LCERPA Working paper No. 2024-1

Updated July 2024

# Signaling Worker Quality in a Developing Country: Lessons from a Certification Program

M. Antonella Mancino
Wilfrid Laurier University

Leonardo Fabio Morales
Central Bank of Colombia

Diego F. Salazar
National University of Singapore

# Signaling Worker Quality in a Developing Country: Lessons from a Certification Program*

M. Antonella Mancino[1], Leonardo Fabio Morales[2], and Diego F. Salazar[3]

[1]Wilfrid Laurier University
[2]Central Bank of Colombia
[3]National University of Singapore

July 26, 2024

**Abstract**

We evaluate the returns of signaling occupation-specific skills for experienced workers using unique administrative data from a nationwide certification program in Colombia. The program certifies skills and issues one of three certificates: basic, intermediate, and advanced. We use regression discontinuity methods to compare workers' earnings around certificate-assignment thresholds. Signaling advanced occupation-specific skills yields significant returns: 9.7%, on average, within two years of certification. Instead, we find no statistically significant effects from signaling basic or in-

*M. Antonella Mancino: amancino@wlu.ca. Leonardo Fabio Morales: lmoralzu@banrep.gov.co. Diego F. Salazar: diegosalazar.net@gmail.com. We want to thank Lance Lochner, Juan Morales, Xiaoxiao Li, Hong Long, Jungho Lee, Sunha Myong, Christine Ho, Siddharth George, Louis-Pierre Lepage, Ingrid Koch, Elizabeth U. Cascio, Jonathan Guryan, and Fabian Lange for helpful comments and suggestions, as well as seminar participants at Wilfrid Laurier University, Singapore Management University, National University of Singapore, Central Bank of Colombia, the 2022 LAMES-LACEA Annual Meeting, the 2023 Midwest Economics Association Annual Meeting, the 2023 RIDGE Forum Labor Workshop, the 2023 SOLE Annual Meeting, AMES 2023, ESAM 2023, and the 2023 EEA-ESEM Congress. We are grateful to William Orozco Daza and Mario Javier Rincón (SENA) for their invaluable help with the data. For excellent research assistance, we thank Linda Nataly Corredor, Felipe Gaviria, and Simon Yarce Arango. All errors are ours.

termediate occupation-specific skills. Our analysis reveals that the primary mechanism behind the observed income effects associated with the advanced certificate is the ability to signal occupation-specific skills to potential employers.

*JEL Codes:* D80, J01, J31, J44

# 1 Introduction

This paper estimates the economic returns of signaling occupation-specific skills among experienced workers in the context of a developing country. Given the current state of the literature, it is uncertain whether providing accurate information about these skills holds any value once workers have accumulated substantial labor market experience. Some studies suggest that employers quickly learn about workers' productivity in the workplace (Lange 2007).[1] Even when employer learning is asymmetric, as long as wages and promotions reflect information about productivity potential, this information is expected to reach the market eventually (Kahn 2013; Pinkston 2009; Schönberg 2007). Therefore, previous findings suggest that experienced workers operate in a nearly frictionless environment, where additional information about productivity is unlikely to drive wage growth or job mobility.

However, these predictions often rely on extrapolations from data gathered during the early stages of workers' careers.[2] Moreover, they are typically derived from labor markets in developed countries, where information frictions are less pronounced and workers generally have higher average education levels.[3] In addition,

1. Lange (2007) uses a symmetric employer learning model and does not condition on staying with the same firm.

2. For instance, Lange (2007) utilizes data from the National Longitudinal Survey of Youth - 1979 Cohort. In the estimation sample, workers are relatively inexperienced, with an average potential experience of eight years. Additionally, Pinkston (2009) and Schönberg (2007) use the same dataset to estimate their models of asymmetric employer learning.

3. For developed countries, academic credentials, diplomas, and college reputation have been shown to mitigate information asymmetries by providing job seekers with indicators of their skills and offering firms valuable tools for screening and comparing candidates (Altonji and Pierret 2001; Arcidiacono et al. 2010; Bedard 2001; Clark and Martorell 2014). Nonetheless, there are reasons to think the same devices are not as effective in the developing world for several reasons. First, workers usually lack formal education and training. Second, for certain industries and sectors, there might not be enough variation in schooling among workers to infer productivity accurately. Third, traditional measures of academic ability may not reflect productivity in occupations that require skills that are mainly acquired on the job, constantly evolve with industry standards, or are not taught in formal academic institutions. Regarding the characteristics of the labor markets in developing countries, see, for example, Behrman (1999) and Rosenzweig (1988).

it remains unclear whether these predictions relate to specific aspects of workers' productivity or encompass their entire spectrum of skills. Recent literature underscores the importance of occupation-specific skills for securing employment and fostering long-term earnings growth for low-educated workers (Bandiera et al. 2020).[4] Consequently, reducing information frictions associated with these skills could impact earnings and mitigate income disparities among comparable workers. Nonetheless, it remains uncertain whether these benefits extend beyond workers entering the labor market to include more experienced workers.

We contribute to the literature by estimating the economic returns associated with signaling occupation-specific skills among experienced workers. We employ a sharp regression discontinuity design using unique administrative data to estimate these returns within a large population of experienced male workers. Our study focuses on a particular signaling device: a certificate issued by the Colombian National Training Service (SENA), a governmental organization responsible for evaluating and certifying workers' skills in Colombia. Starting in 2004, SENA implemented a rigorous evaluation procedure to assess occupation-specific skills acquired as a byproduct of work experience and to determine if those skills are up-to-date with the prevailing industry standards.[5] Program participants are assigned to one of four mutually exclusive and exhaustive categories based on their performance on the certification exam: no certificate, basic, intermediate, or advanced. Each category is defined by sharp thresholds corresponding to different exam-score intervals. Therefore, the signal varies in content depending on the certificate categories.

In an ideal scenario where certificates are randomly assigned, the wage variation across these predefined categories would capture the signaling value of obtaining a certificate. However, in the absence of random assignment, wage differentials not only reflect the signaling value but also encompass productivity disparities among workers and unobserved match-quality factors associated with the interaction between workers and firms. To address potential confounders, we leverage the sharp differences in scores required to obtain a particular certificate as a proxy for random assignment. We first focus on individuals who barely pass the exam and those who barely fail, assuming they are similar in all other dimensions that matter for productivity. Under certain conditions, passing status constitutes a

4. Besides, recent literature has shown that providing credible information on different components of workers' skills or recent job performance can largely improve workers' labor market outcomes (Abebe et al. 2021; Abel et al. 2020; Bassi and Nansamba 2022; Carranza et al. 2022).

5. The certificates under analysis are not legally required to practice the corresponding occupations.

valid approximation for random assignment for individuals with scores close to the passing cutoff (Cattaneo et al. 2020; Cattaneo and Titiunik 2022; Lee and Lemieux 2010). We use this insight to estimate the unbiased signaling returns of the basic certificate by comparing the earnings of the two groups within the two years after certification. We then extend this approach to estimate the returns of obtaining either an intermediate or an advanced certificate, which allows us to explore the distributional effects associated with the content of the signal. In addition, we use administrative data on labor market outcomes for program participants to investigate the potential mechanisms leading to income growth. Our analysis focuses on how obtaining a certificate impacts employment status (i.e., salaried work, self-employment, and unemployment) and how transitions between employers contribute to generating the observed returns.

Several features of our study set it apart from previous literature. First, we estimate the returns to signaling occupation-specific skills, whereas previous literature has examined mostly the effects of signaling academic aptitude as a proxy for productivity.[6] There are strong arguments suggesting occupation-specific skills are valuable and key to explaining post-schooling wage growth.[7] Nonetheless, limited attention has been devoted to analyzing the consequences of providing reliable information about these skills. Some exceptions can be found in the emerging literature studying the returns to occupation certification, predominantly in the context of developed economies, with estimates that span from no effect to a 13% increase in income.[8] However, for the most part, this literature relies on imprecise (self-

6. Some of the relevant papers in this literature are Bedard (2001), Clark and Martorell (2014), Feng and Graetz (2017), Freier et al. (2015), Jepsen et al. (2016), Khoo and Ost (2018), Macleod et al. (2017), and Tyler et al. (2000). Using data from an elite university in Colombia, Arteaga (2018) discusses the importance of signaling for college graduates. More recently, a few studies have focused on the value of signaling noncognitive skills (Bassi and Nansamba 2022), general skills at the hiring stage (Abel et al. 2020; Carranza et al. 2022), or field-specific skills (Busso et al. 2023).

7. Regarding the theoretical importance of task-specific skills, or more generally occupation-specific skills, see, for example, Becker (2009) and Gibbons and Waldman (1999, 2004). For empirical papers discussing the value of occupation-specific skills, see, for example, Kambourov and Manovskii (2009), Neal (1995), Parent (2000), Poletaev and Robinson (2008), and Sanders and Taber (2012). Finally, regarding the importance of skills for post-schooling wage growth, see Rubinstein and Weiss (2006) and Sanders and Taber (2012).

8. This literature further concludes that the effects of licensing on labor market outcomes are larger than the effects of certification. Unlike certification, occupational licensing mandates that individuals can engage in a particular occupation only if they satisfy specific predetermined criteria for competence. See, for example, Albert (2017), Kleiner and Krueger (2013), Kleiner and Vorotnikov (2017), and Xia (2021). In addition, the literature finds that the effects of certification are larger among less-educated individuals (Baird et al. 2021), suggesting the potential role of certification as a signal of skills in situations when general measures of human capital are not

reported) certification measures and predominantly uses subnational-level data, which limits the ability to examine effects across various segments of the economy. In addition, most papers rely on observable characteristics to estimate the effects of certification, and therefore, it is unclear whether self-selection into certification leads to biased estimates.

Second, our setting allows us to isolate the returns to signaling from other confounding factors. In our analysis, individuals are primarily full-time experienced workers who have completed their formal education. The skills they signal are acquired mainly on the job, as a byproduct of experience, rather than through formal education. In addition, the program tests skills without facilitating investment in human capital through lectures or training.[9] Importantly, we provide evidence spanning a wide group of occupations from a national sample of program participants. Therefore, our conclusions are not restricted to particular firms or sectors. Moreover, since our data cover the two-year period after certification, we can evaluate how long the effects take to manifest and how permanent they are.

Third, we study the effects on experienced workers directly. In contrast, most of the literature evaluates the returns to signaling skills when workers first enter or are only a few years into the labor market.[10] The effects on experienced workers have been consistently understudied, and it is unclear whether signaling skills can still generate returns even after agents have accumulated significant experience in the labor market. In fact, our findings suggest that this population group captures significant returns. In such a way, we provide novel evidence about the importance of information frictions on post-schooling income growth.

Lastly, limited attention has been given in the literature to signals that convey information about different skill levels. Previous studies have focused mainly on dichotomous signals and whether or not the worker possesses an occupational

available. Moreover, results in the literature also suggest that occupational licensing reduces asymmetric information and, thereby, the racial wage gap (Blair and Chung 2021, 2022).

9. It is possible that participants in the certification program are still investing in human capital by studying for the exam. Our empirical methodology allows us to address this issue credibly.

10. Regarding the value of academic credentials, see, for example, Arcidiacono et al. (2010), Bedard (2001), Clark and Martorell (2014), and Machin et al. (2020). More recently, the literature has focused on the value of signaling general skills (Abel et al. 2020), noncognitive skills (Bassi and Nansamba 2022), and field-specific skills (Busso et al. 2023). In all cases, these papers focus on young workers entering the labor market. For papers evaluating the labor market returns to the GED, see for example, Cameron and Heckman (1993), Jepsen et al. (2016), and Tyler et al. (2000). This literature focuses on individuals, typically aged between 18 and 25, who have recently entered the labor market.

certificate.[11] We are among the first to provide direct evidence of the distributional effects of displaying signals with different content.[12] Our context enables us to do so because the signaling device categorizes workers' skills into four mutually exclusive and exhaustive categories, each defined by a sharp threshold.

Our estimates reveal that the effects of signaling basic or intermediate occupation-specific skills are not statistically different from zero. This result is expected from the perspective of traditional signaling models since most program participants obtain basic or intermediate certificates. As such, evidence is compatible with the idea of firms paying wages based on average productivity, suggesting that only workers capable of signaling productivity levels above the average are likely to experience wage adjustments.

Indeed, our findings indicate that signaling advanced occupation-specific skills yields large and significant returns. Our estimates show that obtaining an advanced certificate generates an average increase in income of 9.7% during the two years following certification. We provide evidence indicating that the primary mechanism driving the observed effects on income is the potential to signal occupation-specific skills to prospective employers. First, we find that self-employed individuals at the time of certification transition to salaried work within the year following certification. Remarkably, these individuals experience an average income increase of 15.5% in the second year. Second, we estimate large effects on income for salaried workers at the time of certification (10.7%), as well as an average 46.8% increase in the probability of having a job-to-job transition. These findings suggest that the certificate can also be a valuable tool for salaried workers, enabling them to convey critical skill-related information to potential employers.

Notably, our results show that highly experienced workers can still improve their wages through certification, which is compatible with the existence of information asymmetries.[13] We argue that this phenomenon can be attributed to the certificate's unique ability to provide insights into the individual's adherence to current occupation standards, which is rarely discerned from resumes and may be easier to evaluate for an incumbent employer. In such a way, our findings provide com-

11. Examples of papers analyzing dichotomous signals are Clark and Martorell (2014), Jepsen et al. (2016), Machin et al. (2020), and Tyler et al. (2000). The literature studying the economic importance of certificates is relatively new. The following papers provide important contributions in this area: Albert (2017), Kleiner and Krueger (2013), and Kleiner and Vorotnikov (2017).

12. Another paper that provides an analysis of signals with different content is Bassi and Nansamba (2022).

13. See, for example, Aryal et al. (2022), Lange (2007), Pinkston (2009), and Schönberg (2007).

pelling evidence that information frictions remain critical even after workers have accumulated significant experience.

Finally, we offer suggestive evidence indicating that some of the estimated effects on income among salaried workers originate within the firm. While pure learning within the firm could theoretically explain wage adjustments for workers entering the labor force or even workers with low tenure, the same mechanism does not provide a satisfactory answer to rationalize the returns among the most experienced workers. We argue that the characteristics of program participants make it unlikely that returns originate from behavioral responses from either (i) current employers, who might modify their productivity expectations or use the certificate as an efficient screening tool for justifying promotions, or (ii) workers, who might alter their self-assessment of labor market prospects and productivity. Therefore, we favor the narrative that incumbent firms adjust wages to retain valuable workers who, due to the certificate, are more likely to leave and work for other firms.

Overall, our results establish the existence of substantial information frictions in the labor market for experienced workers. Moreover, they suggest that certification can mitigate those frictions by disclosing pertinent information to prospective employers. As such, signaling occupation-specific skills can still benefit individuals with considerable experience in the labor market. This includes not only those without salaried employment but also experienced workers already entrenched in the workforce. Therefore, in alignment with the insights of Bandiera et al. (2020), our results underscore the significant impact of certifying skills in fostering earnings growth and facilitating job transitions.

The rest of the paper is structured as follows. In Section 2, we provide a detailed description of the program. In Section 3, we describe the sample of program participants and elaborate on the procedure for obtaining information on labor market outcomes during and after applying for the certificate. We outline the empirical strategy employed to estimate the causal effects of obtaining a certificate in Section 4. In Section 5, we present the core findings. In Section 6, we delve into the mechanisms behind our main results. Finally, we conclude in Section 7.

# 2 Program Description

Since 2004, the Colombian National Training Service (SENA), a government agency in Colombia, has been responsible for implementing a nationwide certification program.[14] In Colombia, technical norms define the tasks and activities specific to different occupations and the up-to-date quality standards governing the production and provision of goods and services within those occupations. These norms are drafted and continuously revised by industry skill councils and are approved by the government. Based on the criteria defined by such norms, the certification program aims to assess and certify the knowledge and skills that workers acquire through their work experience, as well as the currency with occupational standards. In doing so, the program is tailored to occupations where knowledge is mostly acquired outside of formal education institutions, indirectly targeting lower-educated individuals working in low-paying jobs.

From its inception, the Colombian government has recognized this program as a pivotal policy to enhance firms' productivity and bolster their competitiveness. The policy's underlying objectives are to reduce the costs associated with identifying productive workers, streamline personnel selection processes, and minimize potential mismatches between firms and workers. Furthermore, the policy provides workers with means, i.e., certificates, to publicly showcase their skills and currency with occupational standards. This, in turn, aims to foster smoother transitions into more lucrative employment opportunities, thereby curbing unemployment rates among participants.

During the past decade, the program has gone through a significant expansion, resulting in SENA being entrusted with continually expanding participation, free of charge, across the country. SENA continuously advertised the program among firms, workers' associations, as well as national and local media. Furthermore, to enhance the possibilities of workers finishing the certification process, SENA provides flexible schedules to complete the exams. To date, SENA issues certificates in 912 technical norms, which have been developed by 74 industry skill councils representing the major sectors of the economy.[15] In 2019, SENA certified around 243,000 workers across 117 locations, which is 2.54 times more than those certified in 2010.[16] Importantly, the number of individuals certified between 2017

14. The two primary legal dispositions governing this program are Decreto 933/2003 and Decreto 4108/2011.

15. The six most popular technical norms, accounting for 24% of certifications between 2017 and 2019, were: serving customers following service procedures, handling food in compliance with current regulations, controlling access to restricted areas based on service characteristics and regulations, operating forklifts following technical manuals, and promoting safe and healthy practices in work environments.

16. In general, candidates can take the exam for all norms in any municipality. The primary constraint is the waiting time, as they may have to wait for an instructor from a different

and 2019 accounted for approximately 6.2% of the total workforce in low-paying jobs nationwide. This indicates that while the program is extensive, considerable potential remains to expand its coverage.

Participants join the program to obtain certification in a specific technical norm. To be certified, they must provide evidence of proficiency in executing the task and work activities defined by such a norm, as well as their knowledge of the prevailing quality standards. As a result, the certificate contains valuable information about the skills and knowledge required to perform a specific occupation, in accordance with the current standards. We refer to such skills and knowledge as occupation-specific skills.[17] To initiate the certification process, participants must demonstrate at least six months of experience in a given occupation. Typically, the process is completed within four weeks. Additionally, since technical norms are continuously revised and updated, the certificate remains valid for three years.

To obtain the certificate, individuals must take a two-part exam. The first part, known as the competence exam, entails participants performing a series of tasks and work activities under the observation of a panel of SENA officials. Evaluators assign a pass/fail grade based on the participants' performance. Since all participants must showcase relevant work experience to start the certification process, most of them successfully pass this stage (see Table 1). The second part of the exam involves a multiple-choice knowledge test. This test evaluates participants' understanding of the various concepts related to the occupation and the prevailing quality standards prescribed by the technical norm. SENA administers the exam, which is designed using a randomly selected set of predefined questions.[18] Therefore, the exam's difficulty is constant across participants. The exam is graded by a computer on a scale of 0 to 100 points. Using computerized grading ensures that SENA officials cannot manipulate the results. The score determines the level

location to visit and assist with the practical test. However, SENA does not consider this issue a significant barrier for participants to attain certification.

17. For instance, the technical norms pertaining to plumbing primarily outline the tasks and work activities related to installing and repairing piping fixtures and systems. These norms also define proper network installation, functionality, and durability standards. As a result, the certification program evaluates if individuals can perform occupation-specific activities efficiently while producing outcomes of higher quality and durability.

18. While the technical norm itself is publicly available, the question bank is not accessible. According to SENA, the question bank contains more than 100,000 questions for all the different technical norms. The question bank undergoes continual enhancement and revision to adapt to the evolving standards of a particular occupation. It is important to add that the number of questions in the competence exam ranges from 18 to 44. The specific number of questions for a particular norm depends on the number of tasks and activities described by the norm.

of certification conferred by SENA. Individuals who score below 30 points do not get a certificate, even if they pass the first part. Participants scoring between 30 and 59.9 points receive a basic certificate, while those scoring between 60 and 89.9 points are granted an intermediate certificate. Lastly, participants who score 90 or higher obtain an advanced certificate. Participants are only informed of the certification level attained, and the exact grade remains confidential.

According to SENA's guidelines, participants can improve their certification level by undertaking the second part of the examination in the subsequent fiscal year. Our data show that only 0.5% of participants retake the knowledge test. The lack of information about how close participants are to the certification cutoff likely discourages retaking, even among those who scored just below a given cutoff.

A certificate typically includes the participant's name, identifier number, certificate expiration date, awarded certificate level, and the specific technical norm for which the participant has been certified (see Figure OA1 in online Appendix A).[19] Employers can access certificate information by entering the worker's identification number into SENA's web portal, ensuring that the same information is available to both workers and employers. Consequently, participants are unlikely to conceal the certificate when dissatisfied with the outcome.

In general, no legal barriers prevent workers from continuing their current occupation, even if they do not obtain certification in the relevant technical norm. However, an exception exists for specific technical norms that apply to regulated occupations. Regulated occupations involve tasks where workers face exceptional hazards, or where failure to comply with the prevailing technical standards could lead to unacceptable risks for consumers and workers.[20] For such occupations, certification is mandatory and granted only upon achieving a minimum score of 90 points on the knowledge test. Hence, workers who score below 90 are not certified and should not practice such occupations. Within our dataset, technical norms related to regulated occupations comprise less than 7% of the observations, and we choose to exclude them from the analysis. This exclusion is mainly motivated by

19. According to SENA officials, exam takers always complete the exam, and the data does not indicate any incomplete tests.

20. Some examples of technical norms regarding occupations involving situations where workers face exceptional hazards include those related to tasks performed at elevated heights or the evaluation of equipment utilizing natural gas as an energy source. Examples of activities in which failure to comply with the technical standards could lead to unacceptable risks include installing and maintaining home networks for natural gas distribution and water purification procedures.

the fact that the 90-point threshold leads to distinct and incomparable outcomes for technical norms associated with regulated occupations and those associated with non-regulated occupations. While participants scoring above 90 receive an advanced certificate, regardless of the underlying occupation, participants who score below this threshold would not obtain a certificate in the case of technical norms associated with regulated occupations, and they would receive an intermediate certificate in the case of technical norms associated with non-regulated occupations.

Our analysis of the certification program relies on administrative data provided by SENA, which covers all participants seeking certification in technical norms linked to non-regulated occupations from January 2017 to December 2019. The causal analysis leverages the discontinuity observed in the certification levels (i.e., basic, intermediate, advanced) that arise from variations in underlying scores near the three respective cutoffs (i.e., 30, 60, and 90). SENA has meticulously documented participants' demographic characteristics and the specific technical norms for which they sought certification. Throughout this period, the program issued approximately 627,000 distinct certificates to more than 470,000 participants.[21] It is important to highlight that the institutional context remained consistent throughout the study period. Factors such as the number of evaluators, program coverage, and exam format remained unchanged since 2017. Section 3.1 provides additional information regarding SENA's data.

# 3 Data

Our analysis relies on two sources of information. First, we use SENA's novel administrative data with information about all participants in the certification program between 2017 and 2019. Second, given that our primary data on program participants do not contain information on labor market outcomes after certification, we use administrative records from contributions to the social security system to obtain income and employment status information.

21. The number of certificates exceeds the number of participants due to individuals being eligible for certification in multiple technical norms. In our analysis in Section 5, we focus on the returns to the first certificate.

## 3.1 SENA

We obtained data from SENA on all individuals who started the certification process between January 2017 and December 2019. The data contain information on the technical norm individuals applied to be certified on, the scores on the two-part exam, the test date, employment status at the time of certification, and socio-demographic information, such as educational achievement, age, and geographic location. In total, the data set contains information on 627,340 applications for certification.[22] Table 1 presents descriptive statistics for the complete dataset.

The sample comprises predominantly male individuals (68.4%). We focus on the sample of men since women may have a weaker attachment to the labor market and are more likely to engage in home production activities that are not captured in our data. Furthermore, women may face different productivity priors due to labor market discrimination, which could affect the returns to signaling. While this issue is certainly relevant, it lies beyond the scope of the current paper and is left for future research. Nonetheless, as discussed in Section 5.3, estimates using the full sample of men and women are compatible with our core results.

The second column of Table 1 shows that men in the sample are, on average, 38.5 years old. In addition, consistent with the type of occupations targeted by the certification program, there is a large share of low-educated individuals: Only 4% of the sample have a college degree or more, 46% have completed high school, and 20% have less than high school.

As previously mentioned, the first part of the exam is generally considered a pass for nearly all participants. Therefore, in practice, it does not determine their eligibility for the certificate. In fact, the pass rate for the sample of men stands at 99%. The objective of the second part is to assess participants' comprehension of concepts and current standards outlined in the technical norm, and therefore, such knowledge constitutes occupation-specific skills.[23] The mean score for the

22. This number excludes 41,675 applications for certification on technical norms related to regulated occupations.

23. For instance, while all plumbers likely possess the capability to install and repair drinking water and drainage networks, only the most skilled and up-to-date individuals are familiar with the design principles of the network that allows them to determine the minimum pipe diameter required for the different devices in the restroom according to the current norms and standards. For example, according to the technical norm, a Toilet with a flush tank requires a minimum pipe diameter of 1/2 inch, while a toilet with a Toilet flush valve requires 1 inch, and a urinal with a flushometer requires 3/4 inch. Therefore, approving the exam with a score above 90 points indicates that a plumber, in fact, knows the differences between toilet types and can choose the right pipe diameter to install them successfully.

second part of the exam among the sample of men is 82 points. Approximately 1% of workers fail to attain a certificate, whereas 13%, 40%, and 47% obtain basic, intermediate, and advanced certificates, respectively.

It is important to discuss the distribution of certification levels further. As mentioned, SENA offers flexible schedules for workers to engage in the certification process, making it unlikely for firms to deter workers from participating. Moreover, since there are no legal repercussions for under performing and wages in Colombia are downward rigid (Agudelo and Sala 2017), less productive workers do not face significant consequences for poor performance. Consequently, workers' participation decisions are most likely driven by their expected returns. The distribution of certificates suggests that those most likely to succeed in the program (i.e., obtain higher certification levels) are more likely to participate. Nonetheless, the extent of selection near the relevant certification cutoffs is unlikely to be substantial, as discussed in Section 4.2.

Table 1: Descriptive Statistics

| | SENA data | | Estimation Sample |
|---|---|---|---|
| | Full Sample | Men Only | (Men) |
| | A. Demographic Characteristics (SENA) | | |
| **Demographic Characteristics (Mean)** | | | |
| Age | 38.24 | 38.53 | 45.03 (0.00) |
| Less Than High School | 0.19 | 0.22 | 0.20 (0.00) |
| High School | 0.41 | 0.46 | 0.46 (0.07) |
| Some College | 0.37 | 0.30 | 0.30 0.00) |
| College or More | 0.04 | 0.02 | 0.04 (0.00) |
| **Employment Status (Mean)** | | | |
| Salaried Worker | 0.78 | 0.80 | 0.87 (0.00) |
| Self-Employed | 0.05 | 0.05 | 0.05 (0.00) |
| | B. Certification Program (SENA) | | |
| **Skills Certified** | | | |
| Technical Norms | 912 | 912 | 912 |
| Industry Skill Councils | 74 | 74 | 74 |
| **Certification Level (Mean)** | | | |
| Basic | 0.13 | 0.13 | 0.13 (0.02) |
| Intermediate | 0.40 | 0.39 | 0.39 (0.00) |
| Advanced | 0.47 | 0.48 | 0.47 (0.00) |
| **Certification Two-Part Exam (Mean)** | | | |
| Knowledge | 81.97 | 82.11 | 82.02 (0.00) |
| Competence | 99.15 | 98.91 | 99.02 (0.00) |
| **Individuals** | 627,340 | 429,272 | 181,395 |
| | C. Post Certification Labor Market Outcomes (PILA) | | |
| **Employment Status (Mean)** | | | |
| Salaried Work at Certification | ... | ... | 0.81 |
| Self-Employment at Certification | ... | ... | 0.09 |
| Potential Experience at Certification | ... | ... | 27.38 |
| Salaried Work | ... | ... | 0.77 |
| Self-Employment | ... | ... | 0.09 |
| Job-to-Job Transition Probability | ... | ... | 0.06 |
| Dummy for Accumulated Job-to-Job Trans. | ... | ... | 0.19 |
| **Income (Mean)** | ... | ... | |
| Income | ... | ... | 1,153,149 |
| Ln of Income - Salaried Worker | ... | ... | 13.97 |
| Ln of Income - Self-Employed | ... | ... | 13.90 |
| **Observations** | ... | ... | 1,434,061 |

**Notes:** This table reports descriptive statistics for the full sample of men and women applying to get SENA certificates between 2017 and 2019 (first column), the full sample of men (second column), and the matched sample of men (third column). The matched (estimation) sample corresponds to the subsample of men we could match with PILA. P-values of a difference-in-means test for the full and matched samples of men are reported in parenthesis adjacent to the corresponding means for the matched sample. The first panel reports demographic characteristics and employment information, calculated using SENA data only. The second panel reports information regarding the certification program. The last panel uses PILA data and shows descriptive statistics for selected variables at the time of certification. In addition, it shows descriptive statistics on relevant labor market variables during the two years following certification. Potential experience at the time of certification is calculated by subtracting years of education plus six years from the worker's age. A job-to-job transition is a worker's move from one firm to another in the subsequent quarter. The dummy variable for accumulated job-to-job transitions takes the value one if the individual has had at least one job-to-job transition after certification. The income variable contains zeros in periods when individuals are not salaried workers or self-employed.

## 3.2 PILA and Estimation Sample

We use employer-employee-linked administrative data from the Unified Social Security Contributions Form (PILA, by its Spanish acronym) to obtain the labor market histories of program participants. By law, all workers and firms in the formal sector must report to PILA their contributions to the social security system. PILA provides monthly information on wages, payroll-tax payments, employment type (salaried work or self-employment), and firm and job characteristics. We also observe workers' transitions between employers and in and out of PILA. However, we lack information on individuals working in the informal sector.

We can only match personal identifier numbers between PILA and SENA data for the subsample of program participants who reported to PILA at any point during 2010.[24] This implies that, for individuals who did not report to PILA in 2010, we cannot observe their labor market outcomes at any time. In the third column of Table 1, we report descriptive statistics for the matched sample of men, which is our estimation sample.[25]

The matched sample contains 39% of individuals from the entire sample of men. There are significant differences between the matched and unmatched samples. Nevertheless, the magnitude of such differences is, in most cases, subtle. First, the matched sample is older than the unmatched one. This difference is not surprising since matched individuals reported to PILA in 2010. Hence, younger individuals, who are less likely to have worked in 2010 (seven to nine years before certification), are less likely to be matched. Second, unemployment is less prevalent in the matched sample. This fact is also expected since the matched sample contains individuals already employed at a younger age. Notably, all 912 technical norms are present in the matched sample, and individuals in the estimation sample are as likely to obtain a basic, intermediate, or advanced certificate as those in the full sample.[26]

We aggregate PILA information at the quarterly level in the following way.[27]

24. Law 1581 of 2012 is the general legal framework applicable to managing and protecting personal data. Because of the restrictions imposed by the law, individual identification numbers were only part of PILA in 2010.

25. Section 5.3 presents additional results for the matched sample of men and women.

26. Table OA1 in online Appendix A displays the top 10 technical norms in the matched and unmatched samples. There is a fair degree of overlap in the more prevalent norms between the two samples, with only four norms in the top 10 for the matched sample and not appearing in the top 10 for the unmatched sample.

27. We aggregate the monthly PILA data into quarters to enhance computational efficiency.

First, our measure of income is the average monthly reported income. Second, we classify an individual as employed if he appeared in PILA at least one month in the quarter. Third, if an individual does not report to PILA in any month during the quarter, we classify him as not being employed. Given our data on labor market outcomes, we cannot distinguish unemployment from employment in the informal sector, in which reporting to PILA is not mandatory. Nevertheless, taking advantage of the self-reported data on employment from SENA and comparing it against the employment data on PILA at the time of certification, we can infer the relevance of the informal sector in our sample of participants. While the measure of employment in SENA data is likely more comprehensive than the one from PILA, the employment rate in both samples is remarkably similar. In both data sets, despite the slight differences in composition, the overall employment rate at the time of certification is around 92%, suggesting that the informal sector is not as relevant for our sample as it may be in the general population of low-educated Colombian workers.

Each month, workers must classify their occupational status using PILA's categories. We classify individuals as salaried workers if they are categorized as dependents or belong to any other category in which their employers make contributions to the social security system on their behalf. Conversely, individuals are classified as self-employed if they report being independent workers or belong to any other category in which they have to pay their entire contribution to the social security system.

Workers are allowed to be certified in multiple norms. Nonetheless, most participants (69.3%) have only one certificate, 20.0% have two certificates, and 10.7% have more than two certificates. In our preferred specification, we estimate the returns of the first certificate. As we show in Section 5.3, our conclusions remain when we exclude individuals with more than one certificate between 2017 and 2019. Lastly, we look at outcomes up to two years after certification, when the certificates remain valid.[28]

In our panel, an observation is a worker-quarter pair. The last panel of Table 1 reports summary statistics on labor market outcomes for the estimation sample within two years of certification. The overall employment rate in this two-year

28. SENA certificates are valid for three years. However, we choose to look at outcomes up to two years out because we lack data beyond the third quarter of 2021 and because we want to get a balanced sample of individuals applying for a certificate between 2017 and 2019.

period is 86%, with 77% of workers being salaried. At the time of certification, workers possess, on average, 27 years of potential experience, calculated by subtracting years of education plus six years from the individual's age. The average monthly income for our sample is 1,153 thousand Colombian pesos, which is equivalent to approximately USD 427 in 2018 dollars. This amount is slightly higher than the average minimum wage between 2017 and 2021, standing at 826 thousand Colombian pesos, suggesting that most of the people in our sample work in low-paying jobs that don't require formal education, such as a college degree.

Lastly, one key element of our analysis in Section 4 is the correlation between the score in the knowledge test (i.e., our running variable) and subsequent earnings. For instance, if scores are not correlated with earnings, it is unlikely that firms would infer that an individual is more productive based on higher test scores. To illustrate this correlation, we estimate a regression of earnings on test scores. We employ two measures of earnings: income in levels (including zeros) and the natural logarithm of income within two years of certification. We report results with and without additional controls for each outcome in Table 2. Additional controls include age, education, and an array of fixed effects. Our estimates indicate a positive and significant correlation between earnings and exam scores. For example, estimates in column 2 show that a ten-unit increase in exam scores is associated with an increase in income of 42.3 thousand (4.22%). These results support an information-based interpretation of the results.

Table 2: Exam Scores and Earnings: Correlations

| | (1)<br>**Income** | (2)<br>**Income** | (3)<br>**Ln(income)** | (4)<br>**Ln(income)** |
|---|---|---|---|---|
| Score | 4.351<br>(0.043) | 4.230<br>(0.044) | 0.003<br>(0.000) | 0.002<br>(0.000) |
| Controls | No | Yes | No | Yes |
| Observations | 1,446,151 | 1,446,151 | 1,267,044 | 1,267,044 |
| R-squared | 0.007 | 0.145 | 0.008 | 0.158 |

**Notes:** The reported coefficients are from ordinary least squares regressions and indicate the effect of exam scores on the corresponding outcome. The outcomes are: income in levels (columns 1 and 2) and the log of income (columns 3 and 4). Regressions in columns 2 and 4 include the following control variables: age dummies, education dummies, industry skill councils' fixed effects, and year-of-certification fixed effects. Standard errors are reported in parentheses below the point estimates.

# 4 Empirical Strategy

## 4.1 Research Design

This section describes the empirical strategy used to estimate the returns of obtaining a basic, intermediate, or advanced certificate. Given the nature of the SENA certification program, we use a sharp regression discontinuity (RD) design (Cattaneo et al. 2020; Lee and Lemieux 2010). In a typical RD design, all units receive a score, and the treatment is assigned to units with a score above a known cutoff. The key feature of the RD design is that, given the score, the probability of receiving treatment changes discontinuously at the cutoff. As long as units cannot sort around the known cutoff, which can be verified empirically, the abrupt change in the probability of receiving treatment is as good as random. Therefore, it can be used to learn about the local causal effect of the treatment. Importantly, with a sufficiently large number of mass points in the score, as observed in our study, it is possible to analyze a regression discontinuity design using methods from the continuity-based approach (Cattaneo et al. 2022).

Let $T_{it}^{c} = \mathbf{1}(score_{it} \geq c)$ be an indicator variable that takes the value of 1 if individual $i$, taking the exam in year $t$, obtains a certification score, $score_{it}$, equal or above the threshold $c$. As noted in Section 3.1, we consider three thresholds: 30, 60, and 90, which correspond to obtaining a basic, intermediate, or advanced certificate, respectively. The standard local linear estimator of the RD treatment is implemented by running the following weighted least squares regression:

$$Y_{is} = \alpha + \beta score_{it} + \delta_{RD}^{c} T_{it}^{c} + \tau score_{it} \times T_{it}^{c} + \gamma Z_i' + \varepsilon_{is}, \tag{1}$$

where, $Z_i$ are predetermined covariates and $Y_{is}$ represents the labor market outcome of interest $s > 0$ quarters after certification. Equation (1) is estimated with only individuals with scores within a chosen bandwidth $h$, such that $score_{it} \in [c-h, c+h]$, and with weights applied according to some kernel function. The main parameter of interest, $\delta_{RD}^{c}$, is estimated as

$$\delta_{RD}^{c} = \lim_{score_{it} \downarrow c} E[Y_{is} | score_{it}, Z_i'] - \lim_{score_{it} \uparrow c} E[Y_{is} | score_{it}, Z_i']. \tag{2}$$

Our primary outcome of interest is the natural logarithm of income, which includes earnings from salaried work and self-employment.[29] It is important to note that PILA data does not capture earnings in the informal sector. Thus, our findings should be interpreted within the context of returns to the certificate in the formal sector.

As mentioned in Section 3.2, we look at outcomes up to eight quarters after certification (that is, $s \in [1, 8]$). The predetermined covariates, $Z_i$, include age and education dummies. We also include industry skill councils' fixed effects and year-of-certification fixed effects.[30] Therefore, we estimate the returns to the certificate by exploiting variation within age, education, year of certification, and industry groups. Following Cattaneo et al. (2020), we use a triangular kernel, a first-order polynomial, and choose the bandwidth to minimize the mean squared error (MSE) of the local polynomial RD estimator.[31] We report RD point estimators with robust bias-corrected confidence intervals (Cattaneo et al. 2020). Lastly, standard errors are clustered at the technical norm level to adjust for the correlations induced by industry- and occupation-specific unobserved components.

## 4.2 Validity of the Design

The main threat to identification in an RD design is the possibility that program participants actively manipulate their score around the threshold of interest, implying that individuals just above the threshold are systematically different from individuals just below it. To mitigate this concern, we perform two falsification tests that support the validity of the RD design. First, we examine the density of the running variable, $score_{it}$, around each threshold. Second, we investigate whether treated individuals are similar around each threshold. The intuition for

29. By employing log income as the outcome measure, we exclude individuals reporting zero earnings. This approach helps to avoid potential confounding effects arising from the impact of signaling on employment. For example, if signaling negatively affects overall employment, we would observe an increase in zero-income cases, potentially attenuating the estimated effect on income. A detailed discussion of the effects on employment can be found in Section 6. In Section 5.3.1, we present the results when employing income in levels as the outcome of interest.

30. Ideally, we would like to include technical norm fixed effects. However, given the large number of technical norms (912), we instead choose to include industry skill councils' fixed effects (74), which can be regarded as industry fixed effects. In addition, while it would be interesting to add firm fixed effects, which would allow us to estimate the returns to the certificate within the firm, there is not enough variation in the sample to perform such an exercise. The median proportion of workers participating in the certification program between 2017 and 2019 is 4% of firms' total workers.

31. In Section 5.3, we show that our results are robust to the inclusion of additional controls (year and location fixed effects or no controls at all), using alternative methodologies to choose the bandwidth, using fixed bandwidths, using non-bias-corrected RD estimates, and not adjusting for the presence of mass points during estimation.

these two falsification tests is that if individuals cannot manipulate their score, the number of observations just above the threshold should be similar to the number of observations just below the threshold, and there should be no systematic differences across groups.

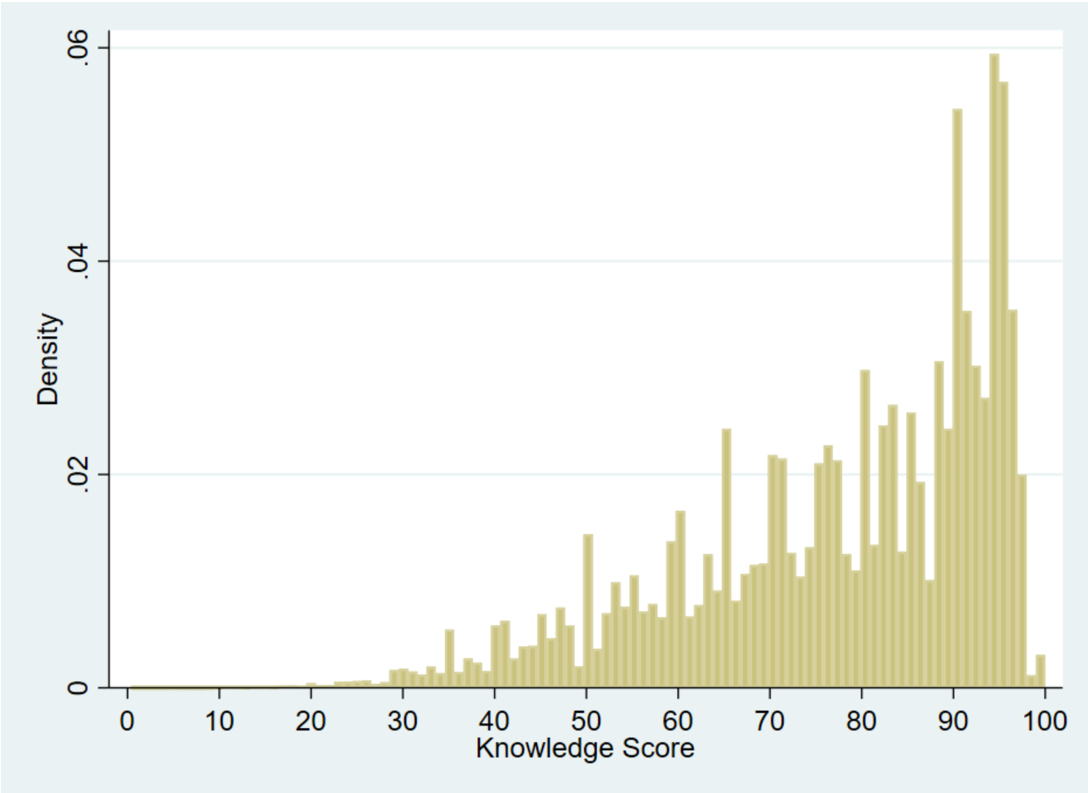
Manipulation of the score seems unlikely in our context for several reasons. As discussed in Section 2, the test format is multiple choice, and the grading is performed (by a computer) in a location different from the testing location, reducing the chances of manipulation. Furthermore, the underlying score is not revealed to participants or employers, who only get to see the certification level. Figure 1 displays the distribution of the scores.[32] Visual inspection of the histogram shows no apparent discontinuities in the scores around the thresholds of interest: 30, 60, and 90. We formally test for the presence of manipulation of the score around each threshold using the test proposed by Frandsen (2017). Frandsen (2017) is the adequate manipulation test in our context since the running variable has mass points at integer values. The critical parameter in the manipulation test is $k$, which dictates the maximum degree of non-linearity in the probability mass function that is still considered compatible with the absence of manipulation.[33] The results of the manipulation test around all three thresholds lead us to fail to reject the null hypothesis of the absence of manipulation (p-value = 1).

To perform the falsification analysis on predetermined characteristics, we estimate Equation (1), using each characteristic as the outcome variable. We analyze the set of predetermined covariates used in the primary analysis, $Z_i$, and income at the time of certification (that is, $s = 0$). The results are presented in Table 3. Our analysis shows that, at the moment of certification, individuals just below the threshold for receiving a basic certificate are not statistically different from individuals just above the threshold in terms of age or schooling. In addition, there are no observed differences regarding the reported income at the time of certification. Importantly, the results from this balancing test also suggest that unobserved differences correlated with pre-certification income and program participation are minimal among agents near the threshold.

32. In the histogram in Figure 1, we exclude the highest score, 100, since it represents a significant mass point (25% of certificates), denying a straightforward visual exploration of continuity. In Figure OA2 in online Appendix A, we display the complete histogram.

33. A smaller $k$ means even tiny deviations from linearity will lead the test to reject the null of no manipulation with high probability (Frandsen 2017). We choose $k$ using the entire distribution of the running variable, not just around the thresholds. Given our sample, the maximum suggested value for $k$ is 0.001.

Figure 1: Distribution of Scores




**Notes:** This figure displays the distribution of scores in the second part of the certification exam (knowledge test) for the matched sample of men. The histogram excludes the highest score, 100, since it represents a significant mass point, denying a straightforward exploration of continuity. In Figure OA2 in online Appendix A, we display the full histogram.

Finally, we reach the same conclusions for individuals around the intermediate and advanced thresholds. In all, our tests show a smooth evolution through the different thresholds, confirming that participants just above and below the respective cutoffs are similar.

# 5 Results

In this section, we present the core results. We use Equation (1) to estimate the returns from obtaining a given certification level. As mentioned before, our primary outcome of interest is the natural logarithm of income. We begin by examining the returns from the basic and intermediate certificates, separately. Subsequently, we present the estimates for the advanced certificate. These results allow us to directly investigate the distributional effects associated with the content

Table 3: Covariate Balance Check

| | (1) Threshold 30 | (2) Threshold 60 | (3) Threshold 90 |
|---|---|---|---|
| Age | 1.204 | -0.142 | -0.105 |
| | (0.959) | (0.342) | (0.734) |
| | [48.255] | [45.369] | [44.739] |
| High School | 0.052 | -0.049 | -0.050 |
| | (0.054) | (0.019) | (0.032) |
| | [0.475] | [0.506] | [0.483] |
| Some College | -0.014 | -0.001 | -0.031 |
| | (0.061) | (0.023) | (0.056) |
| | [0.200] | [0.265] | [0.304] |
| More Than College | 0.017 | 0.006 | 0.001 |
| | (0.007) | (0.004) | (0.014) |
| | [0.015] | [0.035] | [0.042] |
| Income at Certification (1000s) | 36.989 | -31.001 | 91.652 |
| | (50.301) | (29.250) | (79.819) |
| | [863.888] | [1,024.739] | [1,121.895] |
| Number of Observations | 181,395 | 181,395 | 181,395 |
| Effective # of Control Observations | 774 | 11,141 | 8,819 |
| Effective # of Treatment Observations | 2,894 | 17,158 | 19,950 |
| Bandwidth | 8.600 | 9.900 | 3.300 |

**Notes:** Standard errors are reported below the point estimates in parentheses. Standard errors are clustered at the technical norm level. The sample mean for the control group is displayed below the standard error in squared brackets. For each threshold, the analysis uses a fixed bandwidth that is the average of the optimal bandwidths in Table 4. Bandwidths are displayed below the effective number of observations.

of the signal.

## 5.1 Effects of Obtaining a Basic or Intermediate Certificate

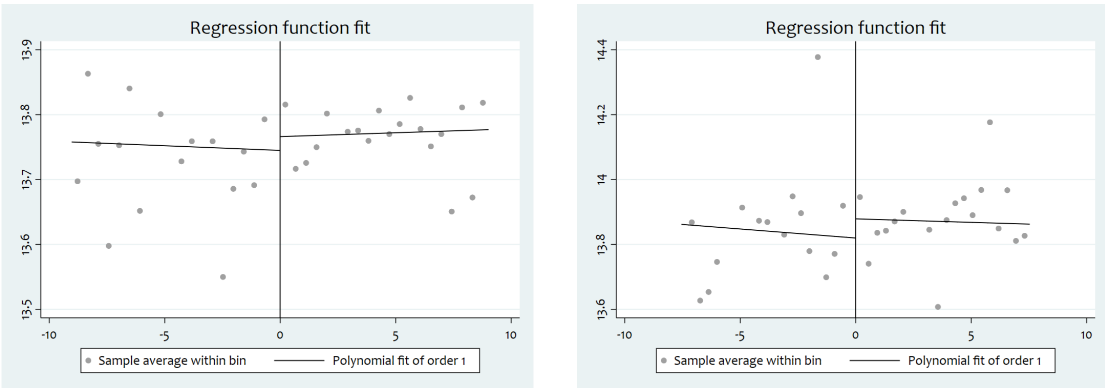
Following Equation (2), the effect of obtaining a basic certificate is measured by the discontinuity observed between individuals who score just below 30 points and those who score just above 30 points. The first panel of Table 4 displays the results on the log of income for quarters one to eight after certification.[34] Figure 2 presents a visual depiction of the effects one and eight quarters after certification. The figure seems to suggest a weak correlation between the running variable (exam scores) and the log of income. Such a pattern is not necessarily surprising, given our research design. Specifically, individuals around the threshold of interest are

34. The estimates in Table 4 offer a comprehensive view of how the optimal bandwidth and, consequently, the number of effective observations vary by quarter.

assumed to be similar, except that the group to the right of the threshold obtained a higher certificate. Furthermore, when employing the entire sample to examine the relationship between earnings and exam scores, Table 2 demonstrates a robust, positive, and significant correlation between the two variables, supporting an information-based interpretation of the results.

Our estimates generally reveal no discernible effect on income within the first two years following certification. Specifically, for individuals with a basic certificate, income changes remain statistically equal to zero even after eight quarters, relative to marginal individuals without a certificate. This result is not entirely unexpected, considering the narrow sample size around the threshold for basic certificates, which affects the precision of the estimates.

Figure 2: Sharp Regression Discontinuity Estimates of the Short- and Medium-Term Effects of Obtaining a Basic Certificate on Log of Income



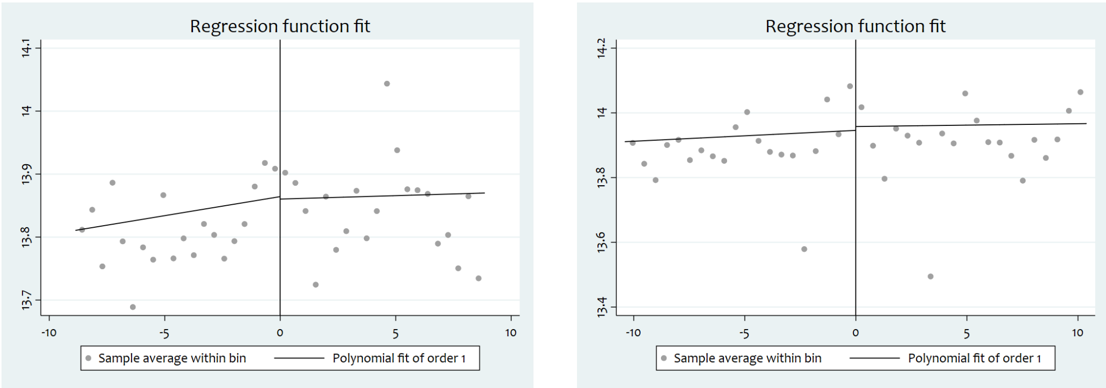

(a) Quarter One

(b) Quarter Eight

**Notes:** The two figures summarize the estimated results of Equation (1), one and eight quarters after certification, using the log of income as the main outcome. The running variable is the exam score, and the discontinuity threshold is 30. All regressions include the controls described in Section 4, use a triangular kernel, and use the bandwidth that minimizes the mean squared error of the local polynomial regression discontinuity estimator.

Turning to the effects of acquiring an intermediate certificate, the second panel of Table 4 presents the results on the log of income one to eight quarters after certification. The effect is measured by the discontinuity observed between individuals who score just below 60 points and those who score just above 60 points. Figure 3 presents a visual depiction of the effects one and eight quarters after certification. Our analysis indicates that the effect is small and not statistically different from zero for individuals obtaining an intermediate certificate relative to marginal individuals who obtain a basic certificate.

Figure 3: Sharp Regression Discontinuity Estimates of the Short- and Medium-Term Effects of Obtaining an Intermediate Certificate on Log of Income




(a) Quarter One (b) Quarter Eight

**Notes:** The two figures summarize the estimated results of Equation (1), one and eight quarters after certification, using the log of income as the main outcome. The running variable is the exam score, and the discontinuity threshold is 60. All regressions include the controls described in Section 4, use a triangular kernel, and use the bandwidth that minimizes the mean squared error of the local polynomial regression discontinuity estimator.

Two plausible explanations can account for the statistically null effects of obtaining either a basic or an intermediate certificate. First, the basic or intermediate certificates may not significantly enhance workers' prospects for transitioning to other firms. Potential employers likely do not want to incur the costs of poaching average workers, and, consequently, there is no incentive for current employers to increase wages. Second, it is possible that the basic and intermediate certificates do not provide new information about workers' productivity, thus leading to minimal revisions in employers' priors and, consequently, earnings.[35]

Both explanations are foreseeable in our context given that 47% of the population applying to be certified gets an advanced certificate. Such an outcome further aligns with predictions of the basic signaling models (for example Spence 1973, 1974; Weiss 1995). In these models, firms pay wages based on average productivity, and only workers capable of signaling productivity levels above the average (e.g., advanced) would experience wage adjustments.

35. One may be worried that employers pay lower wages when the certificate is not as expected (e.g., basic certificate). However, this is likely not the case. In the Colombian labor market, wages are characterized as downward rigid (Agudelo and Sala 2017). Likewise, it is unlikely that workers would be dismissed with cause for not reaching a given certification level, as this would not constitute a breach of the employment contract nor a case for termination with cause.

## 5.2 Effects of Obtaining an Advanced Certificate

For the advanced certificate, the results for all eight quarters are displayed in the third panel of Table 4. Figure 4 presents a visual depiction of the effects on the log of income one and eight quarters after certification.[36] Obtaining an advanced certificate substantially affects income for all quarters. The estimated effect is relatively stable over time, ranging between 8.7% and 12.7%.

The substantial returns associated with acquiring an advanced certificate suggest that it provides employers with new and reliable information. Workers with advanced certificates can distinguish themselves from average workers and experience income growth. For instance, an advanced certificate can signal occupation-specific skills to prospective employers, potentially leading to income adjustments. These adjustments may result from new employers seeking to attract the most productive workers or from incumbent employers aiming to retain their talented workforce. Therefore, obtaining an advanced certificate can contribute to wage growth within the firm, even when the current employer has an accurate prediction of the worker's productivity. In Section 6, we argue that the ability to signal occupation-specific skills to prospective employers emerges as the main mechanism behind our core results. Furthermore, we posit that, considering the predominance of experienced individuals in our sample, many of whom have held significant tenures in their current firms, our results are unlikely to be explained by the certificate's potential to (i) signal skills to incumbent employers, (ii) function as a screening tool for justifying promotions, or (iii) attenuate workers' uncertainties regarding their skills.

36. Similar to Figure 2, Figure 4 suggests a weak correlation between exam scores and the log of income. Although Table 2 shows a robust, positive, and significant correlation between the two variables, this correlation is difficult to observe within the narrow (optimal) bandwidth used for estimation (i.e., approximately $\pm 4$ score units around the threshold). Moreover, the weak correlation between exam scores and the log of income near the threshold supports our research design, which assumes that individuals close to the threshold are similar, except those to the right obtained a higher certificate.

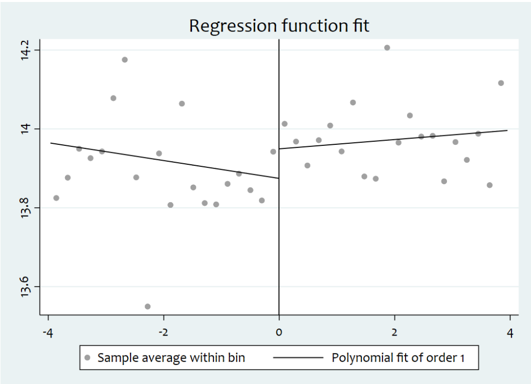
Table 4: Sharp Regression Discontinuity Estimates of the Short- and Medium-Term Effects of Obtaining a Basic, Intermediate, and Advanced Certificate on Log of Income

| | (1) Quarter 1 | (2) Quarter 2 | (3) Quarter 3 | (4) Quarter 4 | (5) Quarter 5 | (6) Quarter 6 | (7) Quarter 7 | (8) Quarter 8 |
|---|---|---|---|---|---|---|---|---|
| Basic Certificate | 0.019 | 0.028 | 0.002 | -0.009 | 0.027 | 0.049 | 0.039 | 0.062 |
| | (0.030) | (0.037) | (0.037) | (0.040) | (0.034) | (0.043) | (0.031) | (0.033) |
| # of Observations | 164,242 | 162,247 | 160,912 | 159,819 | 158,344 | 156,895 | 155,954 | 140,679 |
| Eff. # of Control Obs. | 756 | 633 | 698 | 713 | 617 | 680 | 611 | 641 |
| Eff. # of Treatment Obs. | 3,617 | 2,237 | 2,743 | 3,510 | 2,188 | 2,648 | 2,139 | 2,182 |
| Bandwidth | 10.14 | 7.738 | 9.994 | 10.47 | 7.661 | 9.038 | 7.203 | 8.306 |
| Mean | 13.726 | 13.738 | 13.751 | 13.777 | 13.775 | 13.777 | 13.805 | 13.814 |
| Intermediate Certificate | -0.010 | -0.002 | -0.001 | -0.013 | -0.006 | 0.009 | 0.017 | 0.008 |
| | (0.013) | (0.013) | (0.013) | (0.013) | (0.016) | (0.016) | (0.018) | (0.018) |
| # of Observations | 164,242 | 162,247 | 160,912 | 159,819 | 158,344 | 156,895 | 155,954 | 140,679 |
| Eff. # of Control Obs. | 9,611 | 9,921 | 9,808 | 9,319 | 11,296 | 11,248 | 11,393 | 10,147 |
| Eff. # of Treatment Obs. | 13,679 | 15,196 | 14,987 | 13,323 | 17,176 | 17,302 | 17,182 | 15,226 |
| Bandwidth | 8.798 | 9.714 | 9.357 | 8.753 | 10.30 | 10.79 | 11.00 | 10.41 |
| Mean | 13.829 | 13.840 | 13.863 | 13.873 | 13.878 | 13.892 | 13.909 | 13.908 |
| Advanced Certificate | 0.087 | 0.087 | 0.082 | 0.127 | 0.104 | 0.094 | 0.099 | 0.099 |
| | (0.028) | (0.031) | (0.034) | (0.029) | (0.029) | (0.027) | (0.032) | (0.032) |
| # of Observations | 164,242 | 162,247 | 160,912 | 159,819 | 158,344 | 156,895 | 155,954 | 140,679 |
| Eff. # of Control Obs. | 8,322 | 8,009 | 6,843 | 7,986 | 7,901 | 10,235 | 7,619 | 6,929 |
| Eff. # of Treatment Obs. | 18,098 | 17,852 | 14,410 | 17,669 | 17,400 | 23,044 | 16,776 | 14,754 |
| Bandwidth | 3.955 | 3.548 | 2.981 | 3.684 | 3.625 | 4.113 | 3.082 | 3.013 |
| Mean | 13.885 | 13.901 | 13.913 | 13.927 | 13.937 | 13.956 | 13.970 | 13.971 |

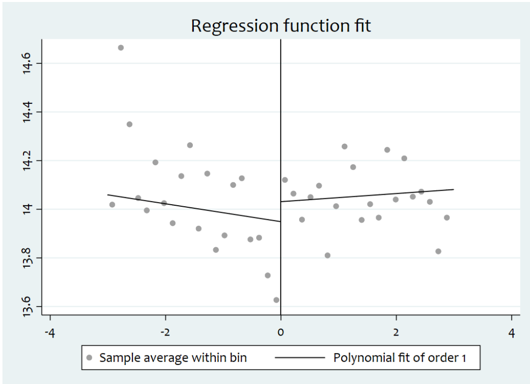
**Notes:** Each cell displays the estimate for the main coefficient of interest in Equation (1), one to eight quarters after certification, for each discontinuity threshold. The outcome is the log of income. The running variable is the exam score, and the three discontinuity thresholds are 30 (first panel), 60 (second panel), and 90 (third panel). All regressions include the controls described in Section 4, use a triangular kernel, and use the bandwidth that minimizes the mean squared error of the local polynomial regression discontinuity estimator. The total number of observations changes across quarters, given the variation in the number of individuals with positive earnings; it further drops in quarter 8 due to missing information in the last quarter for individuals who certified in the last quarter of 2019. The effective number of observations changes across quarters given the variation in the optimal bandwidth. Robust bias-corrected standard errors are reported below the point estimates in parentheses. Standard errors are clustered at the technical norm level. The sample mean for the control group is displayed below the optimal bandwidth.

It is important to add the following caveat. Since employers do not observe the test score, our estimated returns for the advanced certificate most likely reflect priors about the productivity of a broader group of agents, rather than solely those at the threshold (Graetz 2021). Within this broader group, workers holding an advanced certificate may exhibit higher productivity due to greater innate ability, enhanced occupation-specific skills, or a combination of both. Therefore, The RD design cannot disentangle the relative contributions of these factors.

Figure 4: Sharp Regression Discontinuity Estimates of the Short- and Medium-Term Effects of Obtaining an Advanced Certificate on Log of Income




(a) Quarter One




(b) Quarter Eight

**Notes:** The two figures summarize the estimated results of Equation (1), one and eight quarters after certification, using the log of income as the main outcome. The running variable is the exam score, and the discontinuity threshold is 90. All regressions include the controls described in Section 4, use a triangular kernel, and use the bandwidth that minimizes the mean squared error of the local polynomial regression discontinuity estimator.

Finally, there are two additional considerations to highlight. First, our estimates are within the range of estimated returns to certification in developed economies (Albert 2017; Kleiner and Krueger 2013; Kleiner and Vorotnikov 2017). Second, the estimated returns to signaling advanced occupation-specific skills are comparable to spending an additional year at school in Colombia (Garcıa-Suaza et al. 2014; Herrera-Idárraga et al. 2015; Morales et al. 2021). In that sense, the certificate seems to transfer information about a worker's productivity that is as valuable as that coming from schooling. Such a conclusion is important for individuals with characteristics similar to our estimation sample—namely, less educated workers in their forties who have finished school and likely have limited opportunities for wage growth.

## 5.3 Robustness Checks

### 5.3.1 Alternative Outcomes

The outcome of our main specification is the log of income. By using this outcome variable, we exclude individuals reporting zero earnings due to unemployment or working in the informal sector. In this subsection, we utilize income in levels (including zeros) as the main outcome. Unlike our previous estimates, these estimates capture both the extensive (employment) and intensive (earnings) margin effects. The results in Table OA3 in online Appendix B show that the estimated effects on earnings from obtaining basic, intermediate, and advanced certificates remain robust when using income as the main outcome. For instance, the effects of obtaining a basic or intermediate certificate are small and imprecisely estimated. Additionally, the estimated effect of obtaining an advanced certificate averages 152 thousand Colombian pesos, which is equivalent to an increase of 13.4%. These findings are not surprising since most of our agents are employed, and the estimates mainly capture intensive margin effects. We discuss this issue further in Section 6.

### 5.3.2 Alternative Specifications and Alternative Samples

We consider several alternative specifications to evaluate the robustness of our findings. The complete set of results is presented in online Appendix B. Upon examination of the estimates, our main conclusions remain robust across various specifications and samples.

In Figure OA3 in online Appendix B, we show that our main conclusions are robust to using different specifications to evaluate the effects of the certificate.[37] For instance, our core findings are robust to including year fixed effects (to account, among other things, for the potential effect of COVID-19), location fixed effects, excluding controls, using an optimal bandwidth that minimizes the coverage error, using a fixed bandwidth, employing both a fixed bandwidth and a sample reporting earnings in all eight periods considered (leading to a fixed number of total and effective observations over time), using non-bias-corrected RD estimates (Calonico et al. 2014), and not adjusting for the presence of mass points during estimation. The magnitude and significance of the returns to signaling occupation-specific skills remain in all specifications. The most notable deviation

37. The complete set of results, with detailed information on standard errors, number of observations, and bandwidth, are presented in Tables OA4 to OA6 in online Appendix B.

arises when employing a fixed bandwidth to evaluate the effects of obtaining an advanced certificate. However, even in this case, the estimates consistently confirm the existence of positive and significant returns. On average, these returns amount to 80% of the baseline estimates.

In addition, in Tables OA7 to OA9 in online Appendix B, we show that our main conclusions are robust to using different samples.[38] First, our primary findings remain largely unchanged when utilizing a sample comprising both men and women.[39] In this scenario, acquiring either a basic or intermediate certificate yields returns statistically indistinguishable from zero, whereas obtaining an advanced certificate leads to positive, significant, and permanent returns. Compared to the baseline results for the advanced certificate, the returns in the full sample are larger and more precisely estimated for all quarters. This observation suggests that women face larger returns to signaling occupation-specific skills, which aligns with traditional models of discrimination. According to these models, either employers prefer to work with males or possess prior beliefs that women have lower productivity than men, resulting in differential compensation (Aigner and Cain 1977; Lang and Spitzer 2020). The presence of an advanced certificate likely corrects these prior beliefs by providing evidence that both men and women possess equivalent occupation-specific skills. Consequently, in line with our findings, the proportional increase in returns for women should be greater.[40]

Second, our main conclusions remain mostly unchanged when we restrict the sample to include only men who applied for just one certificate in 2017, 2018, and 2019. For the advanced certificate, the results are slightly smaller in magnitude and less precisely estimated, suggesting that the marginal value of signaling more than one occupation-specific skill within an industry is somewhat important. Therefore, our results may be partly driven by the presence of multiple certificates or complementarities between certificates.

Third, to account for the fact that our measure of income may be subject to underreporting, we analyze two subsamples to evaluate the robustness of our results.[41]

38. The complete set of results for alternative samples, with detailed information on standard errors, number of observations, and bandwidth, are presented in Tables OA7 to OA9 in online Appendix B.

39. Table OA2 in online Appendix A provides descriptive statistics for the estimation sample of men and women.

40. This is an interesting finding that deserves closer examination in future research.

41. In Colombia, the monthly contribution to social security includes three categories: pension, health insurance, and insurance to cover occupational hazards. Since the coverage does not

First, we estimate Equation (1) excluding self-employed individuals, who are more likely to under-report earnings. Second, we use a more restricted sample consisting of those individuals who only worked as salaried employees within the two years following certification. Overall, the conclusions for all three certificates remain robust, suggesting that under-reporting does not significantly influence our main results.

To further validate our results, we perform a falsification test to look at placebo thresholds, that is, thresholds other than the real thresholds determining the treatment assignment (Cattaneo et al. 2020). The intuition behind this falsification test is that the probability of receiving treatment should change abruptly only at the true thresholds. Hence, we should not observe abrupt changes in log income at artificial thresholds. We perform the falsification tests by estimating Equation (1) using six placebo thresholds: 25, 35, 55, 65, 85, and 95. The results are summarized in Figure OA7 in online Appendix B.[42] We find no effect on log income for any of the alternative thresholds, which further validates our research design.

The evidence in this section suggests that our dataset satisfies the critical assumptions for sharp RD estimation. Furthermore, our core findings are not driven by a specific bandwidth choice or specific controls and are not affected by under-reported income. Moreover, we provide evidence that our key insights are not sample-specific.

# 6 Mechanisms

Our baseline estimates indicate that obtaining an advanced certificate significantly increases income, whereas intermediate and basic certificates do not have such effects. In this section, we propose that the advanced certificate's positive returns are mostly due to its role in signaling productivity to potential employers. We analyze the data and provide empirical evidence compatible with this possibility. We further discuss some alternative explanations that are likely less prevalent in our context. In particular, based on the characteristics of our sample and

depend on the contribution for health and occupational hazards, individuals have incentives to under-report earnings obtained from self-employment or working in the informal sector. Such an incentive does not exist for salaried workers since the employer makes the payments, which count as labor expenses toward tax returns.

42. The complete set of results for placebo thresholds, with detailed information on standard errors, number of observations, and bandwidth, are presented in Table OA10 in online Appendix B.

the substantial returns observed among experienced employees, we posit that our results are unlikely to be explained by the certificate's potential to (i) signal skills to incumbent employers, (ii) function as a screening tool for justifying promotions, or (iii) attenuate workers' uncertainties regarding their own skills.

## 6.1 Conceptual Framework: Signaling Occupation-Specific Skills to Potential Employers

From the perspective of traditional signaling models (Spence 1973, 1974; Weiss 1995), the returns of the advanced certificate can be explained by its ability to transfer valuable information about the worker's potential productivity to prospective employers. Within our context, the certificate's significance lies in its ability to convey essential information that may not be readily discernible from resumes or other publicly observable attributes, such as experience. This includes pertinent details on whether the individual possesses up-to-date skills aligned with the constantly evolving and prevailing occupation-specific standards. In essence, the certificate provides information that transcends the scope of traditional work experience metrics.

The certificate serves as a valuable instrument for individuals not currently employed in salaried positions, such as the unemployed or self-employed, by facilitating their transition to salaried employment through the provision of crucial insights about their occupation-specific skills to prospective employers (Abebe et al. 2021; Bassi and Nansamba 2022; Carranza et al. 2022; Groh et al. 2015). Additionally, the certificate enables salaried workers to convey skill-related information to potential employers. This mechanism is particularly important in contexts of asymmetric information, where incumbent employers possess more knowledge about workers' occupation-specific skills than potential employers (Kahn 2013; Pinkston 2009; Schönberg 2007). For external firms lacking insider information, accurately assessing these skills and determining whether workers meet current occupational standards can be challenging. The certificate helps bridge this information gap.

Consequently, for salaried workers, the certificate can trigger outside offers from potential employers (i.e., *direct* response), leading to two possible outcomes. First, salaried workers may choose to transition to a new firm if the incumbent employer does not provide a counteroffer or if the counteroffer is insufficiently attractive. Second, the incumbent employer may respond (i.e., *indirect* response) by adjusting wages to retain valuable workers.[43] Therefore, *indirect* responses can also generate positive returns, even though learning about productivity has already occurred in the workplace. Importantly, since information about the individual's alignment with current occupational standards is not perfectly correlated with experience and may not be readily discernible from resumes, the proposed mechanism likely remains relevant even among the most experienced salaried workers. In such a way, positive returns among the most experienced workers reveal that the related information frictions tend to be highly persistent.[44]

In Section 6.3, we explore the possibility of incumbent firms *directly* responding to the information provided by the certificate. This response typically involves reassessing productivity as the incumbent continues to learn about the productivity of its workers. Nonetheless, given the tenure composition of our sample, we posit that this alternative mechanism is unlikely to be the primary driver behind our observed results.

## 6.2 Evidence on Mechanisms

### 6.2.1 The Value of Signaling to Potential Employers: Evidence from the Self-Employed and the Unemployed

We start by evaluating whether signaling occupation-specific skills to potential employers can explain some of the advanced certificate's returns. Our initial emphasis is on participants who are not engaged in an employer-employee relationship at the time of certification, specifically those who are self-employed or unemployed. This focus aims to ensure that incumbent employers do not influence responses from potential employers. In the next subsection, we shift focus to salaried workers, for whom the certificate may generate responses from both incumbent and potential employers.

To the extent that the certificate enables self-employed and unemployed individ-

43. We refer to this possibility as *indirect* response as it is not directly coming from acquiring new information about the worker's productivity. It is important to add that in frictional labor markets marked by information asymmetry, such a response is probable. Within those environments, a wedge often emerges between workers' marginal products and their wages, presenting opportunities for wage adjustments. In contrast, within perfectly competitive markets, firms pay wages in accordance with the worker's marginal product and, therefore, the incumbent employer would be less likely to respond to counteroffers (Postel–Vinay and Robin 2002; Postel-Vinay and Robin 2002).

44. This observation is particularly relevant if incumbent employers are underpaying workers. This idea aligns with some of the results in Adhvaryu et al. (2023).

uals to convey critical information about productivity to potential employers, we should observe transitions into salaried work after certification, accompanied by increases in income. However, due to frictions in the labor market (Lain 2019; Narita 2020), we do not necessarily expect these transitions to occur immediately.

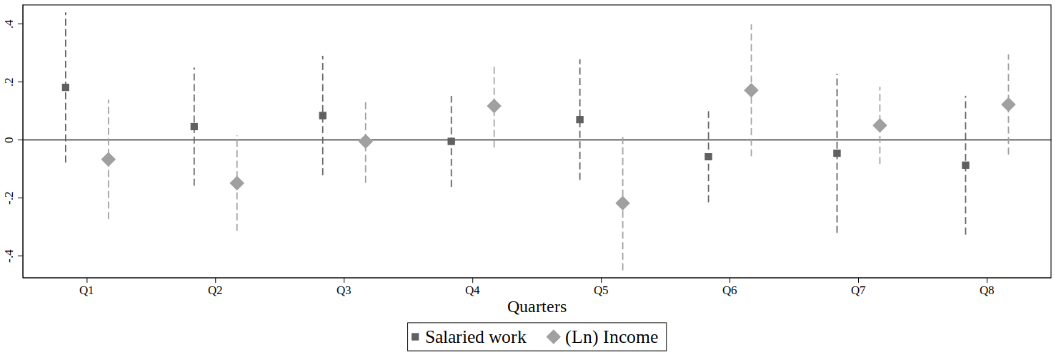
Figure 5 presents estimates for the effect of obtaining an advanced certificate on income and employment outcomes among self-employed individuals at the time of certification.[45] Our findings reveal a positive and statistically significant increase in salaried work, averaging 16.4 percentage points, with this effect becoming evident three calendar quarters after certification. Corresponding increases in income accompany this increase in salaried work. Specifically, our estimates show an average income increase of 15.5% between quarters four and eight, suggesting that potential employers significantly respond to the new information, thereby underscoring the distinctive signaling value of the certificate.

The previous conclusion does not apply to those unemployed at the time of certification. For this group, we find no evidence of increases in either salaried work (Figure 5) or self-employment (Table OA12), and, as expected, no effects on income. One possible reason for the differing outcomes between unemployed and self-employed individuals is that the negative information from employment status may counteract positive information regarding skills.[46] Conversely, individuals attached to the labor market, such as the self-employed, may be better at finding and accepting suitable jobs compared to the unemployed (Blau and Robins 1990; Faberman et al. 2022). Regardless, the evidence suggests that providing information about occupation-specific skills may not be sufficient to help participants transition out of unemployment and improve their income.

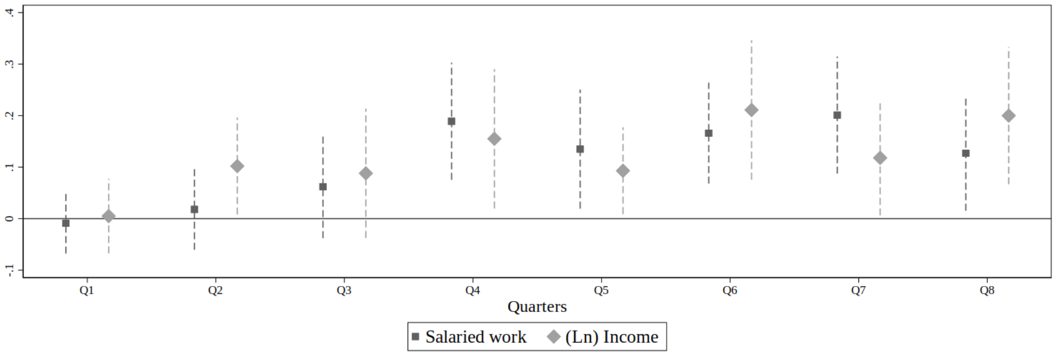
45. The complete set of results, with detailed information on standard errors, number of observations, and bandwidth, are displayed in Table OA12 in online Appendix C. For completeness, Table OA11 in online Appendix C presents the estimates for the effects of obtaining an advanced certificate on income and employment outcomes for the whole sample of men, without subdividing by initial employment status.

46. This explanation aligns with previous literature suggesting that employers are more inclined to hire individuals currently employed to mitigate the risk of hiring subpar candidates, known as lemons (Kugler and Saint-Paul 2004).

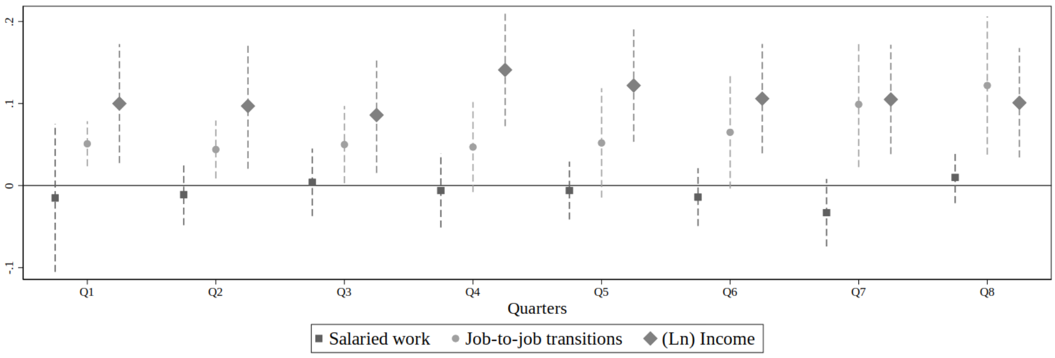
Figure 5: Sharp Regression Discontinuity Estimates of the Short- and Medium-Term Effects of Obtaining an Advanced Certificate by Initial Employment Status




(a) Initial Status: Unemployed




(b) Initial Status: Self-employed




(c) Initial Status: Salaried Work

**Notes:** Each plot displays the estimate for the main coefficient of interest in Equation (1) and its 95% confidence interval, one to eight quarters after certification, for three outcomes based on employment status at certification: unemployed, self-employed, or salaried worker. The three outcomes are salaried work, log of income, and the probability of having changed jobs after certification (for salaried workers only). The running variable is the exam score, and the discontinuity threshold is 90. All regressions include controls described in Section 4, use a triangular kernel, and use the bandwidth that minimizes the mean squared error of the local polynomial regression discontinuity estimator. Standard errors used to compute the 95% confidence intervals are clustered at the technical norm level. Detailed results are displayed in Table OA12 in online Appendix C.

### 6.2.2 The Value of Signaling to Incumbent and Potential Employers: Evidence from Salaried Workers

The certificate also allows salaried workers to signal their skills outside their current firm, thereby narrowing the informational gap between incumbent and potential employers. We begin by presenting evidence of returns for salaried workers and subsequently discuss, to the extent that our data permits, whether this evidence aligns with *direct* responses from potential employers or *indirect* responses from incumbents.

Figure 5 displays the effects on income and employment outcomes up to eight quarters after certification for salaried workers at the time of certification. For all quarters, we observe positive and significant effects on income ranging from 8.6% to 14.1%, with an average increase of 10.7%. In addition, our findings show that the probability of changing jobs (at least once) after certification increases by 6.6 percentage points (46.8%), on average, within the two years after certification. Nonetheless, we find no significant changes in either salaried work or self-employment within the same period (see Table OA12). These results suggest that signaling skills outside the firm trigger transitions to new firms.

To explore potential responses from incumbent employers, Table 5 shows the results for the sample of salaried workers who did not have job-to-job transitions within 3, 12, and 24 months following certification. Estimates show returns ranging between 9.1% and 15.4%. While these estimates may be subject to selection, given that estimation requires splitting the sample by an outcome realized after treatment, the magnitude of the effect provides suggestive evidence that incumbent employers are also reacting to the advanced certificate.

As mentioned in Section 6.1, in the context of asymmetric information between employers, signaling occupation-specific skills to potential employers should remain relevant even among the most experienced salaried workers. To discuss this issue further, we consider three different subsamples: (i) workers with 15 years or less of potential experience, (ii) workers with more than 15 years but less than 30, and (iii) workers with more than 30 years.[47] Figure 6 presents the results on the probability of changing jobs and income for these subsamples.

On the one hand, we find sizable effects on income among less experienced work-

47. Potential experience is calculated by subtracting years of education plus six years from age. The average potential experience in the sample is 27 years (see Table 1).

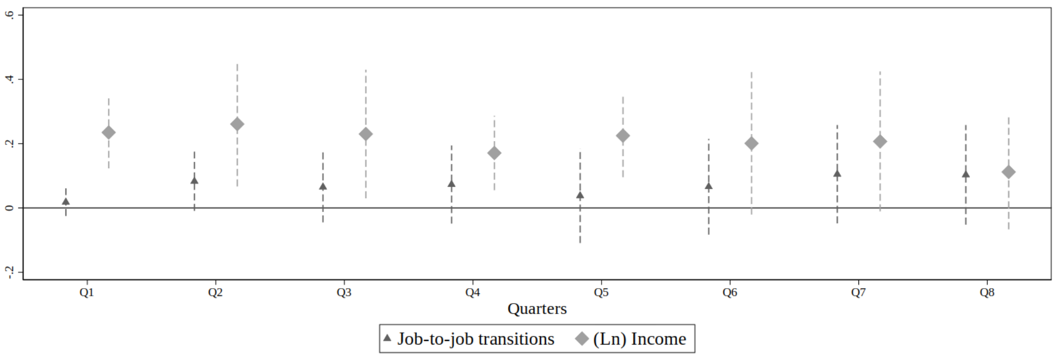
Table 5: Sharp Regression Discontinuity Estimates of the Effects of Obtaining an Advanced Certificate: Salaried Workers Without Job-to-Job Transitions

| **Outcome** | **(1)**<br>**Quarter 1** | **(2)**<br>**Quarter 4** | **(3)**<br>**Quarter 8** |
|---|---|---|---|
| Ln (Income) | 0.111 | 0.154 | 0.091 |
| | (0.038) | (0.036) | (0.032) |
| Observations | 125,524 | 110,886 | 87,052 |
| Control Obs. | 5,653 | 5,009 | 4,594 |
| Treat. Obs. | 10,916 | 9,639 | 9,294 |
| Bandwidth | 2.921 | 2.965 | 3.652 |

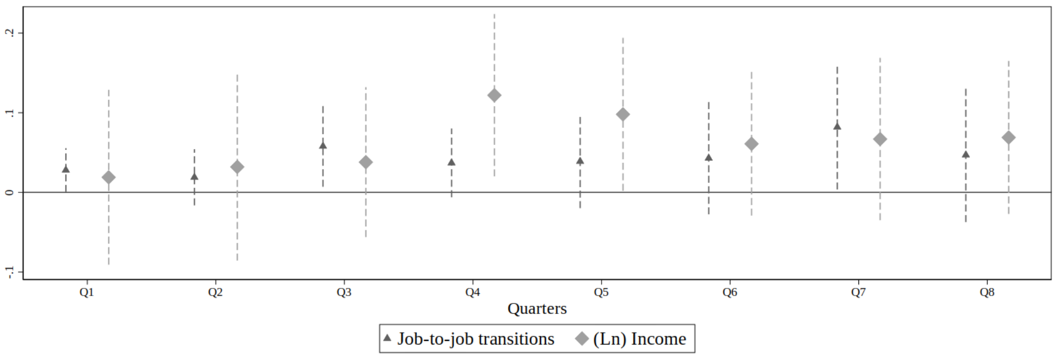
**Notes:** Each cell displays the estimate for the main coefficient of interest in Equation (1). The outcomes are the log of income after one quarter, one year, and two years following certification, respectively, for individuals who did not switch jobs during the corresponding periods . For instance, in column 2, we focus on individuals who did not undergo a job-to-job transition within the first year following certification. All estimates are based on the sample of salaried workers at certification. The running variable is the exam score, and the discontinuity threshold is 90. All regressions include controls described in Section 4, use a triangular kernel, and use the bandwidth that minimizes the mean squared error of the local polynomial regression discontinuity estimator. We report robust bias-corrected standard errors below the point estimates in parentheses. Standard errors are clustered at the technical norm level.

ers. For instance, we estimate an average increase in income of 20.5% for salaried workers with less than 15 years of potential experience. Interestingly, there is no discernible evidence of job mobility, suggesting that wage adjustments may originate within the incumbent firm. On the other hand, while we estimate sizable effects on income among more experienced workers, there is also some evidence of increases in the probability of changing jobs after certification. For example, individuals with 15 to 30 years of potential experience exhibit an average income increase of 6.3%, as well as an average increase in the probability of switching jobs of 4.4 percentage points (28.6%). These results reinforce the notion that potential employers react to the certificate. Importantly, returns for all experience groups are statistically indistinguishable from each other. As such, the evidence suggests that the documented information friction tends to be highly persistent. Overall, our evidence supports the notion that the certificate triggers *direct* responses from prospective employers seeking to attract valuable workers, as well as *indirect* responses from incumbent employers who adjust wages to retain their skilled workforce.

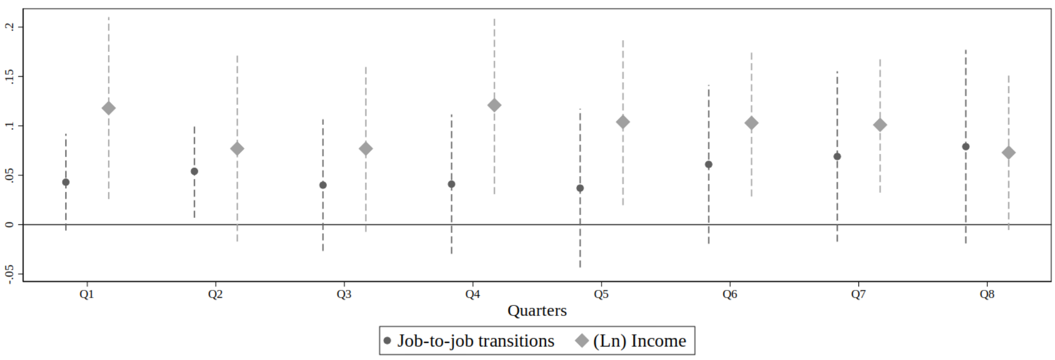
Figure 6: Sharp Regression Discontinuity Estimates of the Short- and Medium-Term Effects of Obtaining an Advanced Certificate for Salaried Workers by Potential Experience




(a) Less than 15 years of potential experience




(b) Between 15 and 30 years of potential experience




(c) More than 30 years of potential experience

**Notes:** Each plot displays the estimate for the main coefficient of interest in Equation (1) and its 95% confidence interval, one to eight quarters after certification, for two outcomes by potential experience: workers with 15 years or less, workers with more than 15 years but less than 30, and workers with more than 30 years. All estimates are based on the sample of salaried workers at certification. The outcomes are the log of income and the probability of having changed jobs after certification. The running variable is the exam score, and the discontinuity threshold is 90. All regressions include controls described in Section 4, use a triangular kernel, and use the bandwidth that minimizes the mean squared error of the local polynomial regression discontinuity estimator. Standard errors used to compute the 95% confidence intervals are clustered at the technical norm level. Detailed results are displayed in Table OA13 in online Appendix C.

## 6.3 Alternative Explanations

### 6.3.1 Signaling Occupation-Specific Skills to Incumbent Employers.

An alternative possibility is that the returns observed within the firm are attributable to the certificate affecting the information set of incumbent employers, leading them to reassess productivity expectations and adjust wages accordingly. This *direct* response from incumbents is likely more pronounced among low-tenured workers, as employers may not have had sufficient time to gauge these workers' productivity and assess if their skills are up-to-date (Altonji 2005; Farber and Gibbons 1996; Jovanovic 1979; Lange 2007). Conversely, this mechanism is potentially irrelevant for high-tenured workers. We posit that, in our context, this possibility is unlikely to be the fundamental driver of returns for several reasons. For the advanced certificate, our sample includes salaried workers with an average tenure of 4.3 years. Additionally, over 65% of those near the threshold for obtaining an advanced certificate have worked in the firm for over five years. Previous research suggests that employers learn quickly, with most learning occurring within three years of tenure (Lange 2007). Therefore, it is improbable that incumbent employers would significantly update their expectations upon observing the certificate.[48]

It is also plausible that the certificate provides incumbent firms an objective criterion for awarding promotions.[49] Given that the certified skills are developed on the job and can be evaluated over time, and considering that the workers in our sample are experienced, current employers most likely have accumulated sufficient information to justify promotions. Therefore, it is unlikely that the certificate alone serves as a definitive criterion for permanently increasing salaries by 10%, particularly among the most experienced employees.

### 6.3.2 Attenuating Workers' Uncertainty About Their Own Skills.

An alternative explanation for the observed increases in income is based on the hypothesis that workers are uncertain about their skill level.[50] According to this

48. Moreover, the skills under consideration primarily pertain to low-paid jobs, making the productivity assessment more likely to occur during the initial years on the job.

49. Benson et al. (2019) discusses the importance of establishing criteria for promotion that do not lead to perceptions of favoritism, unfairness, or the impression that effort in one's job goes unrewarded.

50. For papers using an experimental approach to prove evidence about this channel, see Falk et al. (2006). For papers adopting non-experimental approaches, see, for example, Antonovics and Golan (2012), Golan and Sanders (2019), and Sanders (2014).

explanation, when workers receive an advanced certificate, they learn about their abilities, realize their true prospects in the labor market, and change their behaviors. While workers' uncertainty about their skills might explain part of the effect, it's unlikely to be the main driver for at least two reasons. First, if that were the case, we expect to see a decrease in unemployment as workers transition to self-employment or salaried work.[51] However, our findings don't support this pattern (see Table OA12). Second, this explanation assumes that workers are unaware of their potential. However, it's unclear how to support this assumption in a context where workers have an average of 27 years of potential experience.[52]

# 7 Conclusion

In this paper, we provide causal evidence of the effect of signaling occupation-specific skills on income. We take advantage of a novel program in Colombia that certifies workers' occupation-specific skills. Our study context is unique in that it allows us to directly evaluate the effect of the signal's content, as the certification program offers three levels of certification, which are entirely determined by sharp thresholds: basic, intermediate, and advanced. Using a regression discontinuity design, we estimate returns on earnings up to two years after certification and find that the effects vary significantly with the signal content.

On the one hand, workers with a basic or intermediate certificate experience statistically insignificant returns on earnings within two years. On the other hand, there is a sizable and permanent effect on average earnings (9.7%) for individuals with advanced certificates. We argue that obtaining an advanced certificate mainly impacts earnings by allowing individuals to effectively signal their occupation-specific skills to potential employers, increasing their likelihood of receiving outside offers. Although such a mechanism is particularly prevalent among self-employed individuals, we also find evidence among salaried workers, including the most experienced

51. On the one hand, individuals who receive the certificate may tend to proactively intensify their job search efforts, leading to an upswing in secured employment opportunities (Carranza et al. 2022; Falk et al. 2006; Mueller et al. 2021). Conversely, possessing an advanced certification and learning about one's skills may instill a heightened sense of confidence, fostering the belief in one's capacity to thrive as an entrepreneur and prompting a transition towards self-employment (Asoni 2011; Hamilton et al. 2019; Levine and Rubinstein 2017).

52. Usually, studies in this stream of literature focus on transitions between occupations that happen early in workers' careers. The findings suggest that most learning about one's skills happens early and that experimentation ceases to explain transitions between jobs after a few periods of accumulating job experience. For example, Antonovics and Golan (2012) find that much of the learning occurs in the first seven years.

ones. For salaried workers, results suggest that returns are not only coming from prospective employers but also from incumbent employers, who react to the possibility of losing valuable workers.

Our results provide compelling evidence that certification programs can stimulate post-schooling wage growth among low-educated, experienced workers. The certificate serves as a reliable indicator of productivity, particularly in cases where traditional signals of academic ability are not informative about specific aspects of human capital, such as occupation-specific skills. Additionally, the certificate is valuable when job market history fails to demonstrate the worker's current alignment with constantly evolving occupation-specific standards.

It is important to acknowledge a couple of caveats when interpreting our findings. First, our sample is confined to Colombia, which implies that our results primarily pertain to labor markets characterized by significant information frictions. In this sense, our findings hold broader implications for developing countries sharing similar labor market characteristics. Second, one possible drawback of certification programs is that they can diminish firms' motivations to invest in their employees (Acemoglu and Pischke 1999). However, the motivations for training workers in developed countries may differ from those in our context (see, for example, Caicedo et al. (2022)).

Nonetheless, our findings suggest the existence of information asymmetries among employers even after workers have accumulated considerable labor market experience and the certificate partially corrects them. A key takeaway from our research is that implementing policy measures designed to disclose information pertaining to previously acquired skills, thereby reducing the informational gap between incumbent and potential employers, holds the potential for substantial efficiency gains by increasing job mobility. Due to the rapid evolution of skills demanded in the labor market, policymakers often emphasize the importance of continued development of occupation-specific skills for experienced workers. Nonetheless, according to our findings, another set of effective policy measures involves the design of mechanisms for revealing information about already acquired skills on the job. Moreover, such mechanisms can also incentivize individuals to maintain their skills up-to-date.

# References

Abebe, Girum, A Stefano Caria, Marcel Fafchamps, Paolo Falco, Simon Franklin, and Simon Quinn. 2021. "Anonymity or Distance? Job Search and Labour Market Exclusion in a Growing African City." *The Review of Economic Studies* 88 (3): 1279–1310.

Abel, Martin, Rulof Burger, and Patrizio Piraino. 2020. "The Value of Reference Letters: Experimental Evidence from South Africa." *American Economic Journal: Applied Economics* 12 (3): 40–71.

Acemoglu, Daron, and Jörn-Steffen Pischke. 1999. "The Structure of Wages and Investment in General Training." *Journal of Political Economy* 107 (3): 539–572.

Adhvaryu, Achyuta, Namrata Kala, and Anant Nyshadham. 2023. "Returns to On-the-Job Soft Skills Training." *Journal of Political Economy* 131 (8): 2165–2208.

Agudelo, Sonia A, and Hector Sala. 2017. "Wage Rigidities in Colombia: Measurement, Causes, and Policy Implications." *Journal of Policy Modeling* 39 (3): 547–567.

Aigner, Dennis J., and Glen G. Cain. 1977. "Statistical Theories of Discrimination in Labor Markets." *Industrial and Labor Relations Review* 30 (2): 175–187.

Albert, Kyle. 2017. "The Certification Earnings Premium: An Examination of Young Workers." *Social Science Research* 63:138–149.

Altonji, J. G., and C. R. Pierret. 2001. "Employer Learning and Statistical Discrimination." *The Quarterly Journal of Economics* 116 (1): 313–350.

Altonji, Joseph G. 2005. "Employer Learning, Statistical Discrimination and Occupational Attainment." *American Economic Review* 95 (2): 112–117.

Antonovics, Kate, and Limor Golan. 2012. "Experimentation and Job Choice." *Journal of Labor Economics* 30 (2): 333–366.

Arcidiacono, Peter, Patrick Bayer, and Aurel Hizmo. 2010. "Beyond Signaling and Human Capital: Education and the Revelation of Ability." *American Economic Journal: Applied Economics* 2 (4): 76–104.

Arteaga, Carolina. 2018. "The Effect of Human Capital on Earnings: Evidence From a Reform at Colombia's Top University." *Journal of Public Economics* 157:212, 225.

Aryal, Gaurab, Manudeep Bhuller, and Fabian Lange. 2022. "Signaling and Employer Learning with Instruments." *American Economic Review* 112 (5): 1669–1702.

Asoni, Andrea. 2011. "Intelligence, Self-Confidence and Entrepreneurship."

Baird, Matthew D, Robert Bozick, and Melanie A Zaber. 2021. "Beyond Traditional Academic Degrees: The Labor Market Returns to Occupational Credentials in the United States." *IZA Journal of Labor Economics* 11 (1).

Bandiera, Oriana, Livia Alfonsi, Vittorio Bassi, Robin Burgess, Imran Rasul, Munshi Sulaiman, and Anna Vitali. 2020. "Tackling Youth Unemployment: Evidence from a Labor Market Experiment in Uganda." *Econometrica* 88 (6): 2369–2414.

Bassi, Vittorio, and Aisha Nansamba. 2022. "Screening and Signalling Non-Cognitive Skills: Experimental Evidence from Uganda." *The Economic Journal* 132 (642): 471–511.

Becker, Gary S. 2009. *Human Capital: A Theoretical and Empirical Analysis, with Special Reference to Education.* University of Chicago press.

Bedard, Kelly. 2001. "Human Capital Versus Signaling Models: University Access and High School Dropouts." *Journal of Political Economy* 109 (4): 749–775.

Behrman, Jere R. 1999. "Chapter 43 Labor Markets in Developing Countries," 3:2859–2939. Handbook of Labor Economics. Elsevier.

Benson, Alan, Danielle Li, and Kelly Shue. 2019. "Promotions and the Peter Principle." *The Quarterly Journal of Economics* 134 (4): 2085–2134.

Blair, Peter Q., and Bobby W. Chung. 2021. "A Model of Occupational Licensing and Statistical Discrimination" [in English]. *AEA papers and proceedings* 111:201–205.

———. 2022. "Job Market Signaling through Occupational Licensing." *The Review of Economics and Statistics* (November): 1–45.

Blau, David M, and Philip K Robins. 1990. "Job Search Outcomes for the Employed and Unemployed." *Journal of Political Economy* 98 (3): 637–655.

Busso, Matias, Sebastian Montaño, and Juan S. Muñoz-Morales. 2023. "Signaling Specific Skills and the Labor Market of College Graduates." IZA Discussion Paper 16449.

Caicedo, Santiago, Miguel Espinosa, and Arthur Seibold. 2022. "Unwilling to Train?-Firm Responses to the Colombian Apprenticeship Regulation." *Econometrica* 90 (2): 507–550.

Calonico, Sebastian, Matias D Cattaneo, and Rocío Titiunik. 2014. "Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs." *Econometrica* 82 (6): 2295–2326.

Cameron, Stephen V, and James J Heckman. 1993. "The Nonequivalence of High School Equivalents." *Journal of Labor Economics* 11 (1, Part 1): 1–47.

Carranza, Eliana, Robert Garlick, Kate Orkin, and Neil Rankin. 2022. "Job Search and Hiring with Limited Information about Workseekers' Skills." *American Economic Review* 112, no. 11 (November): 3547–3583.

Cattaneo, Matias D., Nicolás Idrobo, and Rocío Titiunik. 2020. *A Practical Introduction to Regression Discontinuity Designs: Foundations.* Elements in Quantitative and Computational Methods for the Social Sciences. Cambridge University Press.

Cattaneo, Matias D., Nicolás Idrobo, and Rocıo Titiunik. 2022. *A Practical Introduction to Regression Discontinuity Designs: Extensions.* Elements in Quantitative and Computational Methods for the Social Sciences. Cambridge University Press.

Cattaneo, Matias D., and Rocío Titiunik. 2022. "Regression Discontinuity Designs." Working Paper, February.

Clark, Damon, and Paco Martorell. 2014. "The Signaling Value of a High School Diploma." *Journal of Political Economy* 122 (2): 282–318.

Faberman, R Jason, Andreas I Mueller, Ayşegül Şahin, and Giorgio Topa. 2022. "Job Search Behavior Among the Employed and Non-Employed." *Econometrica* 90 (4): 1743–1779.

Falk, Armin, David Huffman, and Uwe Sunde. 2006. "Self-Confidence and Search."

Farber, Henry S, and Robert Gibbons. 1996. "Learning and Wage Dynamics." *The Quarterly Journal of Economics* 111 (4): 1007–1047.

Feng, Andy, and Georg Graetz. 2017. "A Question of Degree: The Effects of Degree Class on Labor Market Outcomes." *Economics of Education Review* 61:140–161.

Frandsen, Brigham R. 2017. "Party Bias in Union Representation Elections: Testing for Manipulation in the Regression Discontinuity Design when the Running Variable is Discrete." In *Regression Discontinuity Designs,* 281–315. Emerald Publishing Limited, May.

Freier, Ronny, Mathias Schumann, and Thomas Siedler. 2015. "The Earnings Returns to Graduating with Honors—Evidence From Law Graduates." *Labour Economics* 34:39–50.

Garcıa-Suaza, Andrés Felipe, Juan Carlos Guataquı, José Alberto Guerra, and Darıo Maldonado. 2014. "Beyond the Mincer Equation: The Internal Rate of Return to Higher Education in Colombia." *Education Economics* 22 (3): 328–344.

Gibbons, Robert, and Michael Waldman. 1999. "A Theory of Wage and Promotion Dynamics Inside Firms." *The Quarterly Journal of Economics* 114 (4): 1321–1358.

———. 2004. "Task-Specific Human Capital." *American Economic Review* 94 (2): 203–207.

Golan, Limor, and Carl Sanders. 2019. "Racial Gaps, Occupational Matching, and Skill Uncertainty."

Graetz, Georg. 2021. "On the interpretation of diploma wage effects estimated by regression discontinuity designs." *Canadian Journal of Economics/Revue canadienne d'économique* 54 (1): 228–258.

Groh, Matthew, David McKenzie, Nour Shammout, and Tara Vishwanath. 2015. "Testing the Importance of Search Frictions and Matching Through a Randomized Experiment in Jordan." *IZA Journal of Labor Economics* 4:1–20.

Hamilton, Barton H, Nicholas W Papageorge, and Nidhi Pande. 2019. "The Right Stuff? Personality and Entrepreneurship." *Quantitative Economics* 10 (2): 643–691.

Herrera-Idárraga, Paula, Enrique López-Bazo, and Elisabet Motellón. 2015. "Double Penalty in Returns to Education: Informality and Educational Mismatch in the Colombian Labour Market." *The Journal of Development Studies* 51 (12): 1683–1701.

Jepsen, Christopher, Peter Mueser, and Kenneth Troske. 2016. "Labor Market Returns to the GED Using Regression Discontinuity Analysis." *Journal of Political Economy* 124 (3): 621–649.

Jovanovic, Boyan. 1979. "Job Matching and The Theory of Turnover." *Journal of Political Economy* 87 (5, Part 1): 972–990.

Kahn, Lisa B. 2013. "Asymmetric Information Between Employers." *American Economic Journal: Applied Economics* 5 (4): 165–205.

Kambourov, Gueorgui, and Iourii Manovskii. 2009. "Occupational Specificity of Human Capital." *International Economic Review* 50 (1): 63–115.

Khoo, Pauline, and Ben Ost. 2018. "The Effect of Graduating with Honors on Earnings." *Labour Economics* 55:149–162.

Kleiner, Morris M, and Alan B Krueger. 2013. "Analyzing the Extent and Influence of Occupational Licensing on the Labor Market." *Journal of Labor Economics* 31 (S1): S173–S202.

Kleiner, Morris M, and Evgeny Vorotnikov. 2017. "Analyzing Occupational Licensing among the States." *Journal of Regulatory Economics* 52:132–158.

Kugler, Adriana D, and Gilles Saint-Paul. 2004. "How Do Firing Costs Affect Worker Flows in a World With Adverse Selection?" *Journal of Labor Economics* 22 (3): 553–584.

Lain, Jonathan. 2019. "Discrimination in a Search and Matching Model with Self-Employment." *IZA Journal of Development and Migration* 9 (1): 6.

Lang, Kevin, and Ariella Kahn-Lang Spitzer. 2020. "Race Discrimination: An Economic Perspective." *The Journal of Economic Perspectives* 34 (2): 68–89.

Lange, Fabian. 2007. "The Speed of Employer Learning." *Journal of Labor Economics* 25 (1): 1–35.

Lee, David S., and Thomas Lemieux. 2010. "Regression Discontinuity Designs in Economics." *Journal of Economic Literature* 48 (2): 281–355.

Levine, Ross, and Yona Rubinstein. 2017. "Smart and Illicit: Who Becomes an Entrepreneur and Do They Earn More?" *The Quarterly Journal of Economics* 132 (2): 963–1018.

Machin, Stephen, Sandra McNally, and Jenifer Ruiz-Valenzuela. 2020. "Entry Through the Narrow Door: The Costs of Just Failing High Stakes Exams." *Journal of Public Economics* 190:104224.

Macleod, W. Bentley, Evan Riehl, Juan E. Saavedra, and Miguel Urquiola. 2017. "The Big Sort: College Reputation and Labor Market Outcomes." *American Economic Journal: Applied Economics* 9 (3): 223–61.

Morales, Leonardo Fabio, Christian Posso, and Luz A Flórez. 2021. "Heterogeneity in the Returns to Tertiary Education for the Disadvantage Youth: Quality vs. Quantity Analysis." *Borradores de Economıa; No. 1150.*

Mueller, Andreas I, Johannes Spinnewijn, and Giorgio Topa. 2021. "Job Seekers' Perceptions and Employment Prospects: Heterogeneity, Duration Dependence, and Bias." *American Economic Review* 111 (1): 324–363.

Narita, Renata. 2020. "Self-Employment in Developing Countries: A Search-Equilibrium Approach." *Review of Economic Dynamics* 35:1–34.

Neal, Derek. 1995. "Industry-Specific Human Capital: Evidence from Displaced Workers." *Journal of Labor Economics* 13 (4): 653–677.

Parent, Daniel. 2000. "Industry-Specific Capital and the Wage Profile: Evidence from the National Longitudinal Survey of Youth and the Panel Study of Income Dynamics." *Journal of Labor Economics* 18 (2): 306–323.

Pinkston, Joshua C. 2009. "A Model of Asymmetric Employer Learning with Testable Implications." *The Review of Economic Studies* 76 (1): 367–394.

Poletaev, Maxim, and Chris Robinson. 2008. "Human Capital Specificity: Evidence from the Dictionary of Occupational Titles and Displaced Worker Surveys, 1984–2000." *Journal of Labor Economics* 26 (3): 387–420.

Postel–Vinay, Fabien, and Jean–Marc Robin. 2002. "Equilibrium Wage Dispersion with Worker and Employer Heterogeneity." *Econometrica* 70 (6): 2295–2350.

Postel-Vinay, Fabien, and Jean-Marc Robin. 2002. "The Distribution of Earnings in an Equilibrium Search Model with State-Dependent Offers and Counteroffers." *International Economic Review* 43 (4): 989–1016.

Rosenzweig, Mark R. 1988. “Chapter 15 Labor Markets in Low-Income Countries,” 1:713–762. Handbook of Development Economics. Elsevier.

Rubinstein, Yona, and Yoram Weiss. 2006. “Post schooling Wage Growth: Investment, Search and Learning.” *Handbook of the Economics of Education* 1:1–67.

Sanders, Carl. 2014. “Skill Accumulation, Skill Uncertainty, and Occupational Choice.” *Working Paper.*

Sanders, Carl, and Christopher Taber. 2012. “Life-Cycle Wage Growth and Heterogeneous Human Capital.” *Annual Review of Economics* 4:399–425.

Schönberg, Uta. 2007. “Testing for Asymmetric Employer Learning.” *Journal of Labor Economics* 25 (4): 651–691.

Spence, Michael. 1973. “Job Market Signaling.” *The Quarterly Journal of Economics* 87, no. 3 (August): 355–374.

———. 1974. “Competitive and Optimal Responses to Signals: An Analysis of Efficiency and Distribution.” *Journal of Economic theory* 7 (3): 296–332.

Tyler, John H, Richard J Murnane, and John B Willett. 2000. “Estimating the Labor Market Signaling Value of the GED.” *The Quarterly Journal of Economics* 115 (2): 431–468.

Weiss, Andrew. 1995. “Human Capital vs. Signalling Explanations Of Wages.” *Journal of Economic Perspectives* 9 (4): 133–154.

Xia, Xing. 2021. “Barrier to Entry or Signal of Quality? The Effects of Occupational Licensing on Minority Dental Assistants.” *Labour Economics* 71:102027.

# Online Appendix A. Additional Figures and Tables

Figure OA1: Example of an Advanced Certificate


Libertad y orden
RÉPUBLICA DE COLOMBIA

El Servicio Nacional de Aprendizaje SENA

*En Cumplimiento del Decreto 933 de 2003*
*otorga*
*Certificado de Competencia Laboral a*
[redacted]
Con CÉDULA DE CIUDADANÍA No. [redacted]
Quien demostró su Competencia Laboral en la Norma



**NIVEL AVANZADO** **Transportar usuarios en los vehículos automotores de servicio especial de acuerdo a normatividad vigente.**
*Código: 280601013*

*En testimonio de lo anterior, se firma el presente en CHÍA, A los veintiuno (21) días del mes de Junio de dos mil diecisiete (2017)*



Firmado Digitalmente por
[redacted]

86097 - 21/06/2017
No Y FECHA REGISTRO

VIGENCIA
21 de Junio de 2020

La autenticidad de este documento puede ser verificada en el registro electrónico que se encuentra en la página web http://certificados.sena.edu.co, bajo el número 951300280601013CC11435671C.

Table OA1: Top 10 Technical Norms: Matched and Unmatched Samples

**Matched Sample**

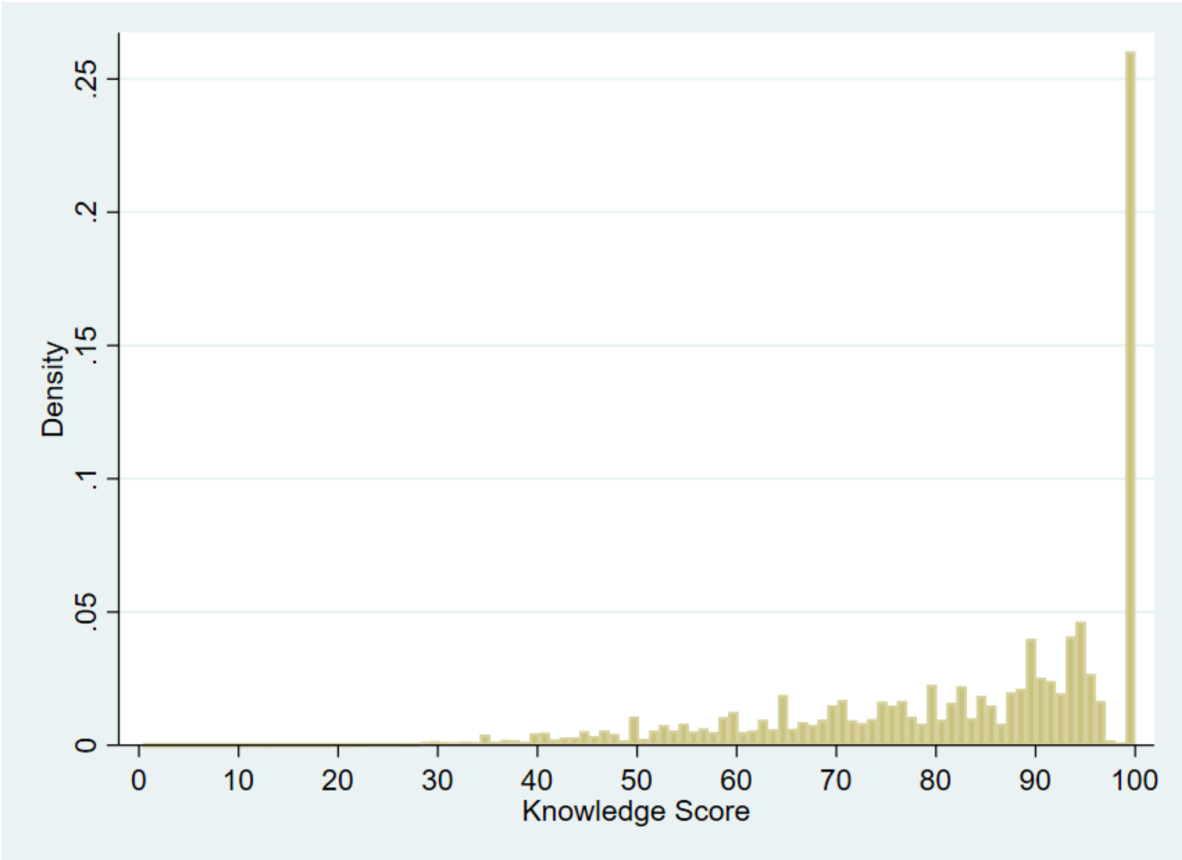
| Ranking in Matched | Name | Share of Participants | Ranking in Unmatched |
|---|---|---|---|
| 1 | Serve customers in accordance with service procedures and regulations. | 0.10 | 1 |
| 2 | To guide classroom training in accordance with technical procedures and regulations. | 0.05 | 3 |
| 3 | Control access in accordance with private security regulations. | 0.04 | 5 |
| 4 | Operate forklifts according to the technical manual. | 0.04 | 23 |
| 5 | Handle food according to current standards. | 0.02 | 2 |
| 6 | Operate the hydraulic excavator according to the technical manual. | 0.02 | 17 |
| 7 | Drive light vehicles according to technical procedures and traffic and transportation regulations. | 0.02 | 7 |
| 8 | Prevent security and surveillance incidents by technical regulations. | 0.02 | 15 |
| 9 | Prepare light vehicles in accordance with legal and technical regulations. | 0.02 | 9 |
| 10 | To drive inter-municipal or special passenger service motor vehicles, category c2, by the regulations in force. | 0.02 | 30 |

**Un-Matched Sample**

| Ranking in Unmacthed | Name | Share of Participants | Ranking in Matched |
|---|---|---|---|
| 1 | Serve customers in accordance with service procedures and regulations. | 0.21 | 1 |
| 2 | Handle food according to current standards. | 0.07 | 5 |
| 3 | To guide classroom training in accordance with technical procedures and regulations. | 0.07 | 2 |
| 4 | Administer immunobiological according to delegation and health regulations. | 0.05 | 76 |
| 5 | Control access in accordance with private security regulations. | 0.04 | 3 |
| 6 | Orient people according to health standards. | 0.03 | 65 |
| 7 | Drive light vehicles according to technical procedures and traffic and transportation regulations. | 0.03 | 7 |
| 8 | Collect potentially recyclable solid waste according to established procedures and current regulations. | 0.02 | 110 |
| 9 | Prepare light vehicles in accordance with legal and technical regulations. | 0.02 | 9 |
| 10 | Transfer users in accordance with coexistence, transit, and land transportation regulations. | 0.02 | 12 |

**Notes:** This table displays the top 10 technical norms in the matched (estimation) and unmatched samples of men.

Figure OA2: Distribution of Scores




**Notes:** This figure displays the distribution of scores in the second part of the certification exam (knowledge test) for the matched sample of men.

Table OA2: Descriptive Statistics: Estimation Sample of Men and Women

| Variable | # Observations | Mean | Std. dev. |
|---|---|---|---|
| Employed | 2,043,639 | 0.88 | 0.32 |
| Income | 2,043,639 | 1,186,950 | 983,688 |
| Salaried Work | 2,043,639 | 0.74 | 0.44 |
| Self-employment | 2,043,639 | 0.12 | 0.32 |
| Ln of Income | 1,800,266 | 13.97 | 0.56 |
| Ln of Income - Salaried Worker | 1,503,237 | 13.99 | 0.55 |
| Ln of Income - Self-Employed | 237,918 | 13.90 | 0.35 |

**Notes:** The table shows descriptive statistics for the panel of men and women two years after certification. Employment and income information are calculated using PILA data. Individuals are classified as employed if they are categorized as salaried workers, self-employed, or if they are assigned to any other category in which employers make contributions to the social security system on their behalf. Therefore, the employment rate is larger than the sum of self-employment and salaried work. The income variable contains zeros in periods when individuals are not employed.

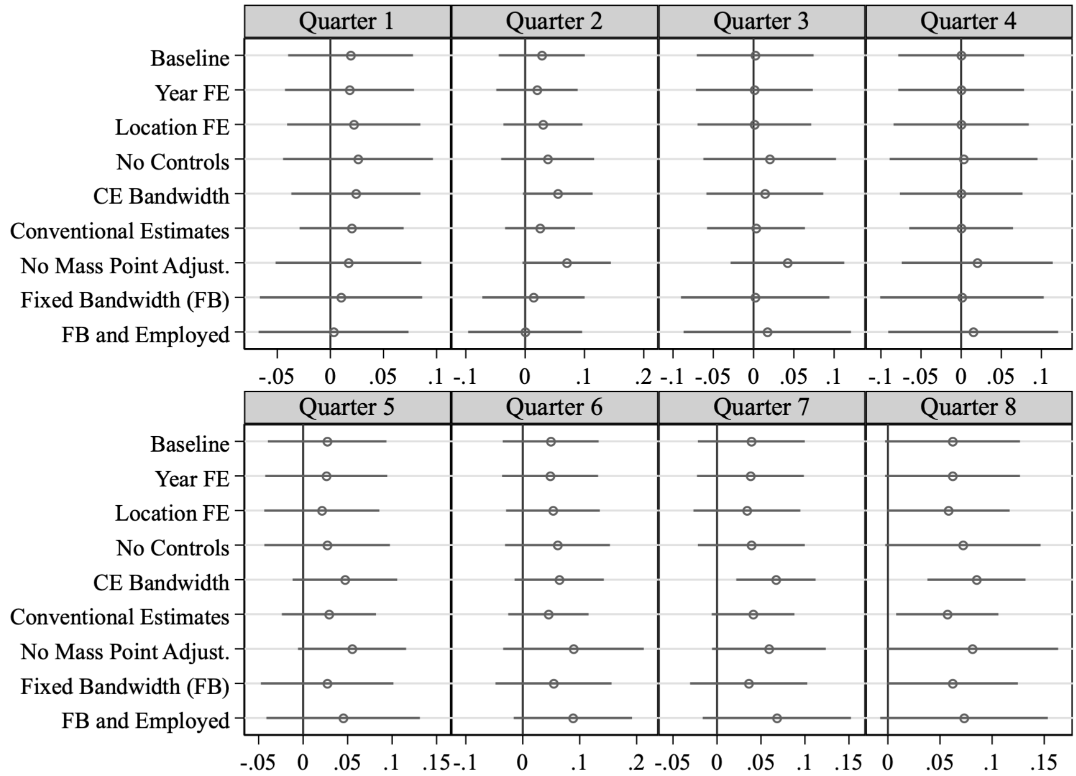
# Online Appendix B. Robustness Checks

Table OA3: Sharp Regression Discontinuity Estimates of the Short- and Medium-Term Effects of Obtaining a Basic, Intermediate, and Advanced Certificate on Income

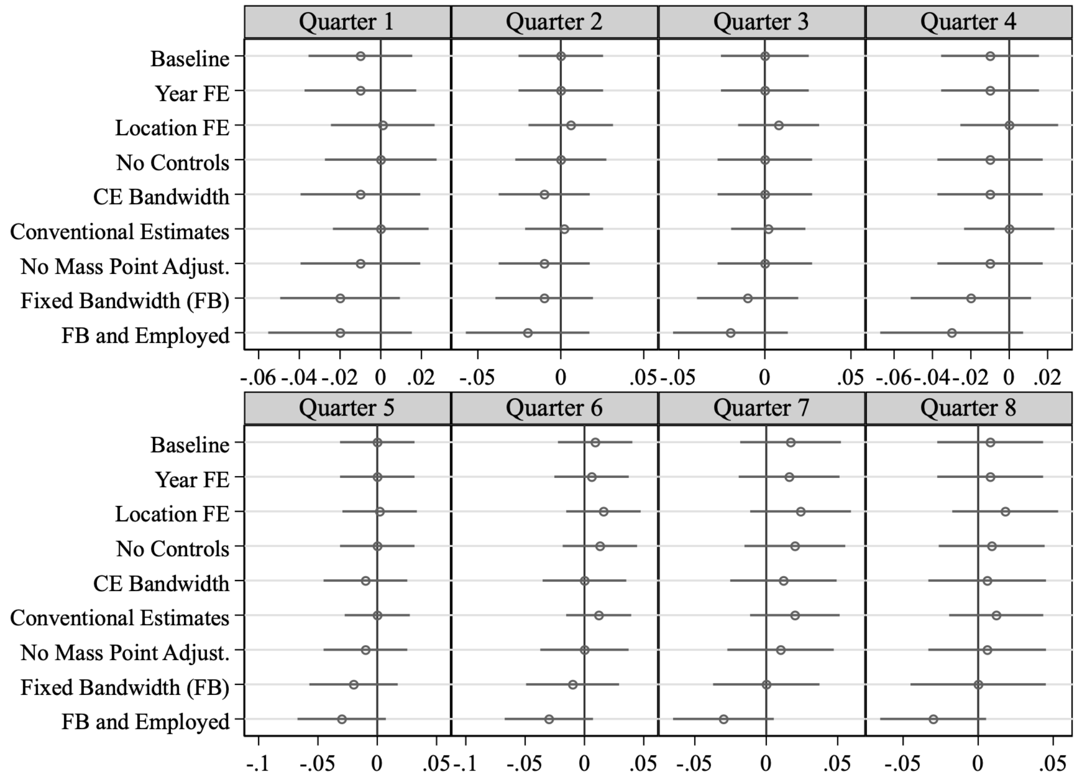
| | **(1) Quarter 1** | **(2) Quarter 2** | **(3) Quarter 3** | **(4) Quarter 4** | **(5) Quarter 5** | **(6) Quarter 6** | **(7) Quarter 7** | **(8) Quarter 8** |
|---|---|---|---|---|---|---|---|---|
| Basic Certificate | -11.268 | 12.325 | 14.312 | 34.469 | -2.758 | -6.750 | -23.010 | -48.701 |
| | (37.991) | (56.370) | (51.004) | (56.670) | (49.290) | (61.441) | (56.371) | (61.426) |
| Observations | 181,395 | 181,395 | 181,395 | 181,395 | 181,395 | 181,395 | 181,395 | 164,296 |
| Control Obs. | 722 | 720 | 773 | 721 | 644 | 641 | 656 | 719 |
| Treat. Obs. | 2562 | 2546 | 2889 | 2561 | 2199 | 2191 | 2200 | 2300 |
| Bandwidth | 7.843 | 7.475 | 8.342 | 7.621 | 6.468 | 6.103 | 6.732 | 7.150 |
| Mean | 854.1 | 851.9 | 856.8 | 848.0 | 849.7 | 862.2 | 880.9 | 872.4 |
| Intermediate Certificate | -14.686 | -3.579 | -11.798 | -34.853 | -31.017 | -17.775 | 2.302 | 1.477 |
| | (23.572) | (23.004) | (22.893) | (21.509) | (25.215) | (21.968) | (23.989) | (23.611) |
| Observations | 181,395 | 181,395 | 181,395 | 181,395 | 181,395 | 181,395 | 181,395 | 164,296 |
| Eff. # of Control Obs. | 10642 | 11139 | 13169 | 11127 | 11127 | 14433 | 11128 | 12043 |
| Eff. # of Treat. Obs. | 15391 | 17152 | 19945 | 17102 | 17068 | 24807 | 17123 | 18324 |
| Bandwidth | 8.890 | 9.684 | 10.05 | 9.290 | 9.103 | 12.23 | 9.529 | 10.98 |
| Mean | 1,010.2 | 1,008.4 | 1,022.1 | 1,025.0 | 1,017.3 | 1,030.5 | 1,035.0 | 1,036.0 |
| Advanced Certificate | 118.847 | 133.427 | 133.932 | 160.513 | 209.209 | 160.135 | 128.697 | 179.001 |
| | (46.048) | (57.677) | (55.030) | (57.401) | (65.843) | (51.583) | (50.741) | (59.829) |
| Observations | 181,395 | 181,395 | 181,395 | 181,395 | 181,395 | 181,395 | 181,395 | 164,296 |
| Eff. # of Control Obs. | 11721 | 8837 | 8837 | 8847 | 7590 | 8848 | 8874 | 6961 |
| Eff. # of Treat. Obs. | 26473 | 20001 | 20001 | 20019 | 16284 | 20019 | 20057 | 14269 |
| Bandwidth | 4.040 | 3.337 | 3.338 | 3.421 | 2.622 | 3.457 | 3.569 | 2.760 |
| Mean | 1,100.7 | 1,119.2 | 1,131.7 | 1,138.6 | 1,135.5 | 1,147.6 | 1,154.5 | 1,150.8 |

**Notes:** Each cell displays the estimate for the main coefficient of interest in Equation (1), one to eight quarters after certification, for each discontinuity threshold. The outcome is income in levels. The running variable is the exam score, and the three discontinuity thresholds are 30 (first panel), 60 (second panel), and 90 (third panel). All regressions include the controls described in Section 4, use a triangular kernel, and use the bandwidth that minimizes the mean squared error of the local polynomial regression discontinuity estimator. The total number of observations drops in quarter 8 due to missing information in the last quarter for individuals that certified in the last quarter of 2019. The effective number of observations changes across quarters given variation in the optimal bandwidth. Robust bias-corrected standard errors are reported below the point estimates in parentheses. Standard errors are clustered at the technical-norm level. The sample mean for the control group is displayed below the optimal bandwidth.

Figure OA3: Sharp Regression Discontinuity Estimates of the Effects of Obtaining a Certificate on Log of Income: Robustness Checks

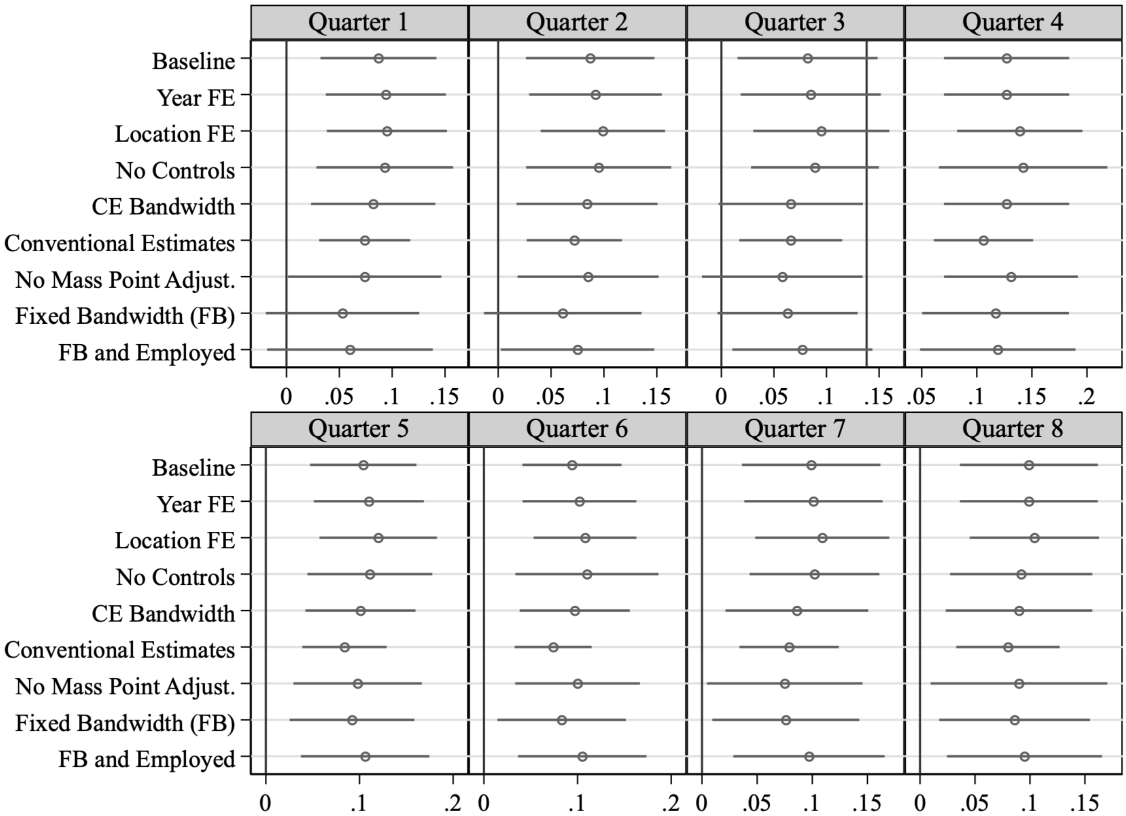



(a) Basic




(b) Intermediate

Figure OA3 Continued. Sharp Regression Discontinuity Estimates of the Effects of Obtaining a Certificate on Log of Income: Robustness Checks




(c) Advanced

**Notes:** Each plot displays the estimate for the main coefficient of interest in Equation (1) and its 95% confidence interval, one to eight quarters after certification. The outcome is the log of income. The running variable is the exam score, and the discontinuity threshold is 30 for the basic certificate (panel a), 60 for the intermediate certificate (panel b), and 90 for the advanced certificate (panel c). In each plot, row 1 displays the baseline results from Table 4. Rows 2 and 3 show the results when we add year and location fixed effects (FE), respectively. Row 4 excludes controls. Row 5 uses the bandwidth that minimizes the coverage error (CE) of the local polynomial regression discontinuity estimator. Row 6 reports non-bias-corrected estimates. Row 7 does not adjust for the presence of mass points. Row 8 uses a fixed bandwidth. Row 9 uses a fixed bandwidth and restricts attention to individuals who are employed in all eight quarters. All specifications include controls described in Section 4, use a triangular kernel, and choose the bandwidth to minimize the mean squared error (MSE) of the local polynomial RD estimator, except for rows 5, 8, and 9. Standard errors used to compute 95% confidence intervals are clustered at the technical-norm level. Detailed results for all three certificates are displayed in Tables OA4, OA5, and OA6.

Table OA4: Sharp Regression Discontinuity Estimates of the Effects of Obtaining a Basic Certificate on Log of Income: Robustness Checks

| | **(1) Quarter 1** | **(2) Quarter 2** | **(3) Quarter 3** | **(4) Quarter 4** | **(5) Quarter 5** | **(6) Quarter 6** | **(7) Quarter 7** | **(8) Quarter 8** |
|---|---|---|---|---|---|---|---|---|
| 1. Baseline | 0.019 | 0.028 | 0.002 | -0.009 | 0.027 | 0.049 | 0.039 | 0.062 |
| | (0.030) | (0.037) | (0.037) | (0.040) | (0.034) | (0.043) | (0.031) | (0.033) |
| Eff. # of Control Obs. | 756 | 633 | 698 | 713 | 617 | 680 | 611 | 641 |
| Eff. # of Treatment Obs. | 3,617 | 2,237 | 2,743 | 3,510 | 2,188 | 2,648 | 2,139 | 2,182 |
| Bandwidth | 10.140 | 7.738 | 9.994 | 10.470 | 7.661 | 9.038 | 7.203 | 8.306 |
| | | | | | | | | |
| 2. Year FE | 0.018 | 0.020 | 0.001 | -0.009 | 0.026 | 0.048 | 0.038 | 0.062 |
| | (0.031) | (0.035) | (0.037) | (0.040) | (0.035) | (0.043) | (0.031) | (0.033) |
| Eff. # of Control Obs. | 715 | 704 | 739 | 713 | 616 | 681 | 610 | 641 |
| Eff. # of Treatment Obs. | 2,821 | 2,760 | 3,511 | 3,510 | 2,174 | 2,660 | 2,126 | 2,182 |
| Bandwidth | 9.937 | 9.359 | 10.200 | 10.470 | 7.432 | 9.223 | 7.138 | 8.306 |
| | | | | | | | | |
| 3. Location FE | 0.022 | 0.030 | 0.001 | -0.003 | 0.021 | 0.053 | 0.034 | 0.058 |
| | (0.032) | (0.034) | (0.036) | (0.043) | (0.033) | (0.042) | (0.031) | (0.030) |
| Eff. # of Control Obs. | 644 | 676 | 672 | 648 | 615 | 680 | 612 | 600 |
| Eff. # of Treatment Obs. | 2,294 | 2,523 | 2,510 | 2,504 | 2,173 | 2,456 | 2,140 | 1,935 |
| Bandwidth | 7.584 | 8.109 | 8.188 | 8.086 | 7.214 | 9.005 | 7.296 | 7.853 |
| | | | | | | | | |
| 4. No Controls | 0.026 | 0.038 | 0.020 | 0.003 | 0.027 | 0.061 | 0.039 | 0.072 |
| | (0.036) | (0.040) | (0.042) | (0.047) | (0.036) | (0.047) | (0.031) | (0.038) |
| Eff. # of Control Obs. | 645 | 574 | 628 | 648 | 682 | 657 | 656 | 599 |
| Eff. # of Treatment Obs. | 2,295 | 1,934 | 2,224 | 2,504 | 2,708 | 2,439 | 2,453 | 1,918 |
| Bandwidth | 7.643 | 6.795 | 7.591 | 8.200 | 9.447 | 8.133 | 8.919 | 7.245 |
| | | | | | | | | |
| 5. CE Bandwidth | 0.024 | 0.055 | 0.014 | -0.001 | 0.047 | 0.064 | 0.067 | 0.085 |
| | (0.031) | (0.030) | (0.037) | (0.039) | (0.030) | (0.040) | (0.023) | (0.024) |
| Eff. # of Control Obs. | 643 | 484 | 621 | 604 | 472 | 546 | 470 | 462 |
| Eff. # of Treatment Obs. | 2,277 | 1,758 | 2,191 | 2,198 | 1,709 | 1,854 | 1,658 | 1,503 |
| Bandwidth | 7.186 | 5.484 | 7.081 | 7.416 | 5.431 | 6.405 | 5.105 | 5.895 |

Continues in the next page...

Table OA4 Continued. Sharp Regression Discontinuity Estimates of the Effects of Obtaining a Basic Certificate on Log of Income: Robustness Checks

| | **(1) Quarter 1** | **(2) Quarter 2** | **(3) Quarter 3** | **(4) Quarter 4** | **(5) Quarter 5** | **(6) Quarter 6** | **(7) Quarter 7** | **(8) Quarter 8** |
|---|---|---|---|---|---|---|---|---|
| 6. Conventional Estimates | 0.020 | 0.025 | 0.003 | -0.007 | 0.029 | 0.045 | 0.041 | 0.057 |
| | (0.025) | (0.030) | (0.031) | (0.033) | (0.027) | (0.036) | (0.024) | (0.025) |
| Eff. # of Control Obs. | 756 | 633 | 698 | 713 | 617 | 680 | 611 | 641 |
| Eff. # of Treatment Obs. | 3,617 | 2,237 | 2,743 | 3,510 | 2,188 | 2,648 | 2,139 | 2,182 |
| Bandwidth | 10.140 | 7.738 | 9.994 | 10.470 | 7.661 | 9.038 | 7.203 | 8.306 |
| 7. No Mass Points Adjustment | 0.017 | 0.070 | 0.042 | 0.020 | 0.055 | 0.089 | 0.059 | 0.081 |
| | (0.035) | (0.038) | (0.036) | (0.048) | (0.031) | (0.063) | (0.033) | (0.042) |
| Eff. # of Control Obs. | 326 | 321 | 410 | 298 | 413 | 311 | 309 | 302 |
| Eff. # of Treatment Obs. | 800 | 777 | 949 | 745 | 936 | 743 | 743 | 659 |
| Bandwidth | 3.689 | 3.514 | 4.077 | 3.029 | 4.398 | 3.704 | 3.618 | 3.060 |
| 8. Fixed Bandwidth | 0.010 | 0.014 | 0.002 | 0.001 | 0.027 | 0.054 | 0.036 | 0.062 |
| | (0.039) | (0.044) | (0.047) | (0.052) | (0.038) | (0.052) | (0.034) | (0.032) |
| Eff. # of Control Obs. | 690 | 679 | 675 | 650 | 660 | 660 | 656 | 641 |
| Eff. # of Treatment Obs. | 2,592 | 2,527 | 2,515 | 2,508 | 2,483 | 2,444 | 2,441 | 2,187 |
| Bandwidth | 8.600 | 8.600 | 8.600 | 8.600 | 8.600 | 8.600 | 8.600 | 8.600 |
| 9. Fixed Bandwidth and | 0.003 | -0.001 | 0.017 | 0.015 | 0.045 | 0.088 | 0.068 | 0.073 |
| Always Employed | (0.036) | (0.049) | (0.053) | (0.054) | (0.044) | (0.053) | (0.043) | (0.041) |
| Eff. # of Control Obs. | 461 | 461 | 461 | 461 | 461 | 461 | 461 | 461 |
| Eff. # of Treatment Obs. | 1,557 | 1,557 | 1,557 | 1,557 | 1,557 | 1,557 | 1,557 | 1,557 |
| Bandwidth | 8.600 | 8.600 | 8.600 | 8.600 | 8.600 | 8.600 | 8.600 | 8.600 |

**Notes:** Each cell displays the estimate for the main coefficient of interest in Equation (1), one to eight quarters after certification. The outcome is the log of income. The running variable is the exam score, and the discontinuity threshold is 30. Row 1 displays the baseline results from Table 4. Rows 2 and 3 show the results when we add year and location fixed effects (FE), respectively. Row 4 excludes controls. Row 5 uses the bandwidth that minimizes the coverage error (CE) of the local polynomial regression discontinuity estimator. Row 6 reports non-bias-corrected estimates. Row 7 does not adjust for the presence of mass points. Row 8 uses a fixed bandwidth. Row 9 also uses a fixed bandwidth and restricts attention to individuals that are employed in all eight quarters. All specifications include controls described in Section 4, use a triangular kernel, and choose the bandwidth to minimize the mean squared error (MSE) of the local polynomial RD estimator, except for rows 5, 8, and 9. In all specifications, except for row 7, we report robust bias-corrected standard errors below the point estimates in parentheses. Standard errors are clustered at the technical-norm level. Bandwidths are displayed below the effective number of observations. The effective number of observations changes across quarters given variation in the number of individuals with positive earnings and the optimal bandwidth.

Table OA5: Sharp Regression Discontinuity Estimates of the Effects of Obtaining an Intermediate Certificate on Log of Income: Robustness Checks

| | **(1)** **Quarter 1** | **(2)** **Quarter 2** | **(3)** **Quarter 3** | **(4)** **Quarter 4** | **(5)** **Quarter 5** | **(6)** **Quarter 6** | **(7)** **Quarter 7** | **(8)** **Quarter 8** |
|---|---|---|---|---|---|---|---|---|
| 1. Baseline | -0.010 | -0.002 | -0.001 | -0.013 | -0.006 | 0.009 | 0.017 | 0.008 |
| | (0.013) | (0.013) | (0.013) | (0.013) | (0.016) | (0.016) | (0.018) | (0.018) |
| Eff. # of Control Obs. | 9,611 | 9,921 | 9,808 | 9,319 | 11,296 | 11,248 | 11,393 | 10,147 |
| Eff. # of Treatment Obs. | 13,679 | 15,196 | 14,987 | 13,323 | 17,176 | 17,302 | 17,182 | 15,226 |
| Bandwidth | 8.798 | 9.714 | 9.357 | 8.753 | 10.300 | 10.790 | 11.000 | 10.410 |
| | | | | | | | | |
| 2. Year FE | -0.012 | -0.006 | -0.003 | -0.013 | -0.008 | 0.006 | 0.016 | 0.008 |
| | (0.014) | (0.013) | (0.013) | (0.013) | (0.016) | (0.016) | (0.018) | (0.018) |
| Eff. # of Control Obs. | 9,608 | 9,913 | 9,808 | 9,319 | 11,295 | 11,248 | 11,128 | 10,147 |
| Eff. # of Treatment Obs. | 13,645 | 15,163 | 14,983 | 13,323 | 17,176 | 17,071 | 17,179 | 15,226 |
| Bandwidth | 8.653 | 9.544 | 9.272 | 8.753 | 10.200 | 10.540 | 10.910 | 10.410 |
| | | | | | | | | |
| 3. Location FE | 0.001 | 0.006 | 0.008 | -0.005 | 0.002 | 0.016 | 0.024 | 0.018 |
| | (0.013) | (0.013) | (0.012) | (0.013) | (0.016) | (0.016) | (0.018) | (0.018) |
| Eff. # of Control Obs. | 10,058 | 11,673 | 9,817 | 9,727 | 11,296 | 11,518 | 11,393 | 10,147 |
| Eff. # of Treatment Obs. | 15,270 | 17,688 | 15,031 | 14,856 | 17,197 | 19,505 | 19,365 | 15,452 |
| Bandwidth | 9.412 | 10.210 | 9.647 | 9.183 | 10.450 | 11.020 | 11.030 | 10.680 |
| | | | | | | | | |
| 4. No Controls | -0.009 | 0.000 | -0.000 | -0.010 | -0.005 | 0.013 | 0.020 | 0.009 |
| | (0.014) | (0.014) | (0.014) | (0.014) | (0.016) | (0.016) | (0.018) | (0.018) |
| Eff. # of Control Obs. | 9,611 | 11,673 | 9,808 | 9,727 | 11,296 | 11,518 | 11,128 | 10,147 |
| Eff. # of Treatment Obs. | 13,692 | 17,688 | 14,991 | 14,865 | 17,196 | 19,505 | 17,182 | 15,227 |
| Bandwidth | 8.851 | 10.160 | 9.465 | 9.213 | 10.380 | 11.030 | 10.990 | 10.540 |
| | | | | | | | | |
| 5. CE Bandwidth | -0.018 | -0.012 | -0.009 | -0.016 | -0.013 | -0.003 | 0.012 | 0.006 |
| | (0.015) | (0.014) | (0.014) | (0.014) | (0.018) | (0.018) | (0.019) | (0.020) |
| Eff. # of Control Obs. | 7,235 | 7,191 | 7,103 | 7,024 | 8,059 | 8,053 | 7,996 | 7,281 |
| Eff. # of Treatment Obs. | 10,890 | 10,911 | 10,675 | 10,595 | 11,819 | 11,751 | 11,680 | 10,510 |
| Bandwidth | 6.139 | 6.777 | 6.528 | 6.107 | 7.183 | 7.530 | 7.671 | 7.278 |

Continues in the next page...

Table OA5 Continued. Sharp Regression Discontinuity Estimates of the Effects of Obtaining an Intermediate Certificate on Log of Income: Robustness Checks

| | (1) Quarter 1 | (2) Quarter 2 | (3) Quarter 3 | (4) Quarter 4 | (5) Quarter 5 | (6) Quarter 6 | (7) Quarter 7 | (8) Quarter 8 |
|---|---|---|---|---|---|---|---|---|
| 6. Conventional Estimates | -0.004 | 0.002 | 0.002 | -0.008 | -0.003 | 0.012 | 0.020 | 0.012 |
| | (0.012) | (0.012) | (0.011) | (0.012) | (0.014) | (0.014) | (0.016) | (0.016) |
| Eff. # of Control Obs. | 9,611 | 9,921 | 9,808 | 9,319 | 11,296 | 11,248 | 11,393 | 10,147 |
| Eff. # of Treatment Obs. | 13,679 | 15,196 | 14,987 | 13,323 | 17,176 | 17,302 | 17,182 | 15,226 |
| Bandwidth | 8.798 | 9.714 | 9.357 | 8.753 | 10.300 | 10.790 | 11.000 | 10.410 |
| 7. No Mass Points Adjustment | -0.018 | -0.012 | -0.009 | -0.016 | -0.013 | -0.005 | 0.010 | 0.006 |
| | (0.015) | (0.014) | (0.014) | (0.014) | (0.018) | (0.019) | (0.019) | (0.020) |
| Eff. # of Control Obs. | 7,232 | 7,167 | 7,099 | 7,036 | 6,918 | 8,040 | 7,971 | 7,281 |
| Eff. # of Treatment Obs. | 10,890 | 10,781 | 10,673 | 10,595 | 10,498 | 11,745 | 11,661 | 10,510 |
| Bandwidth | 6.028 | 6.328 | 6.428 | 6.204 | 6.531 | 7.222 | 7.202 | 7.207 |
| 8. Fixed Bandwidth | -0.023 | -0.019 | -0.014 | -0.020 | -0.020 | -0.014 | -0.001 | -0.002 |
| | (0.015) | (0.015) | (0.015) | (0.016) | (0.019) | (0.020) | (0.019) | (0.023) |
| Eff. # of Control Obs. | 10,069 | 9,922 | 9,818 | 9,738 | 9,569 | 9,550 | 9,445 | 8,661 |
| Eff. # of Treatment Obs. | 15,316 | 15,197 | 15,037 | 14,936 | 14,753 | 14,648 | 14,528 | 13,107 |
| Bandwidth | 9.900 | 9.900 | 9.900 | 9.900 | 9.900 | 9.900 | 9.900 | 9.900 |
| 9. Fixed Bandwidth and | -0.022 | -0.021 | -0.026 | -0.035 | -0.033 | -0.036 | -0.033 | -0.033 |
| Always Employed | (0.018) | (0.019) | (0.017) | (0.019) | (0.019) | (0.019) | (0.018) | (0.018) |
| Eff. # of Control Obs. | 6,114 | 6,114 | 6,114 | 6,114 | 6,114 | 6,114 | 6,114 | 6,114 |
| Eff. # of Treatment Obs. | 9,244 | 9,244 | 9,244 | 9,244 | 9,244 | 9,244 | 9,244 | 9,244 |
| Bandwidth | 9.900 | 9.900 | 9.900 | 9.900 | 9.900 | 9.900 | 9.900 | 9.900 |

**Notes:** Each cell displays the estimate for the main coefficient of interest in Equation (1), one to eight quarters after certification. The outcome is the log of income. The running variable is the exam score, and the discontinuity threshold is 60. Row 1 displays the baseline results from Table 4. Rows 2 and 3 show the results when we add year and location fixed effects (FE), respectively. Row 4 excludes controls. Row 5 uses the bandwidth that minimizes the coverage error (CE) of the local polynomial regression discontinuity estimator. Row 6 reports non-bias-corrected estimates. Row 7 does not adjust for the presence of mass points. Row 8 uses a fixed bandwidth. Row 9 also uses a fixed bandwidth and restricts attention to individuals that are employed in all eight quarters. All specifications include controls described in Section 4, use a triangular kernel, and choose the bandwidth to minimize the mean squared error (MSE) of the local polynomial RD estimator, except for rows 5, 8, and 9. In all specifications, except for row 7, we report robust bias-corrected standard errors below the point estimates in parentheses. Standard errors are clustered at the technical-norm level. Bandwidths are displayed below the effective number of observations. The effective number of observations changes across quarters given variation in the number of individuals with positive earnings and the optimal bandwidth.

Table OA6: Sharp Regression Discontinuity Estimates of the Effects of Obtaining an Advanced Certificate on Log of Income: Robustness Checks

| | (1) Quarter 1 | (2) Quarter 2 | (3) Quarter 3 | (4) Quarter 4 | (5) Quarter 5 | (6) Quarter 6 | (7) Quarter 7 | (8) Quarter 8 |
|---|---|---|---|---|---|---|---|---|
| 1. Baseline | 0.087 | 0.087 | 0.082 | 0.127 | 0.104 | 0.094 | 0.099 | 0.099 |
| | (0.028) | (0.031) | (0.034) | (0.029) | (0.029) | (0.027) | (0.032) | (0.032) |
| Eff. # of Control Obs. | 8,322 | 8,009 | 6,843 | 7,986 | 7,901 | 10,235 | 7,619 | 6,929 |
| Eff. # of Treatment Obs. | 18,098 | 17,852 | 14,410 | 17,669 | 17,400 | 23,044 | 16,776 | 14,754 |
| Bandwidth | 3.955 | 3.548 | 2.981 | 3.684 | 3.625 | 4.113 | 3.082 | 3.013 |
| | | | | | | | | |
| 2. Year FE | 0.094 | 0.092 | 0.085 | 0.127 | 0.110 | 0.102 | 0.101 | 0.099 |
| | (0.029) | (0.032) | (0.034) | (0.029) | (0.030) | (0.031) | (0.032) | (0.032) |
| Eff. # of Control Obs. | 8,313 | 7,998 | 6,843 | 7,986 | 7,884 | 7,937 | 7,619 | 6,929 |
| Eff. # of Treatment Obs. | 18,087 | 17,852 | 14,410 | 17,669 | 17,400 | 17,330 | 16,776 | 14,754 |
| Bandwidth | 3.845 | 3.542 | 2.981 | 3.684 | 3.589 | 3.798 | 3.076 | 3.013 |
| | | | | | | | | |
| 3. Location FE | 0.095 | 0.099 | 0.095 | 0.139 | 0.120 | 0.108 | 0.109 | 0.104 |
| | (0.029) | (0.030) | (0.033) | (0.029) | (0.032) | (0.028) | (0.031) | (0.030) |
| Eff. # of Control Obs. | 8,122 | 7,997 | 6,843 | 7,881 | 7,782 | 7,820 | 7,619 | 6,034 |
| Eff. # of Treatment Obs. | 18,020 | 17,849 | 14,410 | 17,609 | 16,980 | 17,283 | 16,776 | 12,363 |
| Bandwidth | 3.580 | 3.512 | 2.969 | 3.334 | 3.007 | 3.734 | 3.072 | 2.968 |
| | | | | | | | | |
| 4. No Controls | 0.093 | 0.095 | 0.089 | 0.142 | 0.111 | 0.110 | 0.102 | 0.092 |
| | (0.033) | (0.035) | (0.031) | (0.039) | (0.034) | (0.039) | (0.030) | (0.033) |
| Eff. # of Control Obs. | 8,098 | 7,983 | 8,027 | 6,803 | 7,857 | 6,669 | 7,864 | 7,021 |
| Eff. # of Treatment Obs. | 17,987 | 17,821 | 17,701 | 14,293 | 17,367 | 14,068 | 17,274 | 15,174 |
| Bandwidth | 3.478 | 3.406 | 3.736 | 2.793 | 3.474 | 2.959 | 3.811 | 3.595 |
| | | | | | | | | |
| 5. CE Bandwidth | 0.082 | 0.084 | 0.066 | 0.127 | 0.101 | 0.09 | 0.086 | 0.090 |
| | (0.030) | (0.034) | (0.035) | (0.029) | (0.030) | (0.030) | (0.033) | (0.034) |
| Eff. # of Control Obs. | 6,994 | 6,768 | 6,709 | 6,792 | 6,743 | 6,656 | 6,478 | 5,910 |
| Eff. # of Treatment Obs. | 14,625 | 14,389 | 14,143 | 14,238 | 14,054 | 14,004 | 13,750 | 12,101 |
| Bandwidth | 2.744 | 2.462 | 2.068 | 2.556 | 2.515 | 2.854 | 2.138 | 2.094 |

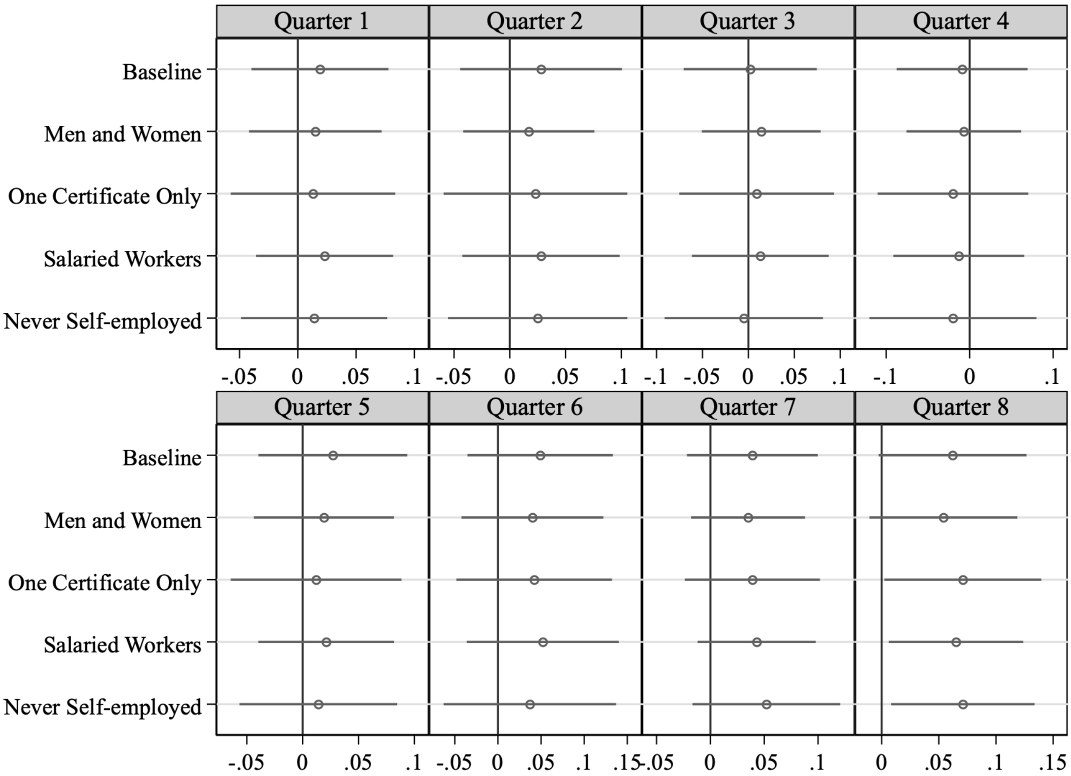
Continues in the next page...

Table OA6 Continued. Sharp Regression Discontinuity Estimates of the Effects of Obtaining an Advanced Certificate on Log of Income: Robustness Checks

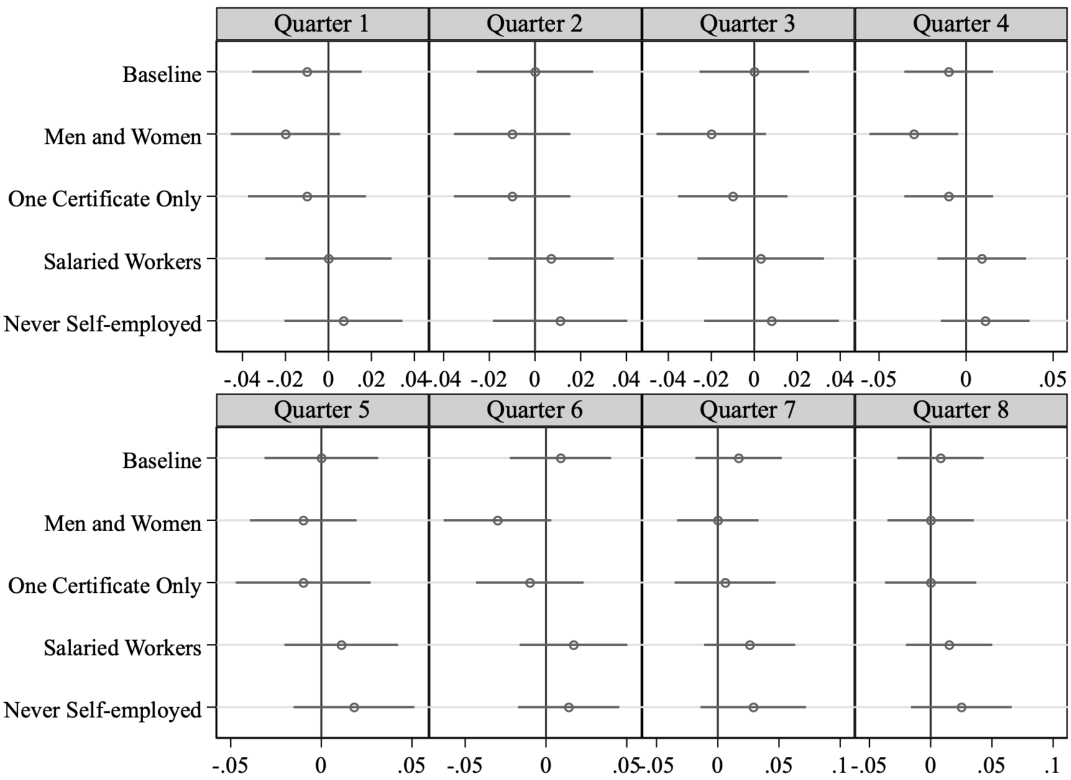
| | (1) Quarter 1 | (2) Quarter 2 | (3) Quarter 3 | (4) Quarter 4 | (5) Quarter 5 | (6) Quarter 6 | (7) Quarter 7 | (8) Quarter 8 |
|---|---|---|---|---|---|---|---|---|
| 6. Conventional Estimates | 0.074 | 0.072 | 0.066 | 0.106 | 0.084 | 0.074 | 0.079 | 0.080 |
| | (0.022) | (0.023) | (0.025) | (0.023) | (0.023) | (0.021) | (0.023) | (0.024) |
| Eff. # of Control Obs. | 8,322 | 8,009 | 6,843 | 7,986 | 7,901 | 10,235 | 7,619 | 6,929 |
| Eff. # of Treatment Obs. | 18,098 | 17,852 | 14,410 | 17,669 | 17,400 | 23,044 | 16,776 | 14,754 |
| Bandwidth | 3.955 | 3.548 | 2.981 | 3.684 | 3.625 | 4.113 | 3.082 | 3.013 |
| | | | | | | | | |
| 7. No Mass Points Adjustment | 0.074 | 0.085 | 0.058 | 0.131 | 0.098 | 0.100 | 0.075 | 0.090 |
| | (0.037) | (0.034) | (0.039) | (0.031) | (0.035) | (0.034) | (0.036) | (0.041) |
| Eff. # of Control Obs. | 6,882 | 6,872 | 6,709 | 6,791 | 6,643 | 6,654 | 6,475 | 5,910 |
| Eff. # of Treatment Obs. | 14,495 | 14,479 | 14,143 | 14,238 | 13,927 | 13,989 | 13,750 | 12,101 |
| Bandwidth | 2.307 | 2.615 | 2.058 | 2.530 | 2.284 | 2.705 | 2.116 | 2.049 |
| | | | | | | | | |
| 8. Fixed Bandwidth | 0.053 | 0.061 | 0.063 | 0.117 | 0.092 | 0.083 | 0.076 | 0.086 |
| | (0.037) | (0.038) | (0.034) | (0.034) | (0.034) | (0.035) | (0.034) | (0.035) |
| Eff. # of Control Obs. | 8,073 | 7,960 | 7,907 | 7,877 | 7,832 | 7,712 | 7,635 | 6,975 |
| Eff. # of Treatment Obs. | 17,925 | 17,760 | 17,603 | 17,577 | 17,303 | 17,181 | 17,105 | 15,081 |
| Bandwidth | 3.300 | 3.300 | 3.300 | 3.300 | 3.300 | 3.300 | 3.300 | 3.300 |
| | | | | | | | | |
| 9. Fixed Bandwidth and | 0.060 | 0.075 | 0.077 | 0.119 | 0.106 | 0.105 | 0.097 | 0.095 |
| Always Employed | (0.040) | (0.037) | (0.034) | (0.036) | (0.035) | (0.035) | (0.035) | (0.036) |
| Eff. # of Control Obs. | 5,100 | 5,100 | 5,100 | 5,100 | 5,100 | 5,100 | 5,100 | 5,100 |
| Eff. # of Treatment Obs. | 10,825 | 10,825 | 10,825 | 10,825 | 10,825 | 10,825 | 10,825 | 10,825 |
| Bandwidth | 3.300 | 3.300 | 3.300 | 3.300 | 3.300 | 3.300 | 3.300 | 3.300 |

**Notes:** Each cell displays the estimate for the main coefficient of interest in Equation (1), one to eight quarters after certification. The outcome is the log of income. The running variable is the exam score, and the discontinuity threshold is 90. Row 1 displays the baseline results from Table 4. Rows 2 and 3 show the results when we add year and location fixed effects (FE), respectively. Row 4 excludes controls. Row 5 uses the bandwidth that minimizes the coverage error (CE) of the local polynomial regression discontinuity estimator. Row 6 reports non-bias-corrected estimates. Row 7 does not adjust for the presence of mass points. Row 8 uses a fixed bandwidth. Row 9 also uses a fixed bandwidth and restricts attention to individuals that are employed in all eight quarters. All specifications include controls described in Section 4, use a triangular kernel, and choose the bandwidth to minimize the mean squared error (MSE) of the local polynomial RD estimator, except for rows 5, 8, and 9. In all specifications, except for row 7, we report robust bias-corrected standard errors below the point estimates in parentheses. Standard errors are clustered at the technical-norm level. Bandwidths are displayed below the effective number of observations. The effective number of observations changes across quarters given variation in the number of individuals with positive earnings and the optimal bandwidth.

Figure OA5: Sharp Regression Discontinuity Estimates of the Effects of Obtaining a Certificate on Log of Income: Alternative Samples

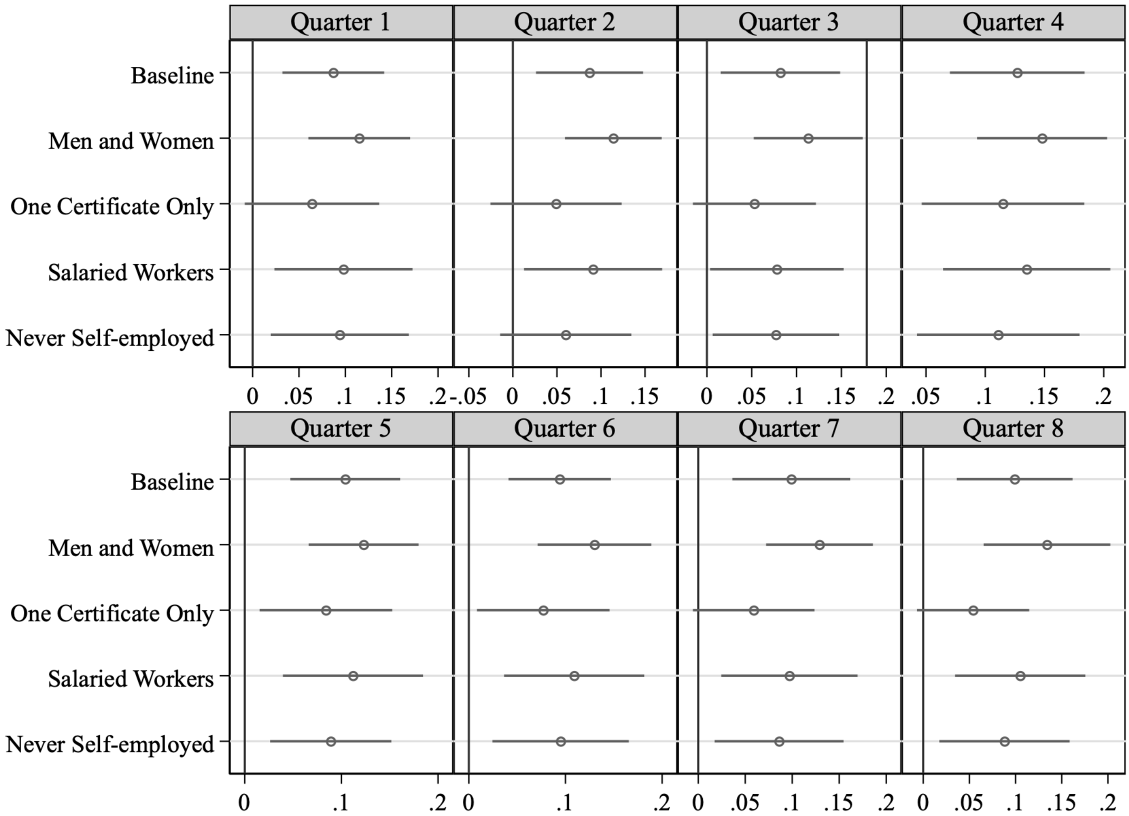



(a) Basic




(b) Intermediate

Figure OA5 Continued. Sharp Regression Discontinuity Estimates of the Effects of Obtaining a Certificate on Log of Income: Alternative Samples




(c) Advanced

**Notes**: Each plot displays the estimate for the main coefficient of interest in Equation (1) and its 95% confidence interval, one to eight quarters after certification. The outcome is the log of income. The running variable is the exam score, and the discontinuity threshold is 30 for the basic certificate (panel a), 60 for the intermediate certificate (panel b), and 90 for the advanced certificate (panel c). In each plot, row 1 displays the baseline results from Table 4. Row 2 uses the full sample of men and women. Row 3 uses a sample of men who applied for only one certificate between 2017 and 2019. Row 4 uses a sample of salaried workers, while the fifth row further excludes individuals who are ever self-employed within two years of certification. All specifications include controls described in Section 4, use a triangular kernel, and choose the bandwidth to minimize the mean squared error (MSE) of the local polynomial RD estimator, except for rows 5, 8, and 9. Standard errors used to compute 95% confidence intervals are clustered at the technical-norm level. Detailed results for all three certificates are displayed in Tables OA7, OA8, and OA9.

Table OA7: Sharp Regression Discontinuity Estimates of the Effects of Obtaining a Basic Certificate on Log of Income: Alternative Samples

| | (1) Quarter 1 | (2) Quarter 2 | (3) Quarter 3 | (4) Quarter 4 | (5) Quarter 5 | (6) Quarter 6 | (7) Quarter 7 | (8) Quarter 8 |
|---|---|---|---|---|---|---|---|---|
| 1. Men and Women | 0.015 | 0.017 | 0.014 | -0.007 | 0.019 | 0.040 | 0.035 | 0.054 |
| | (0.029) | (0.030) | (0.033) | (0.035) | (0.032) | (0.042) | (0.027) | (0.033) |
| # of Observations | 235,346 | 231,969 | 229,967 | 228,652 | 226,496 | 224,094 | 222,619 | 201,123 |
| Eff. # of Control Obs. | 773 | 832 | 907 | 799 | 742 | 861 | 880 | 825 |
| Eff. # of Treatment Obs. | 2,666 | 2,988 | 3,627 | 2,939 | 2,517 | 3,288 | 3,548 | 2,951 |
| Bandwidth | 6.975 | 7.459 | 9.030 | 7.196 | 6.842 | 8.807 | 9.552 | 8.203 |
| 2. One Certificate Only | 0.013 | 0.023 | 0.009 | -0.020 | 0.012 | 0.042 | 0.039 | 0.071 |
| | (0.036) | (0.042) | (0.043) | (0.046) | (0.039) | (0.046) | (0.032) | (0.035) |
| # of Observations | 125,946 | 124,460 | 123,522 | 122,565 | 121,398 | 120,332 | 119,670 | 107,464 |
| Eff. # of Control Obs. | 500 | 537 | 527 | 509 | 482 | 608 | 483 | 510 |
| Eff. # of Treatment Obs. | 1,631 | 1,830 | 1,804 | 1,790 | 1,551 | 2,780 | 1,548 | 1,589 |
| Bandwidth | 6.810 | 7.333 | 7.649 | 7.161 | 6.651 | 10.26- | 6.505 | 7.900 |
| 3. Salaried Worker | 0.023 | 0.028 | 0.013 | -0.013 | 0.021 | 0.052 | 0.043 | 0.065 |
| | (0.030) | (0.036) | (0.038) | (0.040) | (0.031) | (0.045) | (0.028) | (0.030) |
| # of Observations | 144,403 | 142,532 | 141,069 | 139,894 | 138,472 | 137,331 | 136,243 | 122,849 |
| Eff. # of Control Obs. | 661 | 615 | 633 | 653 | 600 | 591 | 579 | 535 |
| Eff. # of Treatment Obs. | 2,505 | 2,229 | 2,431 | 2,978 | 2,190 | 2,177 | 2,162 | 1,702 |
| Bandwidth | 9.410 | 8.173 | 9.158 | 10.46 | 8.613 | 8.886 | 8.361 | 7.977 |
| 4. Never Self-Employed | 0.014 | 0.025 | -0.005 | -0.020 | 0.014 | 0.037 | 0.052 | 0.071 |
| | (0.032) | (0.041) | (0.044) | (0.051) | (0.036) | (0.051) | (0.035) | (0.032) |
| # of Observations | 125,383 | 123,881 | 123,040 | 122,412 | 121,235 | 120,425 | 119,612 | 107,859 |
| Eff. # of Control Obs. | 569 | 553 | 571 | 548 | 559 | 555 | 501 | 544 |
| Eff. # of Treatment Obs. | 1,997 | 1,944 | 2,117 | 2,106 | 2,092 | 2,088 | 1,662 | 1,821 |
| Bandwidth | 8.569 | 8.678 | 9.969 | 9.168 | 9.375 | 9.966 | 7.701 | 9.241 |

**Notes:** Each cell displays the estimate for the main coefficient of interest in Equation (1), one to eight quarters after certification. The outcome is the log of income. The running variable is the exam score, and the discontinuity threshold is 30. Row 1 uses the full sample of men and women. Row 2 uses a sample of men who apply for only one certificate between 2017 and 2019. Row 3 uses a sample of salaried workers, while row 4 further excludes individuals who are ever self-employed within two years of certification. All specifications include controls described in Section 4, use a triangular kernel, and choose the bandwidth to minimize the mean squared error (MSE) of the local polynomial RD estimator. We report robust bias-corrected standard errors below the point estimates in parentheses. Standard errors are clustered at the technical norm level. Bandwidths are displayed below the effective number of observations. The total number of observations changes across quarters given variation in the number of individuals with positive earnings; it further drops in quarter 8 due to missing information in the last quarter for individuals that certified in the last quarter of 2019. The effective number of observations changes across quarters given variation in the optimal bandwidth.

Table OA8: Sharp Regression Discontinuity Estimates of the Effects of Obtaining an Intermediate Certificate on Log of Income: Alternative Samples

| | (1) Quarter 1 | (2) Quarter 2 | (3) Quarter 3 | (4) Quarter 4 | (5) Quarter 5 | (6) Quarter 6 | (7) Quarter 7 | (8) Quarter 8 |
|---|---|---|---|---|---|---|---|---|
| 1. Men and Women | -0.021 | -0.016 | -0.028 | -0.033 | -0.014 | -0.030 | -0.009 | -0.008 |
| | (0.013) | (0.013) | (0.013) | (0.013) | (0.015) | (0.017) | (0.017) | (0.018) |
| # of Observations | 235,346 | 231,969 | 229,967 | 228,652 | 226,496 | 224,094 | 222,619 | 201,123 |
| Eff. # of Control Obs. | 12,060 | 11,949 | 9,896 | 9,814 | 12,655 | 9,636 | 11,354 | 10,343 |
| Eff. # of Treatment Obs. | 17,468 | 17,304 | 15,302 | 15,196 | 18,369 | 15,051 | 16,555 | 14,911 |
| Bandwidth | 7.209 | 7.768 | 6.584 | 6.326 | 8.095 | 6.828 | 7.739 | 7.667 |
| 2. One Certificate Only | -0.013 | -0.010 | -0.012 | -0.018 | -0.016 | -0.011 | 0.006 | -0.002 |
| | (0.014) | (0.013) | (0.013) | (0.013) | (0.019) | (0.017) | (0.021) | (0.019) |
| # of Observations | 125,946 | 124,460 | 123,522 | 122,565 | 121,398 | 120,332 | 119,670 | 107,464 |
| Eff. # of Control Obs. | 11,393 | 10,427 | 9,412 | 9,540 | 9,189 | 9,153 | 9,289 | 8,268 |
| Eff. # of Treatment Obs. | 18,774 | 17,868 | 13,990 | 15,897 | 13,897 | 13,631 | 15,501 | 12,310 |
| Bandwidth | 13.730 | 12.900 | 10.170 | 11.400 | 10.670 | 10.320 | 11.280 | 10.680 |
| 3. Salaried Worker | 0.000 | 0.007 | 0.003 | 0.009 | 0.011 | 0.017 | 0.026 | 0.015 |
| | (0.015) | (0.014) | (0.015) | (0.013) | (0.016) | (0.017) | (0.019) | (0.018) |
| # of Observations | 144,403 | 142,532 | 141,069 | 139,894 | 138,472 | 137,331 | 136,243 | 122,849 |
| Eff. # of Control Obs. | 8,990 | 10,238 | 8,709 | 10,040 | 10,800 | 10,044 | 10,604 | 9,072 |
| Eff. # of Treatment Obs. | 13,060 | 14,730 | 12,787 | 14,361 | 17,906 | 16,330 | 17,678 | 14,624 |
| Bandwidth | 9.699 | 10.530 | 9.673 | 10.370 | 12.030 | 11.300 | 12.130 | 11.800 |
| 4. Never Self-Employed | 0.007 | 0.011 | 0.008 | 0.011 | 0.018 | 0.014 | 0.029 | 0.025 |
| | (0.014) | (0.015) | (0.016) | (0.013) | (0.017) | (0.016) | (0.022) | (0.021) |
| # of Observations | 125,383 | 123,881 | 123,040 | 122,412 | 121,235 | 120,425 | 119,612 | 107,859 |
| Eff. # of Control Obs. | 8,894 | 9,617 | 8,650 | 8,865 | 10,118 | 8,664 | 9,212 | 8,424 |
| Eff. # of Treatment Obs. | 12,633 | 15,731 | 12,414 | 14,177 | 16,260 | 12,164 | 15,317 | 13,638 |
| Bandwidth | 10.730 | 12.160 | 10.930 | 11.430 | 13.020 | 11.000 | 12.800 | 12.320 |

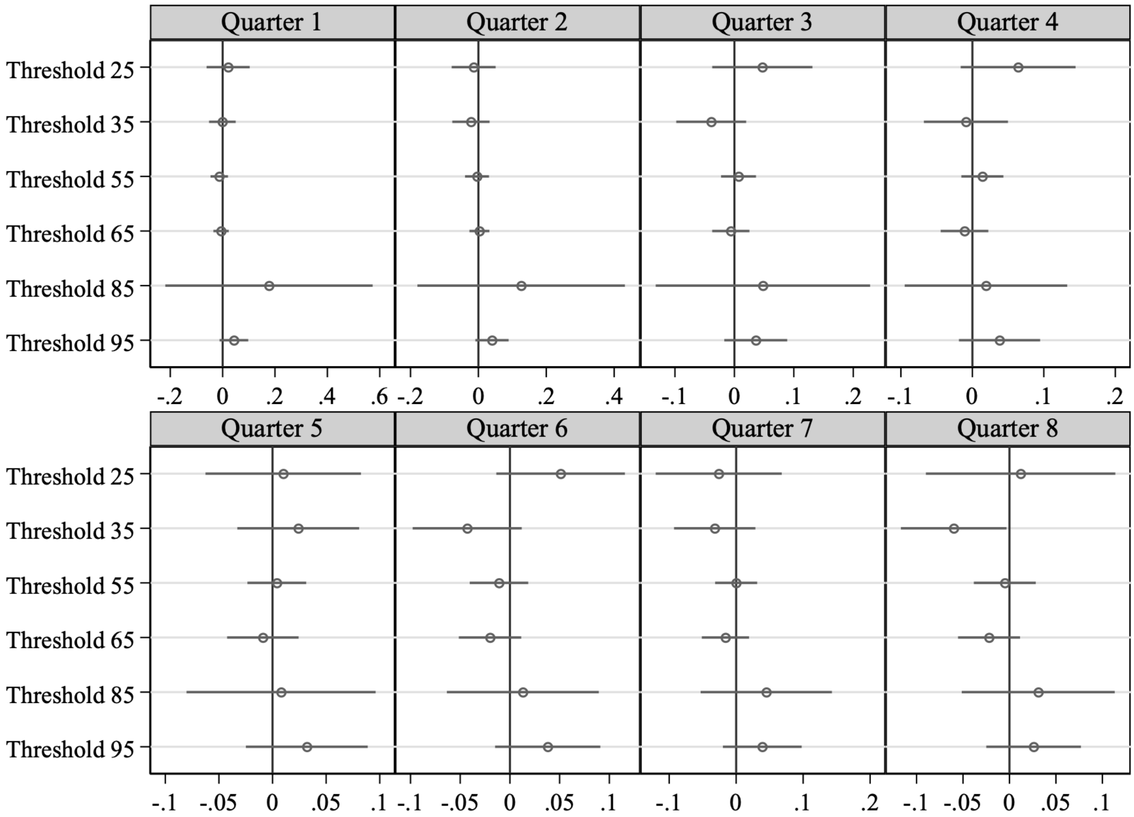
**Notes:** Each cell displays the estimate for the main coefficient of interest in Equation (1), one to eight quarters after certification. The outcome is the log of income. The running variable is the exam score, and the discontinuity threshold is 60. Row 1 uses the full sample of men and women. Row 2 uses a sample of men who apply for only one certificate between 2017 and 2019. Row 3 uses a sample of salaried workers, while row 4 further excludes individuals who are ever self-employed within two years of certification. All specifications include controls described in Section 4, use a triangular kernel, and choose the bandwidth to minimize the mean squared error (MSE) of the local polynomial RD estimator. We report robust bias-corrected standard errors below the point estimates in parentheses. Standard errors are clustered at the technical norm level. Bandwidths are displayed below the effective number of observations. The total number of observations changes across quarters given variation in the number of individuals with positive earnings; it further drops in quarter 8 due to missing information in the last quarter for individuals that certified in the last quarter of 2019. The effective number of observations changes across quarters given variation in the optimal bandwidth.

Table OA9: Sharp Regression Discontinuity Estimates of the Effects of Obtaining an Advanced Certificate on Log of Income: Alternative Samples

| | **(1) Quarter 1** | **(2) Quarter 2** | **(3) Quarter 3** | **(4) Quarter 4** | **(5) Quarter 5** | **(6) Quarter 6** | **(7) Quarter 7** | **(8) Quarter 8** |
|---|---|---|---|---|---|---|---|---|
| 1. Men and Women | 0.115 | 0.114 | 0.113 | 0.148 | 0.123 | 0.130 | 0.129 | 0.134 |
| | (0.028) | (0.028) | (0.031) | (0.028) | (0.029) | (0.030) | (0.029) | (0.035) |
| # of Observations | 235,346 | 231,969 | 229,967 | 228,652 | 226,496 | 224,094 | 222,619 | 201,123 |
| Eff. # of Control Obs. | 12,878 | 12,601 | 10,696 | 12,183 | 12,204 | 11,989 | 11,877 | 9,453 |
| Eff. # of Treatment Obs. | 24,914 | 24,688 | 20,115 | 24,147 | 24,011 | 23,787 | 23,630 | 17,351 |
| Bandwidth | 3.976 | 3.841 | 2.718 | 3.151 | 3.568 | 3.452 | 3.380 | 2.711 |
| 2. One Certificate Only | 0.064 | 0.049 | 0.053 | 0.115 | 0.084 | 0.077 | 0.059 | 0.054 |
| | (0.037) | (0.038) | (0.035) | (0.035) | (0.035) | (0.035) | (0.033) | (0.031) |
| # of Observations | 125,946 | 124,460 | 123,522 | 122,565 | 121,398 | 120,332 | 119,670 | 107,464 |
| Eff. # of Control Obs. | 6,330 | 6,240 | 6,201 | 6,172 | 6,124 | 5,279 | 5,997 | 5,440 |
| Eff. # of Treatment Obs. | 13,671 | 13,298 | 13,191 | 13,206 | 12,980 | 10,669 | 12,829 | 11,237 |
| Bandwidth | 3.112 | 3.024 | 3.011 | 3.055 | 3.006 | 2.941 | 3.090 | 3.077 |
| 3. Salaried Worker | 0.098 | 0.091 | 0.078 | 0.135 | 0.112 | 0.109 | 0.097 | 0.105 |
| | (0.038) | (0.040) | (0.038) | (0.036) | (0.037) | (0.037) | (0.037) | (0.036) |
| # of Observations | 144,403 | 142,532 | 141,069 | 139,894 | 138,472 | 137,331 | 136,243 | 122,849 |
| Eff. # of Control Obs. | 7,250 | 7,173 | 7,071 | 7,077 | 7,030 | 6,917 | 5,909 | 6,206 |
| Eff. # of Treatment Obs. | 15,470 | 15,352 | 15,138 | 15,076 | 14,852 | 15,080 | 12,060 | 12,892 |
| Bandwidth | 3.019 | 3.047 | 3.016 | 3.065 | 3.057 | 3.214 | 2.851 | 3.025 |
| 4. Never Self-Employed | 0.094 | 0.060 | 0.077 | 0.111 | 0.089 | 0.095 | 0.086 | 0.088 |
| | (0.038) | (0.038) | (0.036) | (0.035) | (0.032) | (0.036) | (0.035) | (0.036) |
| # of Observations | 125,383 | 123,881 | 123,040 | 122,412 | 121,235 | 120,425 | 119,612 | 107,859 |
| Eff. # of Control Obs. | 6,337 | 6,241 | 6,216 | 6,219 | 6,257 | 6,087 | 5,191 | 5,463 |
| Eff. # of Treatment Obs. | 13,861 | 13,471 | 13,354 | 13,637 | 13,512 | 13,379 | 10,628 | 11,439 |
| Bandwidth | 3.182 | 3.051 | 3.095 | 3.155 | 3.740 | 3.237 | 2.811 | 3.019 |

**Notes:** Each cell displays the estimate for the main coefficient of interest in Equation (1), one to eight quarters after certification. The outcome is the log of income. The running variable is the exam score, and the discontinuity threshold is 90. Row 1 uses the full sample of men and women. Row 2 uses a sample of men who apply for only one certificate between 2017 and 2019. Row 3 uses a sample of salaried workers, while row 4 further excludes individuals who are ever self-employed within two years of certification. All specifications include controls described in Section 4, use a triangular kernel, and choose the bandwidth to minimize the mean squared error (MSE) of the local polynomial RD estimator. We report robust bias-corrected standard errors below the point estimates in parentheses. Standard errors are clustered at the technical norm level. Bandwidths are displayed below the effective number of observations. The total number of observations changes across quarters given variation in the number of individuals with positive earnings; it further drops in quarter 8 due to missing information in the last quarter for individuals that certified in the last quarter of 2019. The effective number of observations changes across quarters given variation in the optimal bandwidth.

Figure OA7: Sharp Regression Discontinuity Estimates of the Effects on Log of Income - Placebo Thresholds




**Notes:** Each plot displays the estimate for the main coefficient of interest in Equation (1) its 95% confidence interval, one to eight quarters after certification. The outcome is the log of income. The running variable is the exam score, and the (placebo) thresholds of interest are 25, 35, 55, 65, 85, and 95. All specifications include controls described in Section 4, use a triangular kernel, and choose the bandwidth to minimize the mean squared error (MSE) of the local polynomial RD estimator. Standard errors used to compute 95% confidence intervals are clustered at the technical norm level. Detailed results for all placebo thresholds are displayed in Table OA10.

Table OA10: Sharp Regression Discontinuity Estimates of the Effects on Log of Income - Placebo Thresholds

| | **(1) Quarter 1** | **(2) Quarter 2** | **(3) Quarter 3** | **(4) Quarter 4** | **(5) Quarter 5** | **(6) Quarter 6** | **(7) Quarter 7** | **(8) Quarter 8** |
|---|---|---|---|---|---|---|---|---|
| Threshold 25 | 0.021 | -0.014 | 0.047 | 0.064 | 0.010 | 0.051 | -0.026 | 0.012 |
| | (0.042) | (0.033) | (0.043) | (0.041) | (0.037) | (0.033) | (0.048) | (0.052) |
| Eff. # of Control Obs. | 260 | 358 | 256 | 272 | 246 | 311 | 246 | 236 |
| Eff. # of Treatment Obs. | 725 | 1,265 | 703 | 868 | 692 | 1,005 | 687 | 641 |
| Bandwidth | 5.331 | 8.912 | 5.093 | 6.277 | 5.437 | 7.770 | 5.266 | 5.233 |
| Threshold 35 | -0.001 | -0.022 | -0.039 | -0.009 | 0.024 | -0.043 | -0.032 | -0.060 |
| | (0.026) | (0.028) | (0.030) | (0.030) | (0.029) | (0.028) | (0.031) | (0.029) |
| Eff. # of Control Obs. | 1,446 | 1,473 | 1,263 | 1,277 | 1,273 | 1,252 | 1,255 | 1,171 |
| Eff. # of Treatment Obs. | 4,716 | 5,661 | 3,627 | 4,086 | 4,093 | 4,019 | 4,023 | 3,613 |
| Bandwidth | 9.549 | 10.060 | 7.265 | 8.724 | 8.097 | 8.285 | 8.347 | 8.911 |
| Threshold 55 | -0.013 | -0.004 | 0.007 | 0.014 | 0.004 | -0.011 | -0.000 | -0.005 |
| | (0.017) | (0.018) | (0.015) | (0.015) | (0.014) | (0.015) | (0.016) | (0.017) |
| Eff. # of Control Obs. | 9,707 | 9,556 | 10,190 | 8,901 | 10,027 | 8,695 | 9,521 | 7,824 |
| Eff. # of Treatment Obs. | 17,307 | 17,139 | 19,504 | 15,793 | 19,038 | 15,317 | 17,608 | 13,813 |
| Bandwidth | 11.950 | 11.740 | 13.490 | 10.990 | 13.380 | 10.230 | 12.420 | 10.250 |
| Threshold 65 | -0.006 | 0.003 | -0.006 | -0.011 | -0.009 | -0.020 | -0.016 | -0.022 |
| | (0.015) | (0.015) | (0.016) | (0.017) | (0.017) | (0.016) | (0.018) | (0.017) |
| Eff. # of Control Obs. | 18,541 | 15,985 | 18,131 | 16,243 | 16,005 | 15,902 | 13,198 | 13,046 |
| Eff. # of Treatment Obs. | 33,475 | 27,920 | 32,882 | 29,086 | 28,727 | 28,301 | 22,386 | 22,321 |
| Bandwidth | 15.240 | 13.140 | 15.600 | 14.802 | 14.910 | 14.170 | 11.170 | 12.150 |

Continues in the Next Page...

Table OA10 Continued. Sharp Regression Discontinuity Estimates of the Effects on Log of Income - Placebo Thresholds

| | **(1) Quarter 1** | **(2) Quarter 2** | **(3) Quarter 3** | **(4) Quarter 4** | **(5) Quarter 5** | **(6) Quarter 6** | **(7) Quarter 7** | **(8) Quarter 8** |
|---|---|---|---|---|---|---|---|---|
| Threshold 85 | 0.177 | 0.126 | 0.048 | 0.019 | 0.008 | 0.013 | 0.045 | 0.031 |
| | (0.202) | (0.156) | (0.092) | (0.058) | (0.045) | (0.039) | (0.050) | (0.042) |
| Eff. # of Control Obs. | 4,945 | 5,086 | 5,388 | 7,651 | 7,689 | 7,554 | 7,552 | 6,723 |
| Eff. # of Treatment Obs. | 7,217 | 7,131 | 7,187 | 9,954 | 10,154 | 9,760 | 9,667 | 8,717 |
| Bandwidth | 2.126 | 2.376 | 2.651 | 3.050 | 3.280 | 3.065 | 3.121 | 3.163 |
| Threshold 95 | 0.043 | 0.040 | 0.036 | 0.038 | 0.032 | 0.038 | 0.039 | 0.026 |
| | (0.028) | (0.025) | (0.027) | (0.029) | (0.029) | (0.027) | (0.030) | (0.026) |
| Eff. # of Control Obs. | 14,223 | 14,262 | 14,170 | 14,125 | 13,934 | 13,603 | 13,543 | 12,158 |
| Eff. # of Treatment Obs. | 13,838 | 13,735 | 13,595 | 13,541 | 13,417 | 13,268 | 13,192 | 11,454 |
| Bandwidth | 3.563 | 3.656 | 3.616 | 3.614 | 3.583 | 3.468 | 3.472 | 3.581 |

**Notes:** Each cell displays the estimate for the main coefficient of interest in Equation (1), one to eight quarters after certification. The outcome is the log of income. The running variable is the exam score and the (placebo) thresholds of interest are 25, 35, 55, 65, 85, and 95. All regressions include controls described in Section 4, use a triangular kernel, and choose the bandwidth to minimize the mean squared error (MSE) of the local polynomial RD estimator. We report robust bias-corrected standard errors below the point estimates in parentheses. Standard errors are clustered at the technical norm level. Optimal bandwidths are displayed below the standard errors. The effective number of observations changes across quarters given variation in the number of individuals with positive earnings and the optimal bandwidth.

# Online Appendix C. Mechanisms: Additional Results

Table OA11: Sharp Regression Discontinuity Estimates of the Short- and Medium-Term Effects of Obtaining an Advanced Certificate: Additional Outcomes

| | (1) Quarter 1 | (2) Quarter 2 | (3) Quarter 3 | (4) Quarter 4 | (5) Quarter 5 | (6) Quarter 6 | (7) Quarter 7 | (8) Quarter 8 |
|---|---|---|---|---|---|---|---|---|
| 1. Overall Employment | 0.008 | -0.007 | -0.015 | -0.008 | -0.010 | 0.011 | -0.011 | 0.009 |
| | (0.024) | (0.015) | (0.022) | (0.026) | (0.023) | (0.018) | (0.017) | (0.020) |
| Observations | 181,395 | 181,395 | 181,395 | 181,395 | 181,395 | 181,395 | 181,395 | 164,296 |
| Eff. # of Control Obs. | 7,585 | 7,610 | 7,591 | 7,475 | 7,463 | 7,595 | 8,872 | 6,976 |
| Eff. # of Treat. Obs. | 16,237 | 16,375 | 16,287 | 16,078 | 16,061 | 16,288 | 20,057 | 14,358 |
| Bandwidth | 2.588 | 2.908 | 2.638 | 2.238 | 2.049 | 2.783 | 3.559 | 2.991 |
| Mean | 0.921 | 0.906 | 0.899 | 0.896 | 0.889 | 0.876 | 0.866 | 0.866 |
| 2. Salaried Work | -0.046 | -0.039 | -0.001 | -0.028 | -0.031 | -0.000 | -0.011 | 0.020 |
| | (0.082) | (0.047) | (0.034) | (0.043) | (0.049) | (0.028) | (0.031) | (0.030) |
| Observations | 181,395 | 181,395 | 181,395 | 181,395 | 181,395 | 181,395 | 181,395 | 164,296 |
| Eff. # of Control Obs. | 3,979 | 4,344 | 7,474 | 3,979 | 3,965 | 7,479 | 7,478 | 8,073 |
| Eff. # of Treat. Obs. | 12,090 | 12,177 | 16,065 | 12,090 | 12,007 | 16,181 | 16,181 | 17,499 |
| Bandwidth | 1.671 | 1.935 | 2.202 | 1.668 | 1.584 | 2.440 | 2.409 | 3.146 |
| Mean | 0.848 | 0.831 | 0.810 | 0.824 | 0.816 | 0.787 | 0.777 | 0.773 |
| 3. Self-Employment | -0.008 | -0.016 | -0.019 | -0.022 | -0.025 | -0.014 | -0.003 | -0.012 |
| | (0.040) | (0.040) | (0.033) | (0.041) | (0.028) | (0.032) | (0.039) | (0.037) |
| Observations | 181,395 | 181,395 | 181,395 | 181,395 | 181,395 | 181,395 | 181,395 | 164,296 |
| Eff. # of Control Obs. | 3,813 | 3,935 | 3,969 | 3,826 | 4,340 | 4,340 | 3,813 | 3,633 |
| Eff. # of Treat. Obs. | 11,748 | 11,985 | 12,038 | 11,810 | 12,173 | 12,172 | 11,748 | 10,695 |
| Bandwidth | 1.369 | 1.439 | 1.623 | 1.410 | 1.858 | 1.805 | 1.390 | 1.721 |
| Mean | 0.044 | 0.045 | 0.049 | 0.048 | 0.057 | 0.055 | 0.050 | 0.050 |

**Notes:** Each cell displays the estimate for the main coefficient of interest in Equation (1) for the advanced certificate, one to eight quarters after certification. The three outcomes analyzed are overall employment, salaried work, and self-employment. The running variable is the exam score and the discontinuity threshold is 90. All regressions include the controls described in Section 4, use a triangular kernel, and use the bandwidth that minimizes the mean squared error of the local polynomial regression discontinuity estimator. We report robust bias-corrected standard errors below the point estimates in parentheses. Standard errors are clustered at the technical-norm level. Optimal bandwidths are displayed below the standard errors. The total number of observations drops in quarter 8 due to missing information in the last quarter for individuals that certified in the last quarter of 2019. The effective number of observations changes across quarters given variation in the optimal bandwidth. The sample mean for the control group is displayed below the optimal bandwidth.

Table OA12: Sharp Regression Discontinuity Estimates of the Short- and Medium-Term Effects of Obtaining an Advanced Certificate, by Initial Employment Status: Additional Outcomes

| | (1) Quarter 1 | (2) Quarter 2 | (3) Quarter 3 | (4) Quarter 4 | (5) Quarter 5 | (6) Quarter 6 | (7) Quarter 7 | (8) Quarter 8 |
|---|---|---|---|---|---|---|---|---|
| **1. Unemployed** | | | | | | | | |
| 1a. Overall Employment | 0.038 | -0.019 | -0.013 | -0.050 | 0.000 | -0.055 | -0.043 | -0.022 |
| | (0.080) | (0.048) | (0.055) | (0.059) | (0.098) | (0.101) | (0.146) | (0.079) |
| Observations | 14,851 | 14,851 | 14,851 | 14,851 | 14,851 | 14,851 | 14,851 | 13,485 |
| Eff. # of Control Obs. | 616 | 894 | 858 | 891 | 497 | 497 | 492 | 557 |
| Eff. # of Treat. Obs. | 1,837 | 2,321 | 2,266 | 2,316 | 1,515 | 1,514 | 1,486 | 1,597 |
| Bandwidth | 3.589 | 4.822 | 4.073 | 4.723 | 2.592 | 2.561 | 2.276 | 3.374 |
| Mean | 0.549 | 0.575 | 0.568 | 0.600 | 0.642 | 0.628 | 0.620 | 0.614 |
| | | | | | | | | |
| 1b. Salaried Work | 0.181 | 0.046 | 0.084 | -0.005 | 0.070 | -0.058 | -0.046 | -0.087 |
| | (0.132) | (0.104) | (0.105) | (0.080) | (0.106) | (0.080) | (0.140) | (0.122) |
| Observations | 14,851 | 14,851 | 14,851 | 14,851 | 14,851 | 14,851 | 14,851 | 13,485 |
| Eff. # of Control Obs. | 497 | 497 | 497 | 631 | 497 | 615 | 493 | 458 |
| Eff. # of Treat. Obs. | 1,515 | 1,519 | 1,518 | 1,851 | 1,514 | 1,837 | 1,499 | 1,314 |
| Bandwidth | 2.593 | 2.715 | 2.612 | 3.937 | 2.521 | 3.568 | 2.402 | 2.694 |
| Mean | 0.489 | 0.513 | 0.503 | 0.509 | 0.533 | 0.509 | 0.513 | 0.509 |
| | | | | | | | | |
| 1c. Self-Employment | -0.059 | -0.089 | -0.043 | -0.045 | -0.046 | -0.018 | -0.013 | 0.007 |
| | (0.060) | (0.048) | (0.041) | (0.052) | (0.051) | (0.051) | (0.035) | (0.040) |
| Observations | 14,851 | 14,851 | 14,851 | 14,851 | 14,851 | 14,851 | 14,851 | 13,485 |
| Eff. # of Control Obs. | 271 | 494 | 610 | 606 | 606 | 497 | 894 | 784 |
| Eff. # of Treat. Obs. | 1,126 | 1,499 | 1,833 | 1,824 | 1,824 | 1,519 | 2,321 | 1,978 |
| Bandwidth | 1.975 | 2.458 | 3.444 | 3.149 | 3.147 | 2.714 | 4.963 | 4.182 |
| Mean | 0.063 | 0.059 | 0.061 | 0.073 | 0.091 | 0.078 | 0.089 | 0.098 |
| | | | | | | | | |
| 1d. Ln (Income) | -0.067 | -0.149 | -0.005 | 0.117 | -0.218 | 0.171 | 0.050 | 0.122 |
| | (0.105) | (0.084) | (0.073) | (0.073) | (0.118) | (0.116) | (0.068) | (0.088) |
| Observations | 7,676 | 8,156 | 8,378 | 8,521 | 8,800 | 8,936 | 8,967 | 8,158 |
| Eff. # of Control Obs. | 333 | 294 | 665 | 372 | 318 | 723 | 375 | 660 |
| Eff. # of Treat. Obs. | 915 | 841 | 1,643 | 1,038 | 875 | 1,733 | 1,094 | 1,576 |
| Bandwidth | 3.056 | 2.945 | 5.326 | 3.395 | 2.594 | 5.445 | 3.429 | 5.248 |
| Mean | 13.808 | 13.817 | 13.891 | 13.839 | 13.837 | 13.878 | 13.844 | 13.902 |
| **2. Self-Employed** | | | | | | | | |
| 2a. Overall Employment | 0.053 | 0.004 | 0.011 | 0.072 | 0.033 | 0.082 | 0.031 | -0.009 |
| | (0.055) | (0.068) | (0.049) | (0.048) | (0.037) | (0.063) | (0.072) | (0.086) |
| Observations | 16,502 | 16,502 | 16,502 | 16,502 | 16,502 | 16,502 | 16,502 | 14,692 |
| Eff. # of Control Obs. | 503 | 495 | 586 | 582 | 764 | 495 | 495 | 434 |
| Eff. # of Treat. Obs. | 1,619 | 1,615 | 1,810 | 1,808 | 2,388 | 1,615 | 1,615 | 1,414 |
| Bandwidth | 2.711 | 2.427 | 3.680 | 3.329 | 4.597 | 2.473 | 2.328 | 2.261 |
| Mean | 0.905 | 0.869 | 0.865 | 0.843 | 0.863 | 0.844 | 0.826 | 0.820 |

Continues in the next page...

Table OA12 Continued. Sharp Regression Discontinuity Estimates of the Short- and Medium-Term Effects of Obtaining an Advanced Certificate, by Initial Employment Status: Additional Outcomes

| | (1) Quarter 1 | (2) Quarter 2 | (3) Quarter 3 | (4) Quarter 4 | (5) Quarter 5 | (6) Quarter 6 | (7) Quarter 7 | (8) Quarter 8 |
|---|---|---|---|---|---|---|---|---|
| **2. Self-Employed** | | | | | | | | |
| 2b. Salaried Work | -0.009 | 0.018 | 0.062 | 0.189 | 0.135 | 0.166 | 0.201 | 0.127 |
| | (0.030) | (0.040) | (0.051) | (0.058) | (0.059) | (0.050) | (0.058) | (0.057) |
| Observations | 16,502 | 16,502 | 16,502 | 16,502 | 16,502 | 16,502 | 16,502 | 14,692 |
| Eff. # of Control Obs. | 581 | 581 | 494 | 223 | 224 | 503 | 494 | 513 |
| Eff. # of Treat. Obs. | 1,807 | 1,808 | 1,603 | 1,312 | 1,312 | 1,616 | 1,603 | 1,438 |
| Bandwidth | 3.162 | 3.251 | 2.057 | 1.816 | 1.936 | 2.583 | 2.059 | 3.001 |
| Mean | 0.041 | 0.086 | 0.122 | 0.112 | 0.112 | 0.141 | 0.156 | 0.154 |
| 2c. Self-Employment | 0.049 | -0.008 | -0.070 | -0.100 | -0.057 | -0.104 | -0.326 | -0.370 |
| | (0.051) | (0.055) | (0.085) | (0.077) | (0.085) | (0.081) | (0.175) | (0.230) |
| Observations | 16,502 | 16,502 | 16,502 | 16,502 | 16,502 | 16,502 | 16,502 | 14,692 |
| Eff. # of Control Obs. | 581 | 589 | 495 | 495 | 503 | 495 | 224 | 165 |
| Eff. # of Treat. Obs. | 1,805 | 1,817 | 1,603 | 1,603 | 1,616 | 1,603 | 1,312 | 1,154 |
| Bandwidth | 3.091 | 3.759 | 2.215 | 2.200 | 2.593 | 2.210 | 1.928 | 1.700 |
| Mean | 0.859 | 0.778 | 0.735 | 0.693 | 0.720 | 0.703 | 0.661 | 0.636 |
| 2d. Ln (Income) | 0.005 | 0.102 | 0.088 | 0.155 | 0.093 | 0.211 | 0.118 | 0.200 |
| | (0.037) | (0.048) | (0.064) | (0.069) | (0.043) | (0.069) | (0.057) | (0.068) |
| Observations | 14,972 | 14,495 | 14,305 | 14,182 | 13,983 | 13,800 | 13,729 | 12,267 |
| Eff. # of Control Obs. | 1,383 | 511 | 502 | 415 | 640 | 418 | 487 | 424 |
| Eff. # of Treat. Obs. | 2,924 | 1,595 | 1,591 | 1,392 | 2,008 | 1,351 | 1,525 | 1,345 |
| Bandwidth | 5.965 | 3.904 | 3.133 | 2.287 | 4.034 | 2.266 | 3.703 | 3.085 |
| Mean | 13.902 | 13.891 | 13.948 | 13.985 | 13.944 | 13.991 | 14.017 | 14.033 |
| **3. Salaried Worker** | | | | | | | | |
| 3a. Overall Employment | -0.002 | -0.013 | -0.010 | -0.008 | -0.010 | 0.006 | -0.016 | 0.015 |
| | (0.027) | (0.015) | (0.019) | (0.023) | (0.022) | (0.018) | (0.017) | (0.020) |
| Observations | 146,215 | 146,215 | 146,215 | 146,215 | 146,215 | 146,215 | 146,215 | 132,529 |
| Eff. # of Control Obs. | 6,318 | 6,326 | 6,443 | 6,326 | 6,322 | 6,430 | 7,661 | 6,813 |
| Eff. # of Treat. Obs. | 12,684 | 12,696 | 12,925 | 12,692 | 12,684 | 12,889 | 16,139 | 13,716 |
| Bandwidth | 2.112 | 2.299 | 2.956 | 2.229 | 2.118 | 2.859 | 3.917 | 3.060 |
| Mean | 0.949 | 0.933 | 0.926 | 0.921 | 0.910 | 0.899 | 0.889 | 0.887 |
| 3b. Salaried Work | -0.015 | -0.011 | 0.004 | -0.006 | -0.006 | -0.014 | -0.033 | 0.010 |
| | (0.046) | (0.019) | (0.021) | (0.023) | (0.018) | (0.018) | (0.021) | (0.016) |
| Observations | 146,215 | 146,215 | 146,215 | 146,215 | 146,215 | 146,215 | 146,215 | 132,529 |
| Eff. # of Control Obs. | 3,745 | 6,325 | 7,377 | 6,327 | 6,419 | 7,393 | 6,429 | 7,051 |
| Eff. # of Treat. Obs. | 9,512 | 12,684 | 15,645 | 12,770 | 12,800 | 15,645 | 12,851 | 14,205 |
| Bandwidth | 1.828 | 2.198 | 3.022 | 2.408 | 2.583 | 3.039 | 2.795 | 3.845 |
| Mean | 0.941 | 0.917 | 0.904 | 0.896 | 0.884 | 0.871 | 0.860 | 0.855 |

Continues in the next page...

Table OA12 Continued. Sharp Regression Discontinuity Estimates of the Short- and Medium-Term Effects of Obtaining an Advanced Certificate, by Initial Employment Status: Additional Outcomes

| | (1) Quarter 1 | (2) Quarter 2 | (3) Quarter 3 | (4) Quarter 4 | (5) Quarter 5 | (6) Quarter 6 | (7) Quarter 7 | (8) Quarter 8 |
|---|---|---|---|---|---|---|---|---|
| **3. Salaried Worker** | | | | | | | | |
| 3c. Self-Employment | -0.002 | 0.001 | -0.008 | -0.005 | -0.008 | 0.007 | 0.003 | -0.002 |
| | (0.006) | (0.005) | (0.005) | (0.005) | (0.006) | (0.007) | (0.006) | (0.006) |
| Observations | 146,215 | 146,215 | 146,215 | 146,215 | 146,215 | 146,215 | 146,215 | 132,529 |
| Eff. # of Control Obs. | 3,302 | 6,325 | 6,325 | 7,453 | 6,432 | 6,424 | 6,430 | 5,918 |
| Eff. # of Treat. Obs. | 9,148 | 12,684 | 12,684 | 16,042 | 12,916 | 12,849 | 12,857 | 11,336 |
| Bandwidth | 1.341 | 2.169 | 2.155 | 3.458 | 2.875 | 2.630 | 2.806 | 2.979 |
| Mean | 0.002 | 0.009 | 0.012 | 0.014 | 0.016 | 0.019 | 0.018 | 0.019 |
| | | | | | | | | |
| 3d. Acc. Job-to-Job Transitions | 0.051 | 0.044 | 0.05 | 0.047 | 0.052 | 0.065 | 0.099 | 0.122 |
| | (0.014) | (0.018) | (0.024) | (0.028) | (0.034) | (0.035) | (0.039) | (0.043) |
| Observations | 146,215 | 146,215 | 146,215 | 146,215 | 146,215 | 146,215 | 146,215 | 132,529 |
| Eff. # of Control Obs. | 6,326 | 6,326 | 6,326 | 6,327 | 6,326 | 6,326 | 6,316 | 3,451 |
| Eff. # of Treat. Obs. | 12,692 | 12,757 | 12,758 | 12,770 | 12,692 | 12,692 | 12,680 | 8,368 |
| Bandwidth | 2.245 | 2.320 | 2.376 | 2.449 | 2.263 | 2.251 | 2.006 | 1.873 |
| Mean | 0.052 | 0.090 | 0.121 | 0.148 | 0.179 | 0.203 | 0.226 | 0.246 |
| | | | | | | | | |
| 3e. Ln (Income) | 0.100 | 0.097 | 0.086 | 0.141 | 0.122 | 0.106 | 0.105 | 0.101 |
| | (0.037) | (0.039) | (0.036) | (0.035) | (0.035) | (0.034) | (0.034) | (0.034) |
| Observations | 138,151 | 136,228 | 134,891 | 133,826 | 132,261 | 130,891 | 130,005 | 117,239 |
| Eff. # of Control Obs. | 6,984 | 6,009 | 5,964 | 6,790 | 6,741 | 6,671 | 6,594 | 6,038 |
| Eff. # of Treat. Obs. | 12,170 | 12,021 | 11,855 | 14,364 | 14,090 | 14,267 | 13,887 | 12,181 |
| Bandwidth | 3.003 | 2.978 | 2.990 | 3.011 | 3.037 | 3.182 | 3.056 | 3.071 |
| Mean | 13.894 | 13.900 | 13.917 | 13.930 | 13.944 | 13.964 | 13.978 | 13.978 |

**Notes:** Each cell displays the estimate for the main coefficient of interest in Equation (1) for the advanced certificate, one to eight quarters after certification, by initial employment status: unemployed (first panel), self-employed (second panel), salaried worker (third panel). The outcomes analyzed are overall employment, salaried work, self-employment, probability of having switched jobs after certification (for salaried workers only), and log of income. The running variable is the exam score and the discontinuity threshold is 90. All regressions include the controls described in Section 4, use a triangular kernel, and use the bandwidth that minimizes the mean squared error of the local polynomial regression discontinuity estimator. We report robust bias-corrected standard errors below the point estimates in parentheses. Standard errors are clustered at the technical-norm level. Optimal bandwidths are displayed below the standard errors. The total number of observations changes across quarters given variation in the number of individuals with positive earnings (for log of income only); it further drops in quarter 8 due to missing information in the last quarter for individuals that certified in the last quarter of 2019. The effective number of observations changes across quarters given variation in the optimal bandwidth. The sample mean for the control group is displayed below the optimal bandwidth.

Table OA13: Sharp Regression Discontinuity Estimates of the Short- and Medium-Term Effects of Obtaining an Advanced Certificate, by Potential Experience: Additional Outcomes

| | **(1) Quarter 1** | **(2) Quarter 2** | **(3) Quarter 3** | **(4) Quarter 4** | **(5) Quarter 5** | **(6) Quarter 6** | **(7) Quarter 7** | **(8) Quarter 8** |
|---|---|---|---|---|---|---|---|---|
| 1. Experience: 15 Years or Less | | | | | | | | |
| 1a. Acc. Job-to-Job Transitions | 0.018 | 0.083 | 0.065 | 0.073 | 0.038 | 0.066 | 0.105 | 0.103 |
| | (0.022) | (0.047) | (0.056) | (0.062) | (0.075) | (0.076) | (0.078) | (0.079) |
| Observations | 16,374 | 16,374 | 16,374 | 16,374 | 16,374 | 16,374 | 16,374 | 15,244 |
| Eff. # of Control Obs. | 1,472 | 788 | 788 | 788 | 788 | 788 | 771 | 728 |
| Eff. # of Treat. Obs. | 3,225 | 1,457 | 1,457 | 1,460 | 1,458 | 1,456 | 1,442 | 1,304 |
| Bandwidth | 5.16 | 2.798 | 2.726 | 2.843 | 2.823 | 2.626 | 2.48 | 2.486 |
| Mean | 0.067 | 0.103 | 0.147 | 0.171 | 0.213 | 0.253 | 0.271 | 0.304 |
| | | | | | | | | |
| 1b. Ln (Income) | 0.235 | 0.261 | 0.230 | 0.171 | 0.225 | 0.201 | 0.207 | 0.112 |
| | (0.057) | (0.099) | (0.102) | (0.059) | (0.066) | (0.113) | (0.111) | (0.091) |
| Observations | 15,259 | 14,976 | 14,707 | 14,546 | 14,383 | 14,227 | 14,170 | 13,061 |
| Eff. # of Control Obs. | 733 | 813 | 812 | 784 | 779 | 782 | 766 | 967 |
| Eff. # of Treat. Obs. | 1,351 | 1,672 | 1,666 | 1,648 | 1,599 | 1,600 | 1,605 | 2,074 |
| Bandwidth | 2.621 | 3.331 | 3.506 | 3.394 | 3.209 | 3.673 | 3.546 | 4.921 |
| Mean | 13.909 | 13.910 | 13.929 | 13.947 | 13.951 | 13.962 | 13.981 | 13.977 |
| 2. 15< Experience <= 30 | | | | | | | | |
| 2a. Acc. Job-to-Job Transitions | 0.028 | 0.019 | 0.058 | 0.037 | 0.039 | 0.043 | 0.082 | 0.047 |
| | (0.014) | (0.018) | (0.026) | (0.022) | (0.030) | (0.036) | (0.040) | (0.043) |
| Observations | 73,826 | 73,826 | 73,826 | 73,826 | 73,826 | 73,826 | 73,826 | 66,732 |
| Eff. # of Control Obs. | 3,945 | 3,984 | 3,388 | 5,210 | 3,928 | 3,926 | 3,338 | 3,076 |
| Eff. # of Treat. Obs. | 8,352 | 8,373 | 6,582 | 11,360 | 8,298 | 8,142 | 6,507 | 5,729 |
| Bandwidth | 3.366 | 3.651 | 2.981 | 4.143 | 3.125 | 3.080 | 2.494 | 2.820 |
| Mean | 0.057 | 0.099 | 0.129 | 0.164 | 0.187 | 0.213 | 0.241 | 0.262 |
| | | | | | | | | |
| 2b. Ln (Income) | 0.019 | 0.032 | 0.038 | 0.122 | 0.098 | 0.061 | 0.067 | 0.069 |
| | (0.056) | (0.060) | (0.048) | (0.052) | (0.049) | (0.046) | (0.052) | (0.049) |
| Observations | 69,363 | 68,525 | 67,834 | 67,431 | 66,708 | 66,201 | 65,865 | 59,449 |
| Eff. # of Control Obs. | 3,164 | 3,106 | 3,095 | 3,076 | 3,044 | 3,042 | 3,011 | 2,709 |
| Eff. # of Treat. Obs. | 6,037 | 5,984 | 5,920 | 5,937 | 5,859 | 5,821 | 5,812 | 5,049 |
| Bandwidth | 2.115 | 2.165 | 2.312 | 2.489 | 2.459 | 2.514 | 2.516 | 2.309 |
| Mean | 13.896 | 13.913 | 13.929 | 13.930 | 13.944 | 13.964 | 13.978 | 13.971 |

Continues in the next page...

Table OA13 Continued. Sharp Regression Discontinuity Estimates of the Short- and Medium-Term Effects of Obtaining an Advanced Certificate for Salaried Workers by Initial Employment Status

| | (1) Quarter 1 | (2) Quarter 2 | (3) Quarter 3 | (4) Quarter 4 | (5) Quarter 5 | (6) Quarter 6 | (7) Quarter 7 | (8) Quarter 8 |
|---|---|---|---|---|---|---|---|---|
| 3. Experience: More than 30 | | | | | | | | |
| 3a. Acc. Job-to-Job Transitions | 0.043 | 0.054 | 0.040 | 0.041 | 0.037 | 0.061 | 0.069 | 0.079 |
| | (0.025) | (0.024) | (0.034) | (0.036) | (0.041) | (0.041) | (0.044) | (0.050) |
| Observations | 56,015 | 56,015 | 56,015 | 56,015 | 56,015 | 56,015 | 56,015 | 50,553 |
| Eff. # of Control Obs. | 2,215 | 2,217 | 2,218 | 2,218 | 2,218 | 2,218 | 2,218 | 2,040 |
| Eff. # of Treat. Obs. | 4,794 | 4,794 | 4,796 | 4,816 | 4,816 | 4,796 | 4,796 | 4,165 |
| Bandwidth | 2.113 | 2.145 | 2.236 | 2.387 | 2.352 | 2.293 | 2.267 | 2.188 |
| Mean | 0.042 | 0.071 | 0.100 | 0.124 | 0.150 | 0.169 | 0.190 | 0.212 |
| | | | | | | | | |
| 3b. Ln (Income) | 0.118 | 0.077 | 0.077 | 0.121 | 0.104 | 0.103 | 0.101 | 0.073 |
| | (0.047) | (0.048) | (0.043) | (0.046) | (0.043) | (0.038) | (0.035) | (0.040) |
| Observations | 53,529 | 52,727 | 52,350 | 51,849 | 51,170 | 50,463 | 49,970 | 44,729 |
| Eff. # of Control Obs. | 2,171 | 2,129 | 2,399 | 2,413 | 2,381 | 2,354 | 2,331 | 2,132 |
| Eff. # of Treat. Obs. | 4,664 | 4,557 | 4,526 | 4,517 | 5,190 | 5,219 | 5,168 | 4,536 |
| Bandwidth | 2.949 | 2.619 | 3.002 | 3.003 | 3.079 | 3.111 | 3.131 | 3.225 |
| Mean | 13.858 | 13.875 | 13.906 | 13.906 | 13.923 | 13.944 | 13.958 | 13.960 |

**Notes:** Each cell displays the estimate for the main coefficient of interest in Equation (1) for the advanced certificate, one to eight quarters after certification, for salaried workers (at certification) by potential experience: workers with 15 years or less, workers with more than 15 years but less than 30, and workers with more than 30 years. The outcomes analyzed are the probability of having switched jobs after certification and log of income. The running variable is the exam score and the discontinuity threshold is 90. All regressions include the controls described in Section 4, use a triangular kernel, and use the bandwidth that minimizes the mean squared error of the local polynomial regression discontinuity estimator. We report robust bias-corrected standard errors below the point estimates in parentheses. Standard errors are clustered at the technical-norm level. Optimal bandwidths are displayed below the standard errors. The total number of observations changes across quarters given variation in the number of individuals with positive earnings (for log of income only); it further drops in quarter 8 due to missing information in the last quarter for individuals that certified in the last quarter of 2019. The effective number of observations changes across quarters given variation in the optimal bandwidth. The sample mean for the control group is displayed below the optimal bandwidth.