the earnings of law graduates practicing law are substantially higher than those lawyers working in alternative occupations. This suggests that the mean earnings difference between the two groups of law graduates is driven by the upper tail of the earnings distribution: the earnings return to practicing law is driven entirely by top-earning lawyers.

[19]The inverse cumulative distribution function gives the level of earnings corresponding to a given percentile in the earnings distribution. For example, at the 90th percentile, the function returns the earnings level that separates the top 10% of earners from the rest.

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



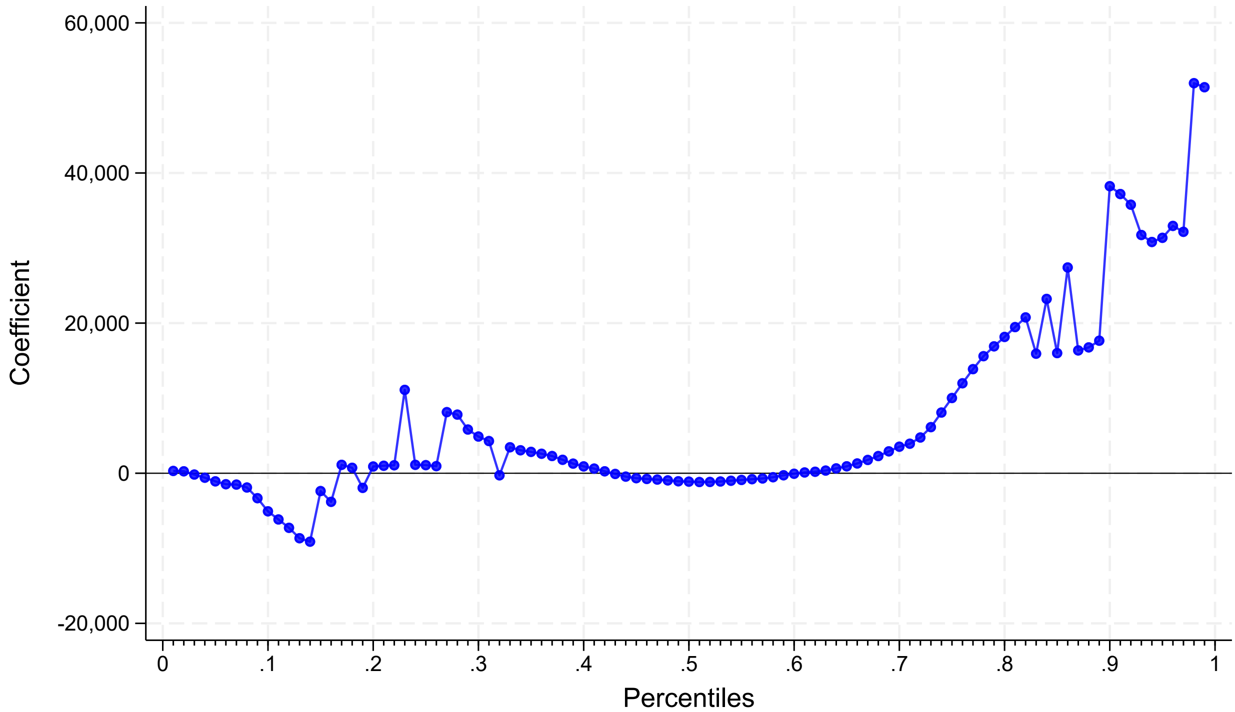

Notes: This figure reports the inverse cumulative distribution functions of annual earnings for licensed (lawyers) and non-licensed (non-lawyers) workers. The sample includes 1,972 observations.

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 10 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 earning 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 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.

[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.

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 workers raises the question of why such a disparity exists. We consider four potential explanations. First, lawyers' high earnings may stem from monopoly rents generated by entry barriers: lawyers face less competition than law graduates who fail to obtain a license and work in occupations without occupational-licensing requirements. Second, non-monetary factors—such as differences in working hours or job flexibility between practicing law and alternative occupations—may compensate for these earnings differences. Third, lawyers' earnings could reflect a compensating differential for higher income risk, as the legal profession is characterized by greater earnings volatility. Finally, the license may produce a sheepskin effect, serving as a signal of quality and allowing licensed individuals to command higher wages, even when their human capital is equivalent to that of unlicensed law graduates—because lawyers and non-lawyers completed the same law degree and passing the bar exam does not increase their human capital.

The following subsections discuss each of these potential mechanisms in detail. From our analysis, we conclude that the most plausible explanation for the large returns to the license to practice law is a compensating differential for the greater income risk that lawyers face in their profession.

### 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, other facts suggest that rents are unlikely to play a major role in the legal profession in our context. First, because law graduates can take the bar exam an unlimited number of times, any potential monopoly rent is likely to be eroded by graduates retaking the exam until they pass. Indeed, many individuals eventually succeed in entering the legal profession, even after failing on their first attempt. In our sample, 60% of candidates who failed the bar on their first attempt subsequently passed in the following years, contributing to a steady influx of new lawyers each year. As a result, the number of lawyers in Italy exceeds that of other European countries; by 2010, Italy had the highest number of registered lawyers among European countries with comparable populations, with 217,000 lawyers, compared to Spain (161,000), Germany (153,000), and France (50,000). These numbers have continued to grow, maintaining a similar gap in the number of lawyers over time.[21] Second, we empirically investigate the presence of monopoly rents by analyzing the relationship between lawyers' revenues and the level of competition within the legal profession. In the following paragraphs, we elaborate further on this last point.

Individual revenues of each lawyer are the product of the market price of legal services and the quantity of legal services that each lawyer provides. In a perfectly competitive market, prices are equal to marginal costs. However, under imperfect competition, there is a wedge between prices and marginal costs, commonly referred to as the markup over marginal costs. By definition, markups decline as competition among lawyers increases, while we assume that aggregate demand for legal services does not. This implies the absence of supplier-induced demand—that is, demand for legal services is not caused by the number of lawyers.

Under these assumptions, if total revenues decrease as the number of lawyers increases, it must be due to a decline in markups. Conversely, if total revenues are insensitive to competition, this would suggest the absence of markups and, hence the absence of monopoly rents. This provides an indirect way to test for the presence of monopoly rents, which we can implement in our setting because we observe lawyers' revenues.

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 $i$:

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

where $D$ does not depend on $N$: the total demand for legal services in the economy, $D$, does not

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

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. $\tau_t$ represents year fixed effects, $\phi_c$ denotes CZ fixed effects, and $t \times \phi_c$ captures CZ-specific linear time trends, which account for variation in the demand for legal services. Our primary focus is on the coefficient $\beta$, 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 $\beta$ 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

[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.

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 reflect 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 aspect making the legal profession different from alternative occupations for law graduates; lawyers typically work longer hours and have less flexibility.

Social security data do not contain information on working hours, which prevents us from replicating our IV estimates using hours as an outcome. Instead, we use microdata from the Labor Force Survey (LFS)[24], where workers self-report their education and the number of hours worked during the week preceding the interview. We focus on law graduates in Piedmont, the region where the city of Turin is located. The survey shows that law graduates practicing law work 13% more hours than law graduates employed in other occupations. Lawyers are self-employed and do not benefit from paid

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

leave, which may lead them to take fewer vacations and, consequently, work more hours. However, interviews for the LFS take place outside the holiday season, so this potential holiday bias is not a concern. These results indicate that part of the earnings gap between lawyers and non-lawyers stems from differences in hours worked. Nevertheless, this 13% difference in hours cannot account for the 50% earnings premium we find, suggesting that other factors must contribute to it.

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 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 suggest 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 $\rho$ 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 $\rho$ such that the expected utilities of the two groups are equal.

We estimate a coefficient of risk aversion ($\rho$) 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.

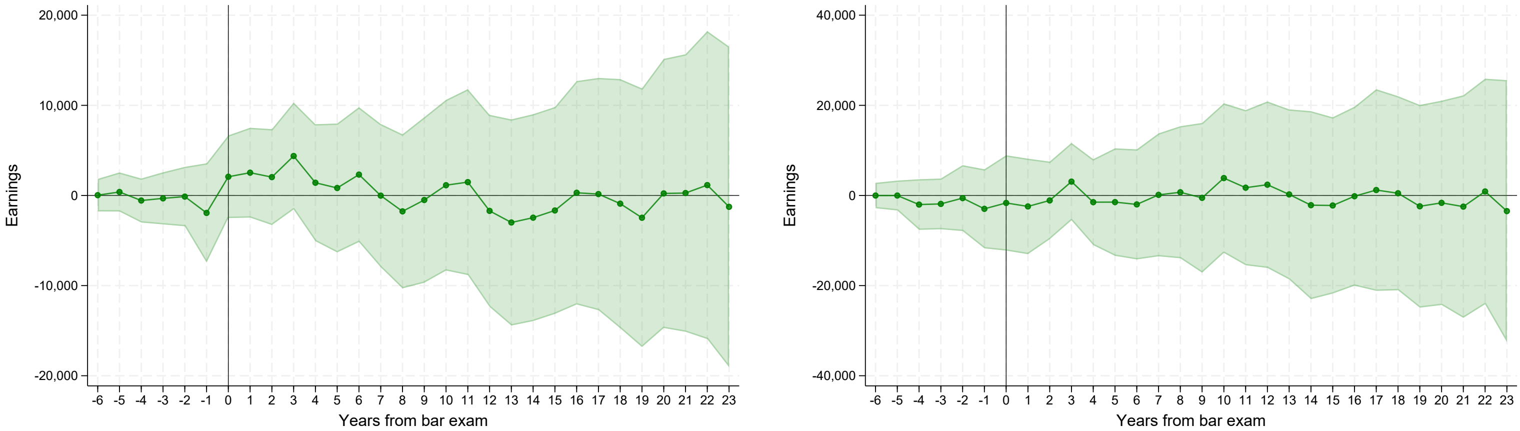
Finding a reasonable coefficient of risk aversion also explains why we observe a first-stage: not everyone who barely fails the bar exams tries again later and eventually succeeds. Consider a recent law graduates who has just missed the passing threshold by a few points; their choice is to wait and try 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 concentrate at the top of the distributions, 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 earning 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 indicate the absence of a sheepskin effect: the only value of the license to practice law lies in providing access to the regulated profession itself. Outside the legal profession, holding a license to practice law confers no additional economic benefit. This rules out the possibility that the returns to the license arise from a sheepskin effect and strengthens our interpretation that the earnings premium reflect something inherent to the legal profession, such as a compensating differential for the risk associated with practicing law.

## 8 Conclusions

Quantifying the returns to professional licensing has long been a central topic in labor economics and has become even more salient in light of the substantial rise in occupational licensing requirements in recent decades.

The literature on occupational licensing has compared the earnings of workers in licensed and non-licensed occupations, controlling for workers' characteristics. Other studies have used a difference-in-differences approach, comparing U.S. states with varying licensing requirements over time; however, this approach captures general equilibrium effects stemming from the labor supply shock induced by differences in the stringency of occupational-licensing requirements. The economic education literature

[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.

has leveraged admission cutoffs and lotteries for university programs to examine the returns to different fields of study; yet, these studies capture both the impact of access to a regulated occupation and the effect of 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 law graduates who marginally pass the bar exam on their first attempt earn 50% more than those who marginally fail. This earnings premium remains stable throughout the 23 years following their first attempt at the bar. We also find that the observed effect is primarily driven by individuals at the top of the earnings distribution. There are several potential mechanisms that could explain the earnings difference between law graduates practicing law and those working in other occupations. Understanding whether this premium reflects limited competition is crucial for evaluating the potential social cost of occupational licensing. Our empirical analysis rules out the possibility that the salary gap is due to a lack of competition. Instead, our results suggest that the higher income uncertainty faced by lawyers relative to non-lawyers is the most plausible explanation for the estimated returns to licensing—indicating that the earnings premium enjoyed by lawyers is a compensating differential for the greater risk associated with the legal profession.

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. However, care should be taken in generalizing our results, as monopoly rents can vary across regulated occupations and countries, as shown by Ketel et al. (2016) for Dutch doctors and dentists.

# References


Abadie, A. (2003). Semiparametric instrumental variable estimation of treatment response models. *Journal of econometrics 113*(2), 231–263.

Akerlof, G. (1970). The market for lemons: Quality uncertainty and the market mechanism. *Quarterly Journal of Economics 89*, 488–500.

Almond, D. and J. J. Doyle Jr (2011). After midnight: A regression discontinuity design in length of postpartum hospital stays. *American Economic Journal: Economic Policy 3*(3), 1–34.

Anderson, D. M., R. Brown, K. K. Charles, and D. I. Rees (2020). Occupational licensing and maternal health: Evidence from early midwifery laws. *Journal of Political Economy 128*(11), 4337–4383.

Bamieh, O. and A. Cintolesi (2021). Intergenerational transmission in regulated professions and the role of familism. *Journal of Economic Behavior & Organization 192*, 857–879.

Basso, G., E. Brandimarti, M. Pellizzari, and G. Pica (2021). Quality and selection in regulated professions.

Bhai, M. and D. T. Mitchell (2022). The effects of occupational licensing reform for nurse practitioners on children's health. *Southern Economic Journal*.

Blair, P. Q. and B. W. Chung (2019). How much of barrier to entry is occupational licensing? *British Journal of Industrial Relations 57*(4), 919–943.

Blair, P. Q. and B. W. Chung (2022). Job market signaling through occupational licensing. *Review of Economics and Statistics*, 1–45.

Blair, P. Q. and M. Fisher (2022). Does occupational licensing reduce value creation on digital platforms? Technical report, National Bureau of Economic Research.

Bleemer, Z. and A. Mehta (2022). Will studying economics make you rich? a regression discontinuity analysis of the returns to college major. *American Economic Journal: Applied Economics 14*(2), 1–22.

Bordignon, M., T. Nannicini, and G. Tabellini (2016). Moderating political extremism: single round versus runoff elections under plurality rule. *American Economic Review 106*(8), 2349–2370.

Bowblis, J. R. and A. C. Smith (2021). Occupational licensing of social services and nursing home quality: A regression discontinuity approach. *ILR Review 74*(1), 199–223.

Buonanno, P. and M. Pagliero (2018). *Occupational Licensing, Labor Mobility, and the Unfairness of Entry Standards*. Centre for Economic Policy Research.

Canaan, S. and P. Mouganie (2018). Returns to education quality for low-skilled students: Evidence from a discontinuity. *Journal of Labor Economics 36*(2), 395–436.

Cassidy, H. and T. Dacass (2021). Occupational licensing and immigrants. *The Journal of Law and Economics 64*(1), 1–28.

Dahl, G., D.-O. Rooth, and A. Stenberg (2020). High school majors, comparative (dis) advantage, and future earnings. Technical report, National Bureau of Economic Research.

Dahl, G. B., K. V. Løken, and M. Mogstad (2014). Peer effects in program participation. *American Economic Review 104*(7), 2049–2074.

Daly, M., M. F. Jensen, and D. le Maire (2022). University admission and the similarity of fields of study: Effects on earnings and skill usage. *Labour Economics 75*, 102118.

Farronato, C., A. Fradkin, B. J. Larsen, and E. Brynjolfsson (2024). Consumer protection in an online world: An analysis of occupational licensing. *American Economic Journal: Applied Economics 16*(3), 549–579.

Friedman, M. and S. Kuznets (1945). *Income from independent professional practice.* Number frie54-1. National Bureau of Economic Research.

Gittleman, M., M. A. Klee, and M. M. Kleiner (2018). Analyzing the labor market outcomes of occupational licensing. *Industrial Relations: A Journal of Economy and Society 57*(1), 57–100.

Gittleman, M. and M. M. Kleiner (2016). Wage effects of unionization and occupational licensing coverage in the united states. *ILR Review 69*(1), 142–172.

Goldin, C. (2014). A grand gender convergence: Its last chapter. *American economic review 104*(4), 1091–1119.

Grosz, M. (2020). The returns to a large community college program: Evidence from admissions lotteries. *American Economic Journal: Economic Policy 12*(1), 226–253.

Guiso, L., L. Pistaferri, and F. Schivardi (2005). Insurance within the firm. *Journal of Political Economy 113*(5), 1054–1087.

Hastings, J. S., C. A. Neilson, and S. D. Zimmerman (2013). Are some degrees worth more than others? evidence from college admission cutoffs in chile. Technical report, National Bureau of Economic Research.

Imbens, G. W. and J. D. Angrist (1994). Identification and estimation of local average treatment effects. *Econometrica 62*(2), 467–475.

Imbens, G. W. and T. Lemieux (2008). Regression discontinuity designs: A guide to practice. *Journal of econometrics 142*(2), 615–635.

Ingram, S. J. (2019). Occupational licensing and the earnings premium in the united states: updated evidence from the current population survey. *British Journal of Industrial Relations 57*(4), 732–763.

Jardina, A., P. Q. Blair, J. Heck, and P. Debroy (2023). The limits of educational attainment in mitigating occupational segregation between black and white workers. Technical report, National Bureau of Economic Research.

Jepsen, C., P. Mueser, and K. Troske (2016). Labor market returns to the ged using regression discontinuity analysis. *Journal of Political Economy 124*(3), 621–649.

Johnson, J. E. and M. M. Kleiner (2020). Is occupational licensing a barrier to interstate migration? *American Economic Journal: Economic Policy 12*(3), 347–73.

Kaplan, D. M. and Y. Sun (2017). Smoothed estimating equations for instrumental variables quantile regression. *Econometric Theory 33*(1), 105–157.

Kawaguchi, D., T. Murao, and R. Kambayashi (2014). Incidence of strict quality standards: Protection of consumers or windfall for professionals? *The Journal of Law and Economics 57*(1), 195–224.

Ketel, N., E. Leuven, H. Oosterbeek, and B. van der Klaauw (2016). The returns to medical school: Evidence from admission lotteries. *American Economic Journal: Applied Economics 8*(2), 225–254.

Ketel, N., E. Leuven, H. Oosterbeek, and B. Van Der Klaauw (2019). Do dutch dentists extract monopoly rents? *Journal of health economics 63*, 145–158.

Kirkeboen, L. J., E. Leuven, and M. Mogstad (2016). Field of study, earnings, and self-selection. *The Quarterly Journal of Economics 131*(3), 1057–1111.

Kleiner, M. M. and A. B. Krueger (2010). The prevalence and effects of occupational licensing. *British Journal of Industrial Relations 48*(4), 676–687.

Kleiner, M. M. and A. B. Krueger (2013). Analyzing the extent and influence of occupational licensing on the labor market. *Journal of Labor Economics 31*(2), S173–S202.

Kleiner, M. M. and R. T. Kudrle (2000). Does regulation affect economic outcomes? the case of dentistry. *The Journal of Law and Economics 43*(2), 547–582.

Kleiner, M. M., A. Marier, K. W. Park, and C. Wing (2016). Relaxing occupational licensing requirements: Analyzing wages and prices for a medical service. *The Journal of Law and Economics 59*(2), 261–291.

Kleiner, M. M. and E. J. Soltas (2023). A welfare analysis of occupational licensing in us states. *Review of Economic Studies 90*(5), 2481–2516.

Kleiner, M. M. and E. Vorotnikov (2017). Analyzing occupational licensing among the states. *Journal of Regulatory Economics 52*, 132–158.

Koumenta, M. and M. Pagliero (2019). Occupational regulation in the european union: coverage and wage effects. *British Journal of Industrial Relations 57*(4), 818–849.

Lee, D. S. and T. Lemieux (2010). Regression discontinuity designs in economics. *Journal of economic literature 48*(2), 281–355.

Leland, H. E. (1979). Quacks, lemons and licensing: A theory of minimum quality standards. *Journal of Political Economy 87*(6), 1328–1346.

Mocetti, S., L. Rizzica, and G. Roma (2021). Regulated occupations in italy: Extent and labour market effects. *International Review of Law and Economics 66*, 105987.

Pagliero, M. (2011). What is the objective of professional licensing? evidence from the us market for lawyers. *International journal of industrial organization 29*(4), 473–483.

Pellizzari, M., G. Pica, et al. (2011). Liberalizing professional services: Evidence from italian lawyers. *U. of Bocconi working paper (May 2011).*

Pizzola, B. and A. Tabarrok (2017). Occupational licensing causes a wage premium: Evidence from a natural experiment in colorado's funeral services industry. *International Review of Law and Economics 50*, 50–59.

Plemmons, A. (2022). Occupational licensing's effects on firm location and employment in the united states. *British Journal of Industrial Relations 60*(4), 735–760.

Stigler, G. J. (1971). The theory of economic regulation. *The Bell Journal of Economics and Management Science 2*(1), 3–21.

Thornton, R. J. and E. J. Timmons (2013). Licensing one of the world's oldest professions: Massage. *The Journal of Law and Economics 56*(2), 371–388.

Timmons, E. J. and R. J. Thornton (2019). There and back again: The de-licensing and re-licensing of barbers in alabama. *British Journal of Industrial Relations 57*(4), 764–790.

Zhang, T. (2019). Effects of occupational licensing and unions on labour market earnings in canada. *British Journal of Industrial Relations 57*(4), 791–817.

Zinovyeva, N. and M. Tverdostup (2021). Gender identity, coworking spouses, and relative income within households. *American Economic Journal: Applied Economics 13*(4), 258–84.

## 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:

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

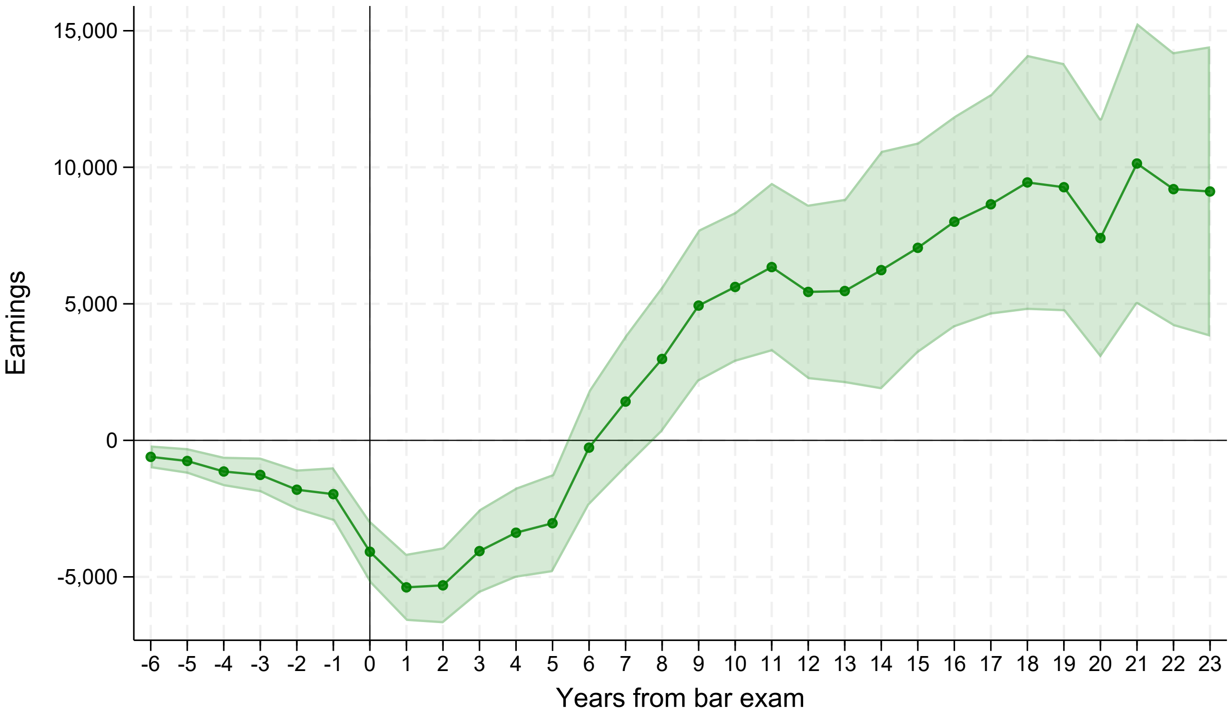
where each column refer to a separate regression with different outcomes denoted by $Y_i$; $P_i$ is a dummy variable taking value 1 if the individual scores 90 or more at the his or her first attempt at the bar exam; and $f(g_i)$ is a polynomial in the grades at the first bar exam; $X_i$ is a vector of controls including year of the exam fixed-effects, gender and the year of birth of the individual; $\nu_i$ 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 | | | ✓ |

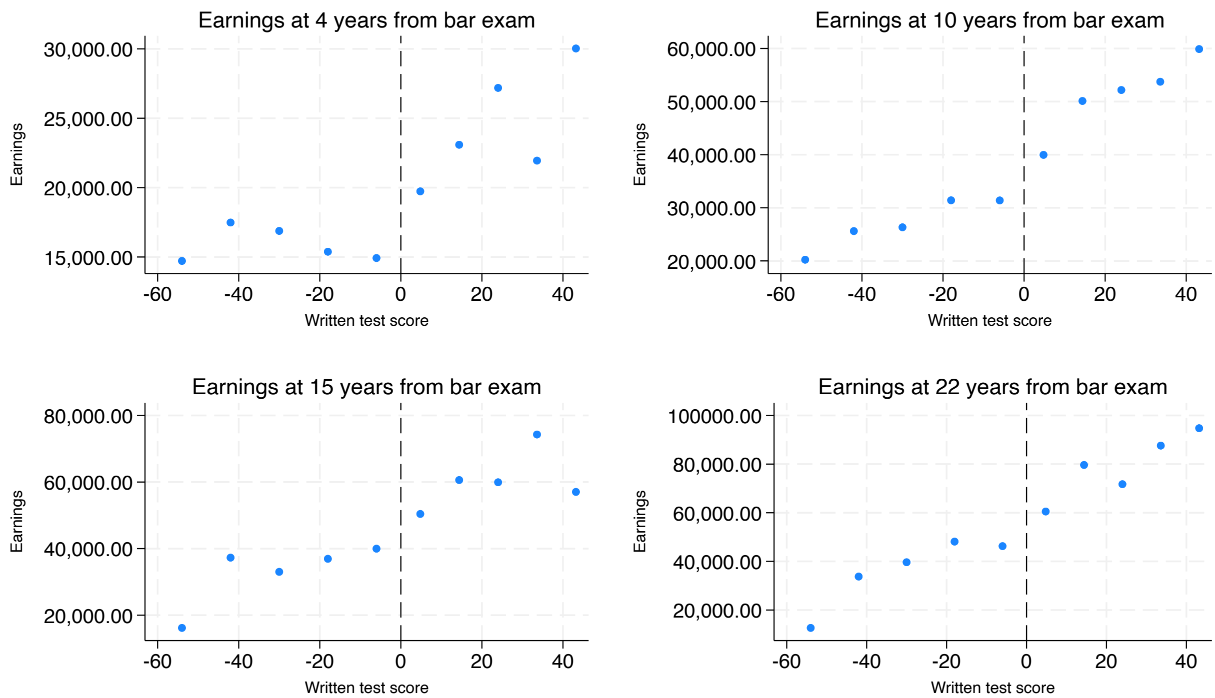
Note: 1,972 observations. The average earnings of law graduates without a license are 30,655.

Figure A.1: OLS earnings



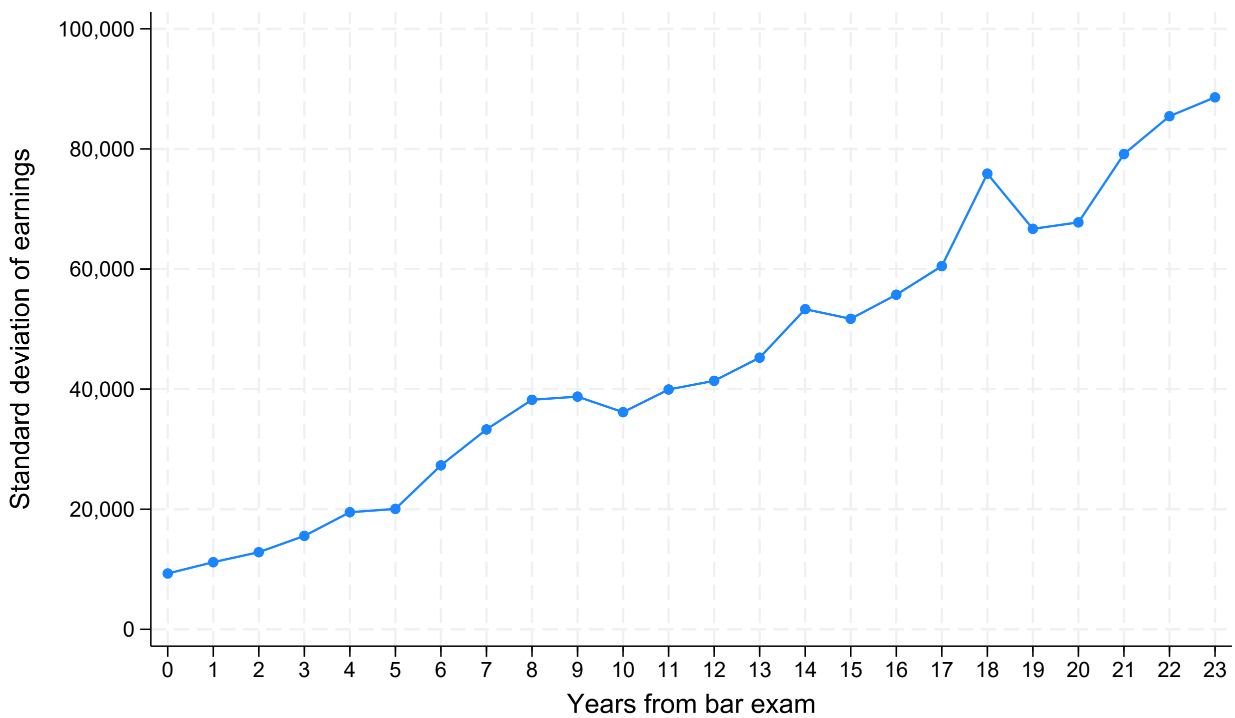

Notes: controls include gender and year of birth dummies.

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



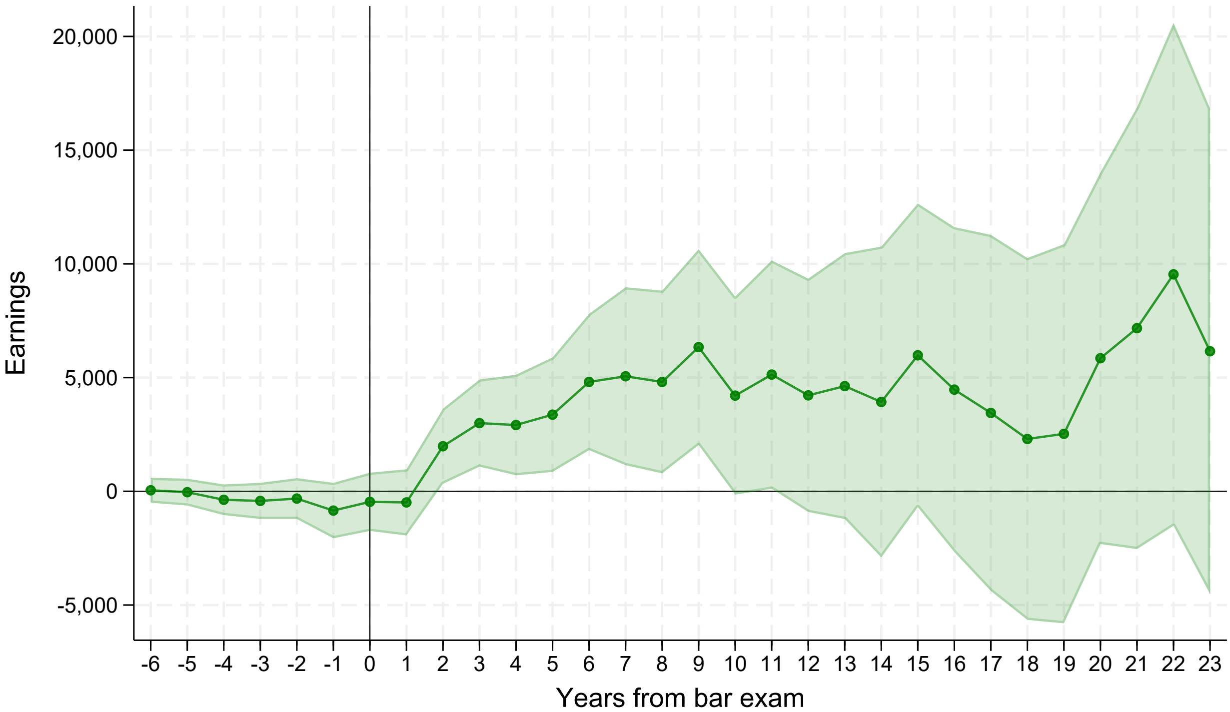

Notes: The figure shows that passing the bar exam on the first attempt increases earnings. Each dot refers to the earnings of individuals within an interval of the grade received at the first bar exam. These intervals contain roughly the same number of observations.

Figure A.3: Standard deviation of earnings



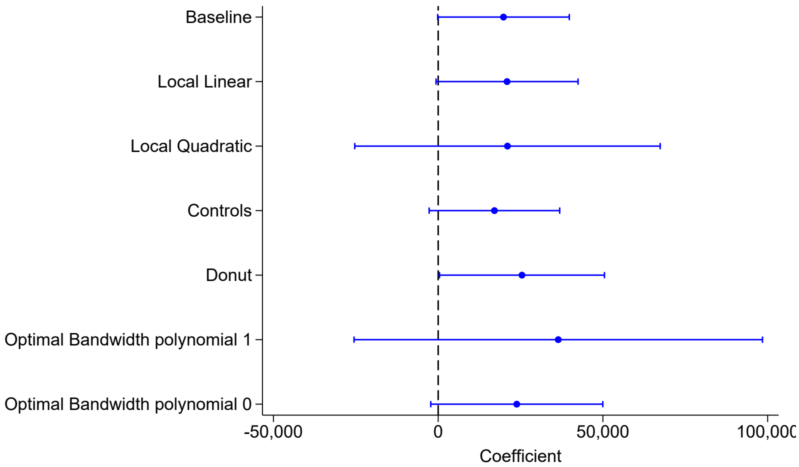

Notes: the figure reports the standard deviation of earnings in each year form the first bar exam.

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

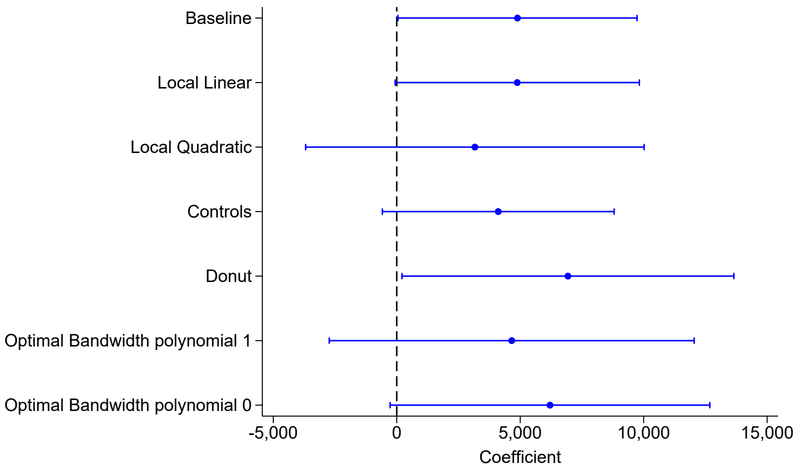



Notes: Full sample and linear polynomial.

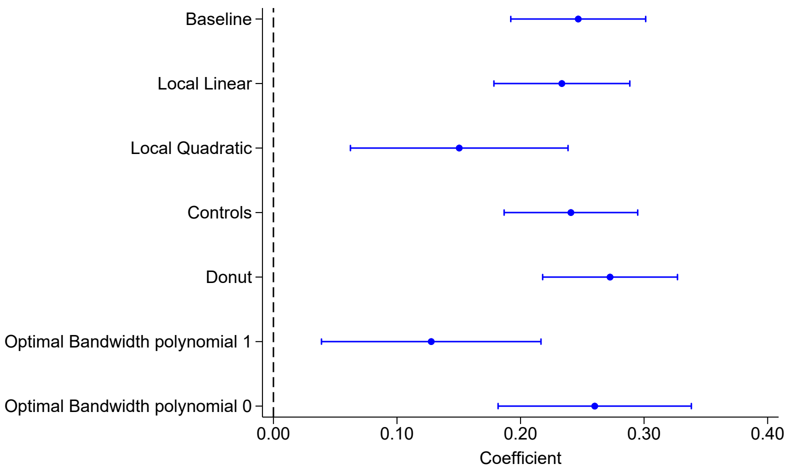
Figure A.5: Robustness to bandwidth choice and polynomials




(a) Instrumental variable estimates



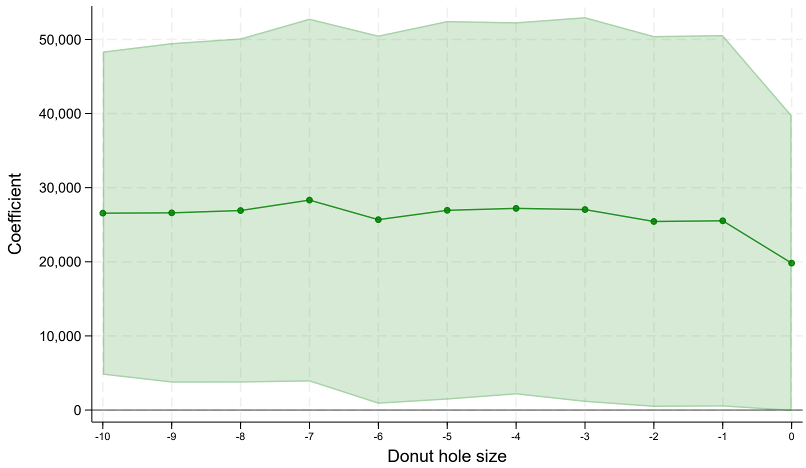

(b) Reduced form estimates



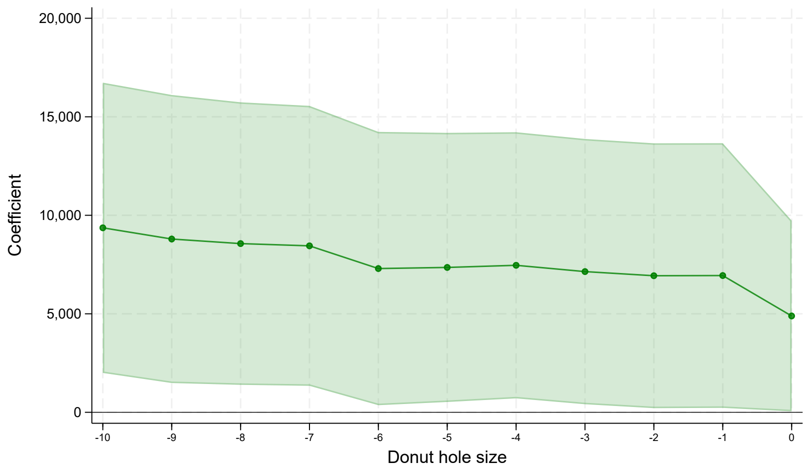

(c) First-stage estimates

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.

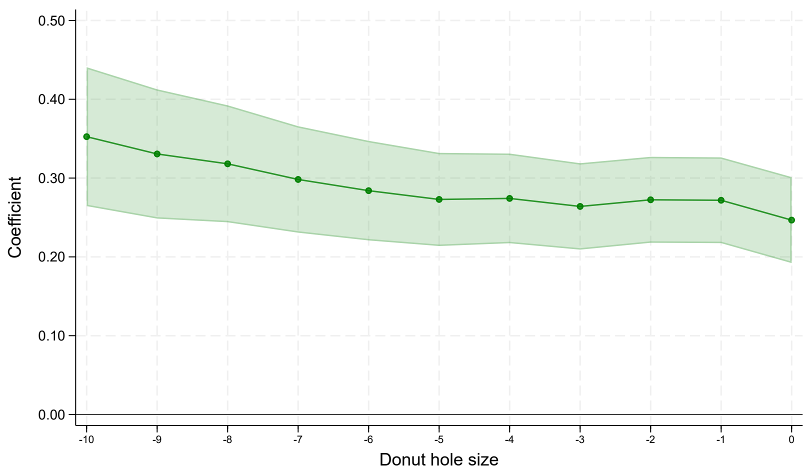
Figure A.6: Donut hole—second to twenty-third years after the bar




(a) Instrumental variable estimates

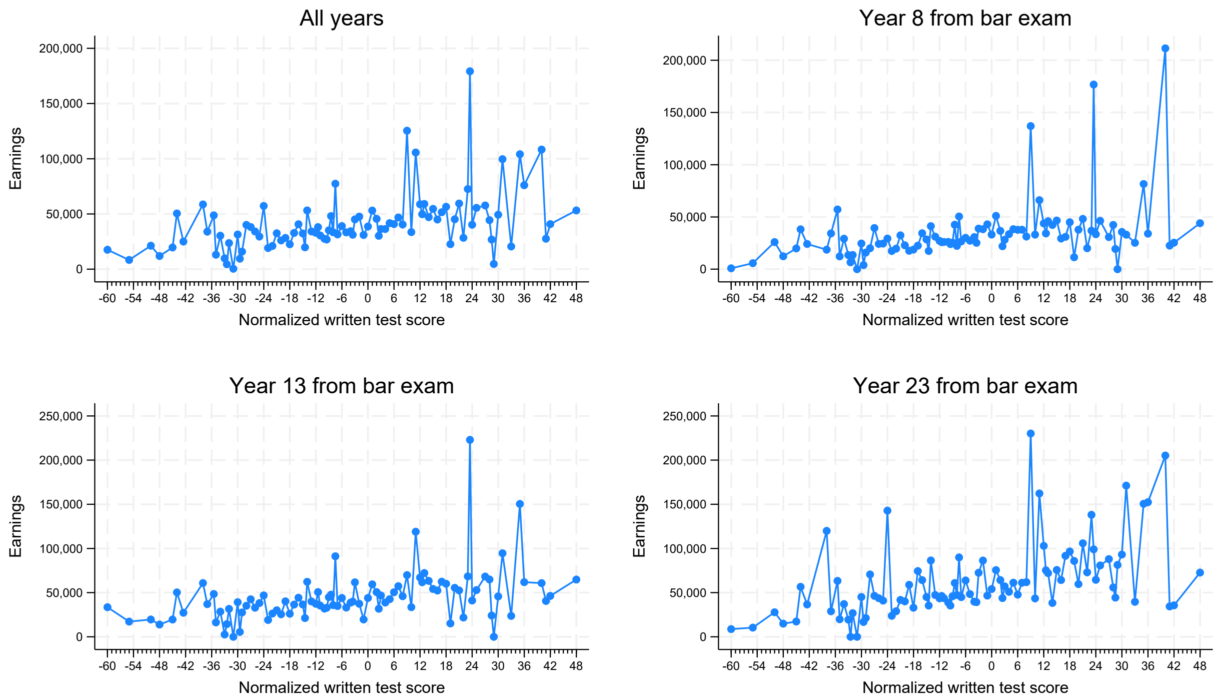



(b) Reduced form estimates




(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.