Opportunities and Entrepreneurship: Evidence on Advanced Labor Market Experience*

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Abstract

This paper provides the first experimental estimates of the effects of managerial labor market experience on career outcomes and subsequent measures of business creation. Exploiting randomized lotteries for government contracts to manage construction projects in the Dominican Republic, we show that after five years, individuals who win contracts are (i) 7.8 percentage points more likely to be owners of formal firms and (ii) less likely to be private-sector employees. Also, the firms created by lottery winners are more likely to hire and survive than those of non-winners. We use a selection model to recover the distribution of heterogeneity that drives selection into and returns from contractual experience. We find that workers select in based on potential gains and that the young drive firm creation. Since individuals with higher marginal benefits are willing to incur higher costs to participate, reducing the number of contracts allocated in the same event, or increasing application costs, would screen out applicants who may benefit from the program the least.

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1 Introduction

Firm creation plays a pivotal role in markets by introducing innovation, fostering competition, and driving job growth. Developing countries lag in measures of entrepreneurship along many dimensions, including formal firm creation and business growth, despite an abundant supply of self-employed individuals (La Porta and Shleifer, 2014). Many interventions designed to tackle this shortfall, by providing financial capital, business training, or assistance in hiring, have not substantively affected formal firm creation or growth.[1]

These interventions may fall short because they (i) fail to address other constraints to business creation, or (ii) do not target the individuals most likely to benefit. In this paper, we provide the first experimental evidence of a different intervention—temporary labor market experience in managerial roles—and find that it substantially increases the creation of new, growing businesses. We then estimate a structural model using repeated costly choices to apply for this experience to identify entrepreneur self-selection. We show that costly application processes improve targeting. Taken together, our results highlight a significant stepping stone to business creation, managerial jobs, and suggest that many entrepreneurial intervention designs, by reducing participation costs, may not target those most likely to benefit.

A recent literature points towards the importance of both managerial job experience and targeting. Managerial job experience is correlated with entrepreneurship (Liang et al., 2018), but, to date, finding a source of exogenous variation has been elusive.[2]

Suggestive evidence on the importance of targeting comes from Meager (2019), which finds significant heterogeneity in returns to entrepreneurship interventions. However, potential entrepreneurs are not easily identified from observable characteristics (McKenzie and Sansone, 2019); finding ways to target based on unobservable potential is critical. A natural targeting method, which is used in our setting, is to levy costs on applicants for the program. If people have private information about their potential, then a simple Roy model suggests that this can improve targeting. We find that this is indeed the case.

We study these two questions—the effect of managerial experience on entrepreneurship, and the importance of targeting the intervention towards high-ability individuals—using an atypical procurement scheme in the Dominican Republic (DR). This scheme randomly assigns contracts to manage the construction of schools, hospitals, and housing projects to applicants from the population of licensed civil engineers and architects. From 2012 to 2015, the government allocated over 2300 high-value contracts using this mechanism across a series of lottery events. The recipients primarily engage in managerial tasks including: hiring employees, purchasing materials, finding subcontractors, budgeting, and overseeing the execution of the work. Contract values are also assigned randomly within application groups and directly affect the amount of income received.

Our study context addresses several limitations common to experimental studies of labor

[1] Reviewed in Woodruff (2018).

[2] Consistent with the importance of managerial experience, middle-age individuals are much more likely to become entrepreneurs than the young and old (Azoulay et al., 2018).

market experience in developing countries (McKenzie, 2017a). First, we can study medium-run impacts by linking program randomization to novel, high quality administrative and self-collected survey data to measure a wide range of outcomes up to five years after the allocation of contracts. These include formal wage employment, income, firm creation, and future work with the government. We also collect information on firms owned by individuals in this sample (size, survival, sales) to understand patterns of entrepreneurial quality. Second, our sample size is large because the national scale of the program induced widespread entry into these lotteries. Over 80% of recently licensed engineers entered at least one lottery event. Finally, we investigate general equilibrium impacts by using spatial and temporal variation in contract allocation to examine local labor market employment externalities from this program.

We proceed in three steps. Relying on the lottery design, we first provide intention-to-treat estimates documenting the effect of winning a contract on a range of outcome measures. Second, we use a selection model to recover the distribution of latent heterogeneity driving both selection into and returns from contractual experience. We then use this to explore potential changes to program targeting. Lastly, we conclude with a fixed effect analysis to explore possible program spillovers by looking at patterns of non-recipient employment.

We first use reduced-form evidence comparing lottery winners to non-winners to document that the contracts lead to subsequent measures of business creation or entrepreneurship in the long-term. Lottery winners do not start firms at the time of contract receipt, but shift into firm creation and ownership as the contracts finish. Five years later, winners are 7.8 percentage points (22%) more likely to have started a firm. This effect is matched by a similar shift out of wage work in the long-term, indicating the measured firm creation is not the result of the formalization of existing informal firms. The newly created firms are predominantly in the same sector, construction, but lottery winners also start more firms of other types than non-winners. Additionally, lottery winners see a substantial increase in net income, but the effect decays and incomes are similar across groups after five years.

The combined set of results suggests that the increases in business creation we observe are primarily driven by increases in managerial experience. We first show the firms are not created to access further public sector contracts or as a result of an improvement of potential firm co-ownership networks. To differentiate between managerial experience and access to financial capital, we look at heterogeneous effects of the program based on randomly assigned contract values. Compared to other contracts, higher value contracts increase income, but lead to small, insignificant, and negatively-signed estimates on firm creation and growth. Thus, observable variation in contract size and subsequent income levels does not seem to be associated with differential measures of business creation. This suggests to us that lottery-induced access to future capital or credit may not be a significant mechanism here. It is difficult to formally rule out though because credit constraints could be highly non-linear, and we do not observe the credit access of created firms.

A central concern of interventions designed to generate new entrepreneurs is the quality of their newly created firms.[3] We find that firms created by lottery winners are more likely to have hired employees and survived through their second year of business than firms created by non-winners. Thus, the program may have beneficial impacts for long-term firm development.

In the second part of the paper, we use a selection model to evaluate self-selection and generalize our treatment effects beyond the sample of lottery participants. We link panel data on individuals' choices to apply to the lotteries to uncover both observed and unobserved heterogeneity in preferences to participate. Variation in lottery choices shifts individuals in and out of application over time, while the lottery randomization induces variation in treatment status throughout the distribution of preferences. We nest our structural preference estimates into a model of outcomes using a two-step control function approach, as in Walters (2018), to uncover aggregate treatment effects and show which sources of heterogeneity drive our estimated effects.

We find evidence of self-selection into this program from individuals who are unobservably always more likely to be firm owners and unobservably more likely to become firm owners as a result of contractual experience. The allocation of contracts across a series of lotteries in this context selects individuals who are willing to repeatedly incur the costs of applying. Given that individuals with higher marginal benefit are willing to incur higher costs to participate, reducing the number of contracts allocated in the same event, or increasing application costs, would select individuals more likely to start firms as a result of this program. This model also shows that the effects of the program are highest for young individuals.

Finally, to assess the aggregate effects of this government hiring, the third section of our paper explores whether non-winners' employment and income varies with the share of individuals who received a government contract in their local labor market. There is substantial variability in the number of contracts disbursed. By 2015, the province with the lowest (highest) share of government contracts to individuals is 4.2% (16.7%). Utilizing a panel fixed effects approach that controls for time-invariant individual, age, and region by time differences, we find that an additional 1% of the local labor market with government contracts leads to a 0.6% *increase* in employment for non-winners.[4] Yearly incomes for non-winners are also higher, but the difference is statistically insignificant.

Overall, these results show that managerial jobs can be an important path to entrepreneurship. However, numerous reasons suggest they may be under-provided in developing countries. Contract enforcement and monitoring issues of managers are cited as a reason by In-

[3] A growing literature has emphasized heterogeneity in entrepreneur quality and their resulting success. See, for example, Schoar (2010); Hurst and Pugsley (2011); Haltiwanger et al. (2013); Humphries (2016); Levine and Rubinstein (2017); Hombert et al. (2017); Gendron-Carrier (2018).

[4] The estimated effects of government contractual hiring on non-winner employment do not change the conclusions from the analysis of direct program effects. The reduced-form treatment-control comparison shows that the contract offers reduced employment by 12 percentage points. By scaling up the indirect effects to the level of the full sample, we conclude that the indirect effects account for a 4.8 percentage point increase in control group employment levels. Therefore, accounting for the general equilibrium effects on employment do not explain the substantial shift away from employment for contract recipients. The "crowd-in" of employment in the control group highlights the importance of studying the aggregate implications of interventions of this type.

dian firm owners to create these jobs only for their male family members (Bloom et al., 2013). More related to this work, if firms believe managerial experience will cause employees to create competing firms, they will under-provide these jobs and experiences. Each of these market failures would provide reasons behind the low skill accumulation from job experience in developing country labor markets (Lagakos et al., 2018).

This paper contributes to several areas of existing research. First, we contribute to the literature on the role of human capital, and, more specifically, managerial experience, to spur business creation and other measures of entrepreneurship. The existing literature has found mixed results of the importance of labor market experience (Evans and Leighton, 1989; Lazear, 2005; Silva, 2007; Humphries, 2016; Hincapié, 2017; Gendron-Carrier, 2018). This paper is most closely related to Liang et al. (2018), who examine, through a model and associated cross-country regression analysis, the role of managerial experience in generating entrepreneurship. We see our contribution as establishing a causal relationship behind their mechanism.

Second, we contribute to work studying entrepreneurship and labor markets in developing countries. A literature on management interventions has found somewhat conflicting results—while intensive, individualized management consulting is beneficial for existing firms (Bloom et al., 2013; Bruhn et al., 2018), generalized training is not (reviewed in Quinn and Woodruff, 2019), and managerial internships may have entrepreneurial effects only for individuals placed at high-quality firms (Abebe et al., 2019). We show business-creation effects of a new intervention: temporary experience in a managerial position. There have been a large number of evaluations of job experience and public sector employment programs in developing countries, but these programs are often for entry-level or low-skill employment (reviewed in McKenzie, 2017a).

Third, we contribute to the literature studying self-selection in program take-up for potential entrepreneurs. Recent work in developing countries shows self-selection in program application based on observable characteristics (Alatas et al., 2016), but self-selection in entrepreneurship has been more difficult to separate from other screening mechanisms used in program allocation (McKenzie, 2017b; Fafchamps and Quinn, 2017; Beaman et al., 2014). Our combination of lottery randomization with a selection model is most similar to Walters (2018) and van Dijk (2019).

Fourth, we contribute to the literature focusing on studying "at-scale" interventions. Recent critiques in development economics discuss how ambiguous theoretical predictions on the net effects of interventions motivate moving beyond typical small-scale RCT approaches (Acemoglu, 2010; Muralidharan and Niehaus, 2017). This paper is most closely related to papers on interventions that study the aggregate effects of improved public workfare programs in India (Muralidharan et al., 2017), job search assistance in France (Crépon et al., 2013), and unemployment insurance (Lalive et al., 2015; Schmieder et al., 2016; Johnston and Mas, 2018).

Section 2 provides background on the Dominican procurement system. Section 3 explains our data and applicants to the program, Section 4 describes the lottery-based analysis and main results, Section 5 contains the selection model and associated results, and Section 6 analyzes the local labor effects of this program. Section 7 concludes.

2 The *sorteo de obras*

In 2006, the Dominican government revamped its public procurement system by updating the protocols for its existing procurement schemes, creating new schemes, and instituting a new governing and supervisory body, the General Directorate of Public Procurement (*Dirección General de Compras y Contrataciones*, DGCP). Since 2007, the DGCP has maintained the national registry of state suppliers (*Registro de Proveedores del Estado*, RPE) and managed records on all procurement processes in the country. Although each government agency is responsible for executing its own procurement processes, the DGCP establishes the protocols to follow (*pliegos de condiciones*) for the different procurement schemes and publishes the processes in an online portal (*Portal Transaccional*, PT).

The same 2006 reform mandated that a new procurement scheme, the *sorteo de obras*, a true random allocation of government procurement contracts to applicants, is to be used for any service project falling within specific contractual size thresholds, which vary over time.[5] [6] The process of implementing this procurement scheme is generally homogeneous and is shown in Figure A1. First, the procuring institution announces the opening of the procurement process, which includes posting it in the PT. Second, all interested RPE members can sign up to participate in the process. The window between the posting of the process and the closing date to declare interest is large enough that non-RPE inscribed individuals and firms can sign up and participate. Following the closing date to declare interest, the government institution running the process reviews all the participants and disqualifies those who do not meet the criteria. Third, all eligible participants are entered into the lottery of their choosing. Fourth, the selection of winners is a public affair. Entrants attend the lottery at a common location to enter their name into a randomization device, typically on a piece of paper placed into a transparent cylinder. A government official spins the cylinder and pulls the winning entry (see Figure A2a). Lottery winners are then required to take out an insurance policy

[5]Among other things, the new procurement scheme was meant to address the lack of democratization, transparency, and oversight of the country's bloated and corrupt procurement system (Artana et al., 2006). The program is increasingly advocated on the basis of providing greater opportunities to skilled workers who struggle to advance in their careers.

[6]It is common for procurement agencies around the world to provide some equity regulations to incentivize new entrants. This is a more extreme example, but is not wholly unique. This paper shares the use of some form of randomness in procurement contract allocation with (Ferraz et al., 2015; Lee, 2017; Fadic, 2018), but is differentiated by studying different phenomena and *a priori* differences in expected mechanisms, and by developing numerous extensions to our analysis. First, the sample here is primarily individuals, enabling us to study phenomena related to entrepreneurship and career evolution rather than within-firm changes. Analyzing small to medium-sized firms, the mentioned papers generally find that firm size increases as a function of shocks to firm demand and that these effects can be long-lasting. Our results on firm effects are generally in agreement but are a small part of our study. Second, since almost no entrants had previous experience with similar contracts, the treatment in this setting has a high potential to generate learning; other studies are much less likely to generate such learning. For example, in the case of Fadic (2018), 95% of baseline firm revenues came from the Ecuadorian government. Finally, we are further differentiated by having secondary forms of randomization to delve deeply into mechanisms, by analyzing self-selection, and by studying general equilibrium effects.

within one week of the lottery date and then begin the contractual period.

A single *sorteo* process (i.e., a lottery event) can range from one to hundreds of offered contracts, with contracts often split into mutually exclusive groups (hereafter referred to as blocks). The blocks segment the contracts offered, often by location—regionally or provincially—or sometimes by occupation type. Interested applicants may enter only one of these contract blocks per lottery event. Within the blocks, there are almost always multiple contracts offered. For each contract, first, second, and third place winners are chosen. In case the lottery winner is unable to complete the requirements of the contract, the second place winner will get the contract. The process is analogous for the third place winner. Within a lottery event-block group, all entrants have the same probability of winning one of the contracts and the first place winners are chosen without replacement so that no entrant can receive multiple contract offers. Lottery losers are allowed to enter future lottery events, but winners are restricted from entering again until finishing their previous contract.

Eligibility requirements to the lottery events are generally common but may vary. The basic requirements include having relevant professional licensing (exequatur), registration as a state provider (RPE), being up-to-date on taxes, and not currently working on a government contract. Some lottery events impose either regional requirements (e.g., applicants must come from a particular region) or experiential (e.g., usually between 0-2 years of relevant experience), although many impose none. To be in the RPE, an individual or firm has to fill out a form (in-person or online) and meet six criteria: (i) have a national ID; (ii) be registered with the tax authority and up-to-date with taxes; (iii) present a sworn statement that they are not related in the first or second degree to a public official; (iv) for certain individual professionals, be a member of their respective association (exequatur); (v) for certain economic sectors, have a certification of competency; and (vi) for firms, present a list of shareholders. Upon receiving all the required information and documents, the DGCP usually clears an individual or firm to be in the RPE within three days.

The costs of application to these lottery events are non-monetary and reflect the opportunity cost of time. Applications require that individuals prepare and submit paperwork to verify their eligibility and attend lottery events. Learning about the events and where to apply signifies a secondary potential cost in the case of information frictions. As there are multiple events, entrants who repeatedly enter are required to incur these application costs for each event.

Winners are responsible for all implementation and management of the contracts. The relevant institution procuring services alongside the DGCP establishes protocols to follow and provides plans for the hired entrant to implement. The contract recipient takes these plans and completes the primarily managerial job of overseeing the process. Typically implementation includes finding workers, materials, and subcontractors for the more technical aspects. This process involves planning and coordination across a large number of entities. Contract recipients receive 10% of the contract size as payment. The contract size in *sorteo* contracts is preset, but, as in construction projects around the world, cost overruns are possible.

Figure 1a presents a histogram of entrants and winners across the lotteries. The ratio of entrants to winners is large: the contracts are oversubscribed by a factor of 26, indicating a high revealed preference valuation. More contracts were offered in the years 2012-2014 due to a high volume of government infrastructure spending corresponding to a school construction drive. Figure 1b documents the exact timing of lotteries and number of contracts offered in more detail. There are two salient details from this figure. First, there are a number of lottery events that happen over time. Second, the majority of contracts were from four of the lottery events.

The contract sizes vary greatly, from tens of thousands to millions USD, with the average project size around \$640,000 (Figure A3). Within each lottery event-block, there is still considerable variation in the size of the winning contract. This variation is random to individuals since they apply only at the lottery event-block level. A one standard deviation change in within event-block contract size of about \$250,000 maps directly into an income standard deviations for the winner of \$25,000.

3 Data

This project draws from an array of administrative datasets. We document them below. A discussion of matching between datasets follows.

3.1 Datasets

State suppliers: From the DGCP, we obtained the registry of state providers with information up-to-date as of May 2018. For every unique national ID, the DGCP assigns a unique state supplier ID (RPE number), which serves as identification across all the procurement databases. The RPE dataset contains basic demographic information, date of registration, and contact information.

Procurement processes: For each procurement processs since 2007 overseen by the DGCP, the DGCP collected and provided us with: (i) date the contract began; (ii) government institution; (iii) contract ID; (iv) procurement scheme; (v) contract amount; (vi) brief description of the goods, services, or works being contracted; and (vii) RPE number of winning supplier. For all of the lotteries in our sample, we supplemented the DGCP's information through Freedom of Information requests to the relevant government agencies that carried out the procurement calls. From them (*e.g.*, Ministry of Education, Ministry of Public Works and Communications), we obtained information on all the individuals who applied and qualified (*habilitados*), and were chosen as first, second, or third place, for all the lottery events. Likewise, from the Ministry of Education, we obtained information on the percentage of completion for each of the awarded contracts. We use these records to construct the randomization and sample and also as outcomes.[7]

[7] We are missing some of the minor *sorteo* events primarily in the most recent years.

Tax filings: All individuals and firms economically active in the DR have to meet yearly tributary obligations based on their economic output and transactions. The DGII, the country's tax collection agency, provided yearly information for all individuals and firms in our sample from 2007 to 2018. Individuals are required to submit a yearly tax declaration to DGII. From these declarations, we obtained information concerning gross income, profits, number of employees, employment, and salary (not from self-employment) income. These records are augmented with income reported from other declaring sources, to mitigate issues of income misdeclaration. In the case of conflicting reports of income between self-declaration and declaration from other sources, we take the maximum value. For firm entrants, we obtained information concerning their income, profits, and number of employees.

Firm ownership: For our study sample, we use a yearly, anonymous list of all firm holdings, the beginning and ending date of this holding, the approximate date of firm registration with DGII, and their percentage of participation from 2010 to 2018. Random but unique identifiers identify all firms owned in the sample. We further measure firm income, profits, and size for all owned firms in the sample for the years 2010 to 2018 using DGII tax records.

Intellectual property: From the National Office of Industrial Property (*Oficina Nacional de la Propiedad Industrial*, ONAPI), we obtained the commercial trademarking on all business names, and distinctive business signs in the country. Following the procedure in Appendix A1, we matched the respective records to national IDs. For individuals, we matched based on whether they are listed as one of the persons of record for commercial names (proxy for greater firm formalization) and business signs. If an individual is listed as a firm owner in either DGII or ONAPI records, we count business signs owned by the firm as also owned by the individual. These records provide a second measure of firm ownership and provide greater information on firm activity.

Survey: To complement the administrative data, we conducted our own survey. Reflecting the design of the program, we stratify the random sampling by the lottery event-block. We sampled 2,038 individual by event observations in our analysis events comprised of 1,925 unique individuals from events between 2012 and 2015. Within event-blocks, we sampled the same number of lottery winners as non-winners but include the condition that we always sample at least 10 non-winners. The survey took place over the phone or in-person between late April and early July 2019. We successfully surveyed 716 unique individuals, which corresponds to 765 entrant by event observations. Survey response rates are 36.8% and are not differential between treatment and control. The survey primarily adds to this study by showing greater detail on potential mechanisms including direct management experience and loan activity.

Exequatur: Upon completion of a tertiary degree, certain professionals in the DR—including engineers and architects—have to petition the President to be allowed to practice in their respective field. In order for Dominican engineers and architects to get an exequatur, their formal professional licensing, they have to present the Presidency's Legal Office with a certified copy of their university degree and their letter of intent to join the Dominican College of Engineers, Arquitects, and Surveyors (*Colegio Dominicano de Ingenieros, Arquitectos, y Agrimensores*, CODIA).[8] Whereas being in the RPE implies that one has an exequatur and is a member of CODIA, the data obtained from the Presidency's Legal Office gives us the exact date the Presidency granted the exequatur.[9] We further use this data to identify all individuals in the country who were eligible to enter a lottery.

Matching: We provide a summary of matching between datasets here and include a full description in Appendix A1. The vast majority of matching between datasets is done through unique, administrative identifiers: the national ID or the RPE number. Almost all administrative datasets have these identifiers. Participation records from the randomly-assigned procurement lotteries come with one of the two unique identifiers 72.5% of the time. When it is not included, we match records using the entrant's full name which finds a unique match for almost all of the observations. This is due to the high name uniqueness in this setting and since we can often match to the list of state providers rather than the full population.

3.2 Descriptive statistics

3.2.1 Entrants

Table 1 summarizes the characteristics of the entrant population in the full sample in column (1) and in the analysis sample in column (4). Each observation corresponds to the entrant by lottery event-block level. All entrants, individuals and firms, are included. We describe the entrant population for the analysis sample here, which accounts for 32.5% of the overall sample. As mentioned, we restrict our main reported results to these events to maximize the post-contract period to measure outcomes. On average, entrants have entered 0.8 lotteries previously. The sample is 87% non-firms, which correspond to individuals who enter a lottery on their own behalf. The other 13% of entrants are firms registered with DGII and entered under the firm title. The majority of entrants are male, which reflects that men are more represented in the engineering and architecture sectors. The vast majority are taxpayers in the period prior to the lottery (82%), have never won a previous contract with the government (98%), and have few employees (0.28 for individuals, 4.27 for firms). In the year prior to the events, 10% of individuals are owners of firms.

[8] Membership in CODIA is conditional on the Presidency granting the exequatur.

[9] Since one is supposed to obtain the exequator right after graduating university, the date one was granted the exequator is a good proxy for age and professional experience.

3.2.2 Comparison to non-entrants

We compare entrant to non-entrant individuals using exequatur records, the registry of all licensed civil engineers and architects.[10] We show selection into the program based on years of being professionally licensed and by yearly income within age groups in Figure 2. Panel (a) shows that individuals who received licensing more recently are more likely to enter the lottery than individuals who have been licensed for more time. However, the share of those who ever entered a lottery is high across the distribution; while 80% of individuals who received licensing in the years 2010-2012 applied at least once, so did about 50% of those who received their licensing 20 years earlier.

Panel (b) of the same figure shows selection by income within five-year age bins. Individuals 20-35 who applied look very similar to same age non-entrants. However, older entrants appear negatively selected on income. Non-entrants report higher income. This income gap appears to be persistent throughout the distribution of individuals above age 35. This age-based selection suggests that a simple comparison of the effects of the program by age may not be generalizable to the full population. This is likely true of all labor market interventions that are dependent on some selection stage.

4 Reduced-form analysis and main results

4.1 Empirical strategy

To estimate the effect of temporary managerial job experience on career and business creation outcomes, we exploit the random assignment of public contracts. Our main specifications take the following form:

$$y_{iebt} = \beta \mathbb{1}[\text{winner}]_{ieb} + \gamma_{eb} + \varepsilon_{iebt} \tag{1}$$

where y_{iebt} is the outcome of interest at time t for entrant i who applied to the set of contracts at event e in block b. The fixed effect γ_{eb} restricts comparisons within the set of individuals who applied in the same lottery event and block, and hence had the same probability of receiving a contract. The indicator $(\text{winner})_{ieb}$ is a 1 if individual i won one of the lotteries (i.e., came in first) in block b at event e and 0 if not. Time or period t is defined to be relative to the year of lottery occurrence. Consequently, period 0 and 1 correspond to the year of the lottery event and the year afterwards, respectively.

The program design of a series of randomization events (i.e., lotteries in this case) is analogous to a number of recent papers (Cellini et al., 2010; Gelber et al., 2015). We follow their data setup by creating the analysis dataset at the entrant by application level. This corresponds to multiple stacked panel observations for repeat entrants, with the observations in reference to the relative period structure. Across all specifications, we cluster at the individual level to account for correlated outcomes that arise because of the inclusion of the same

[10] The same exercise for firms is more difficult because we are not able to identify the universe of eligible firms.

entrant across multiple lotteries. All main estimates are intention-to-treat. Figures showing the evolution of treatment effects over time are run as period-by-period regressions of Equation 1.

Because lottery winners are restricted from entering other events if they have an unfinished contract, the effect of lottery winning may affect the likelihood of future lottery winning. In the case of differential future lottery winning, Equation 1 measures both the effect of lottery winning on subsequent career outcomes and the dynamic effect on future lottery winning. In Appendix A2, we adopt the approach of Cellini et al. (2010) to estimate treatment effects that remove the channel of future lottery winning. The estimator comes at the cost of imposing assumptions on treatment effect homogeneity that are potentially restrictive. Since the effect of lottery winning on subsequent lottery winning is modest, the estimates across both approaches are similar.

The lotteries provide randomized priority lists of up to three positions, but the described research design only exploits the winning lottery position. As discussed in de Chaisemartin and Behaghel (2018), our estimator, labeled the "Initial Offer" (IO) estimator, is consistent, but may lead to efficiency losses. Take-up rates in this randomization are extremely high, so efficiency losses are negligible.

We modify the regression above to look at intensive margin variation in contract size. Our main specification takes the following form:

$$y_{iebt} = \beta \mathbb{1}[\text{winner}]_{ieb} + \tau \mathbb{1}[\text{winner}]_{ieb} M_{ieb} + \gamma_{eb} + \varepsilon_{iebt} \tag{2}$$

where M_{ieb} is an intensive margin characteristic of the job, such as the awarded contract size. Our interest in contract size is to understand whether there are potential capital and scale effects on future outcomes. For example, a small contract may not allow individuals and firms to overcome financial capital constraints that would be ameliorated by larger contracts. Figure A3 shows there is large variation in offered contract sizes. The intensive margin characteristics are orthogonal to unobserved factors of the participants conditional on the event-block fixed effects, γ_{eb}. This is because multiple jobs are allocated randomly within the same event-block and applicants are not permitted to apply to specific contracts within an event-block.

We provide validation of the randomization of lottery winning in Table 1. In columns (3, 6) of this table, we regress observable characteristics of the entrants on lottery winning and event-block fixed effects. Lottery winners are not statistically different from non-winners along any of the dimensions we test. Not surprisingly, a joint test of whether these observable characteristics are correlated to lottery winning fails to reject the null hypothesis of no correlation ($p = 0.42$ overall and $p = 0.66$ for individuals in the analysis sample). Analogously, we show that contract size is uncorrelated to observable characteristics of entrants in Table A1 (joint test $p = 0.94$).

This randomization procedure is relatively unique for providing true random variation in important intensive-margin characteristics. Even under our fortuitous conditions, the exclusion restriction required for our estimates of intensive-margin effects may still be violated if other characteristics of the contracts vary with the size of the contract (e.g., the government provides additional assistance to these candidates). We are unaware of any such correlated treatments.

4.2 Main results

4.2.1 Sample definition

We restrict our sample to lottery events where we observe completion records and require the median contract to be finished within two years from the lottery date. We chose this to minimize the likelihood of overlap between time directly working on the government contracts and the time afterward. We also split the analysis by individual entrants and firm entrants because of the sample size differences, the different outcomes for these two agents, and the large mean level differences in outcomes. If we did not, the individual sample would dominate the results due to its much higher representation among entrants (87% of the sample).

4.2.2 Career outcomes

Figure 3 shows the main sectoral effects of winning a lottery for a government contract in the population of individual entrants. We display figures for all of these estimates to allow the reader an understanding of the timing of the treatment. Panel A shows the effect of winning a lottery contract on the likelihood of having a lottery contract that is open or unfinished. The likelihood of having an open contract is highest in periods 0-1 and falls to 0.35 by period 3. The rate of contract completion is slower in the final 2 periods, with 23% of lottery winners still having an open contract 5 years later. Thus, there still appear to be some individuals who have not finished. Many of these unfinished contracts have been held up due to delays from the government in acquiring a land parcel for the project or other necessary inputs such as government payments.

Panel B shows the evolution of wage employment. Individuals may manage their government contracts while working at another job, but we see that many exit formal wage employment. In period 1, lottery winners are 8.7 percentage points less likely than the control group to be working for a wage in the private sector in that year. This decline in formal wage employment continues and stays relatively steady. After 5 years, lottery winners are 12 percentage points less likely to have any wage employment. This evolution in wage employment does not follow the trend in project completion rates. Even as many lottery winners finish their contracts, there is little evidence of a return to wage employment.

In Panel C, we see that lottery winners are more likely to own and start firms.[11] To reduce the possibility of double-counting firm creation in the case of multiple firm owners, we conservatively scale firm outcomes by firm ownership shares and the share of months in

[11] Table A5 shows that firm ownership and firm creation are almost the same in this sample. We define firm creation as registering ownership shares of a firm within 6 months of the time of the firm's registration of existence. Using this definition, firm ownership and firm creation lead to indistinguishable estimates of the main effects, 0.078 and 0.069, respectively.

the year when the individual is an owner.[12] Lottery winners have similar firm ownership as non-winners in periods 0-1, the years of most active work on the government contracts. In year 2, when individuals begin to finish contracts, firm ownership becomes significantly positive. By period 5, lottery winners have 0.053 more firms.

We examine the effects of contract offer on owned firm size, scaled by participation shares, in Panel D. In periods 0-1, we find that the estimated effects on owned firm size are negative and insignificant. The effect is positive and highly statistically significant in periods 4-5 when the majority of contracts have finished. Throughout the study period, the effects continue to grow and seem to lag the effect on owned firms, as one would expect from businesses that continue to expand. This measure corresponds to aggregate increases in employment as a function of firm ownership, not including the owner as one of the employees.

In Table 2, we report the effects on firm creation and firm characteristics more thoroughly. We aggregate firm outcomes and characteristics to the level of the individual, with outcomes unconditional on firm ownership, as in McKenzie (2017b).[13] Columns (1-2) show lottery winners are more likely to ever become firm owners and to become owners of more firms. Column (3) restricts firm ownership to 2018 and scales by the mentioned shares. Columns (4-6) report firm characteristic outcomes. We see that winners account for proportionally higher employment in their business and higher incomes. We see no differential effects on firm profits, but the relatively low control group means suggest possible tax avoidance.[14]

Table 3 reports the effects of lottery winning on individual income and profits. Throughout all periods 0-5, results on total income are positive and statistically significant. In periods 0-2, total income is highest because individuals are being paid the full contract amount as income. These effects decay to about a tenth of the size by the final period. Net income differences are large in the first periods, but the difference in net income decays and incomes are similar between the groups in period 5. Net income is somewhat hard to measure because in early periods some contract recipients report no costs despite clearly having project associated costs. Furthermore, individuals who start new firms may be better able to shield their firm profits or income. In general, though, these results suggest important benefits for the incomes of individuals in the sample.

Overall, individuals who win lotteries shift out wage employment and into firm creation as contracts are finished, and have greater incomes than lottery non-winners.

In Appendix A3, we investigate the effect of lottery winning on the sample of firm entrants. Firms finish contracts at a similar rate to individuals. In the period after the heavy contractual period, lottery winning firms are more likely to sell in the private sector as the contracts finish, but the difference decays by the final period. Firm size increases during the

[12]Firm ownership, called FO, is denoted using the number of firms owned, N, the share of the firm the individuals owned, S, and the share of months in the year that the individual was a firm owner, M. That is, $FO = \sum_{i=1}^{N} S_i \cdot M_i$. This procedure more accurately represents firm ownership in the case of non-unique owners and reduces concerns of double-counting if, for example, lottery winners are more likely to co-own firms. In the case of two winners co-owning a firm, we count that firm only once, rather than twice in the case of an unadjusted firm ownership variable.

[13]Individuals who do not own firms receive a 0 for firm characteristics.

[14]The DR has a value-added tax system that taxes profits at each stage of transactions. This system is commonly used to reduce tax avoidance and non-compliance, but falters when firms can inflate their costs.

period of heavy contracting and appears to stay large, but becomes insignificant. Overall though, it is hard to fully assess the effects on existing firms who receive these contracts. The results are underpowered due to the much smaller sample for firms than for individuals, the much larger heterogeneity in firm outcomes, and the analysis sample restriction we make in order to document project completion.

4.2.3 Mechanisms

We first show that the firm creation results are unlikely to arise: from trying reduce liability on their randomly assigned government contract, from trying to acquire further government contracts, or increased firm co-ownership networks. Our results highlight how young individuals are most likely to create new firms. We also document that contracts provide new managerial experiences, and variation in incomes received arising from randomly assigned contract sizes does not affect firm creation.

Government-specific firm creation: Individuals in this context may start a firm to reduce personal business liability on their randomly-assigned government contract or to win future government contracts. We do not find evidence for either of these possible mechanisms.

Two pieces of evidence indicate that firms are not started to reduce personal liability during the lottery contractual period. First, at the beginning of the contractual period, the contract recipient is required to take out an insurance policy related to the work in the first week after the event. They are not allowed to have a firm sign the insurance policy on their behalf, so are not allowed to reduce their liability in this manner. Figure 3 shows that lottery winners do not start firms at the time of the event, providing evidence that there is no incentive to start businesses at this time. Instead, lottery winners begin to start new firms two years after the lottery contracts were awarded, which corresponds to the time when recipients begin to finish their contracts.

In Table A6, we examine the likelihood that the new firms of lottery winners are created to win more government contracts in the future. In column (1), we show that lottery winners start more firms. In columns (2-5), we analyze whether these firms have won more government contracts, aggregating firm outcomes to the owner level. We show that despite having many more firms, lottery winners are not more likely to own a firm that has ever won a randomly assigned government contract nor non-randomly assigned government contract, and have no difference in their cumulative value of government contracts. Additionally, the control group means show that very few of these firms have ever won any government contracts, indicating more broadly that this does not appear to be an important reason behind firm creation here. Taken together, it does not appear that lottery winner firms are created to work further with the government along these major dimensions.

Increasing business connections: Contracts in this context are allocated by randomly assigning applicants to a set of jobs. These jobs are independent, but may allow entrants to interact, see each other's quality, and develop future networks of firm co-ownership. Firm co-ownership is common, as is true of formal firm ownership around the world.[15] We show in two ways that it is unlikely that the business creation effects of this program are driven by introducing a set of new potential firm co-owners.

The first piece of evidence against this channel is by looking at any firm ownership compared to majority share firm ownership. If co-ownership is an important factor, we would expect individuals to be taking small non-majority ownership shares in firms. Table A5 shows similar effects of lottery winning on ownership of any firm (7.8 percentage points) and majority firm ownership (5.1 percentage points).

We also evaluate this potential mechanism directly by studying whether lottery winners in the same block are more likely to own firms jointly. We adopt a similar strategy as other papers that have looked at endogenous future group work outcomes as a function of schooling or place of residence (Zimmerman, 2019; Bayer et al., 2008).

We use a differences-in-differences approach, exploiting differences across individuals in the same event-block, whether they are lottery winners, and the combination of these variables. The data is arranged at the entrant by entrant pair level. Individuals are considered linked if they are co-owners of the same firm, as derived from the firm ownership dataset. We include either full entrant block by entrant block fixed effects or separate fixed effects for the block of the application of each entrant to control for differences in both applicant group differences and application group interaction differences. We cluster standard errors at the entrant block by entrant block level to allow arbitrary correlation within groups.

Across three specifications, Table A3 shows that winners from the same lottery group have positive but insignificant differences in their firm co-ownership likelihood. The null effects suggest that firm co-ownership is not much more common among the lottery winner group than other potential firm co-owners and is not the driver of the firm creation results.

Heterogeneous impacts and age as a proxy for experience: If an important channel for entrepreneurship is managerial experience, we would expect the effects of this experience to be larger for the young. We investigate the effects of the program by age of the individual at the time of the contract in Table 4. We use age as the major bifurcation in someone's career due to the high correlation between age and experience in general and in our sample. We bin the age of the individual at the time of the lottery events into three age bins (20-34, 35-49, 50-64), which correspond closely to terciles of the sample. All outcomes correspond to the year 2018 to mark the latest post-period year for which we have data.

The effects of the contracts are highly heterogeneous across age bins. The youngest individuals (20-34) have the highest declines (19 percentage points) in formal wage employment due to lottery winning. Middle career individuals leave wage employment on average by 11 percentage points and the oldest leave by 6.6 percentage points. We can clearly reject that the effects are constant across the age bins. Analogously, we find the largest increases in firm ownership for individuals who are young. We measure the average effects of contract receipt on the number of firms created as 0.13 for the young, 0.04 for the middle, and an insignificant

[15] The average firm ownership share in the data is 41%.

0.025 for the oldest group. When aggregating characteristics of firms up to the entrant level, the young own firms that account for high levels of revenues and employees. The effect on firm profits is positive but not statistically significant. The results for the other age groups are insignificant and smaller in magnitude.

Table A2 expands the set of possible other heterogeneous impacts, and finds that age appears to be the most important. In this table, we report heterogeneous effects of the program one-at-a-time and jointly for age, sex, previous income, past formal employment, and whether the individual is from the largest city. All variables besides age are insignificant both separately and jointly, in contrast to the strong effects by age in both specifications. Females have insignificantly positive heterogeneous estimates in both specifications. The lack of a negative effect for females contrasts other influential studies that shows females have low returns to interventions such as credit (De Mel et al., 2008). Heterogeneous impacts by past income are economically small when viewed as a separate interaction and positive in the joint estimation. This is inconsistent with the program inducing firm creation for a set of individuals with credit constraints that arise from low past incomes. Past formal employment is negatively-signed in both specifications, but the interpretation of the coefficient is difficult because many people were previously working but not in a formal wage job.

Financial capital and managerial experience: Managerial jobs will always bundle potential income effects relevant to starting a new business. It is thus difficult to fully rule out effects of capital, nor would we fully want to. That said, in this section, we try to assess which is the most important driver. In the previous section, we showed that the effects of the program do not show large heterogeneity as a function of past income, providing one piece of evidence against financial capital.

To provide additional evidence on whether capital is a driving factor in firm creation, we exploit heterogeneity in contract size. Since individuals do not apply for a single contract but rather a set of possible contracts, there is built in heterogeneity in contract size that is randomly assigned to winners.[16] A larger contract should provide more capital to a potential entrepreneur. This is not a clean test, as managing a larger contract may provide more experience as well. That said, the difference in experience gained may not be large, as the projects are relatively homogeneous in terms of the steps necessary for completion. Each contract has a large fixed component of finding workers, materials, subcontractors, and budgeting, with a variable component that is the daily management and final execution.

Table 5 presents heterogeneous effects of contract size as measured in $200,000 increments. We evaluate the effect of contract size on entrepreneurship outcomes including becoming an owner, firm income and firm size, on financial capital measured by net income, and on management experience measured by days spent in the contract. In column (1), we show that the contracts lead to large increases in the likelihood of becoming a firm owner, but contract size has no effect on becoming an owner. The point estimate is small and opposite-signed than what we would expect if firm creation were driven by increases in

[16]See Table A1 for supporting evidence.

capital. Columns (2-3) also show that contract size has little effect on aggregated firm income or firm size. In column (4), we show that contract size greatly increases net income received by the recipients. These patterns show that greater potential financial capital does not translate into measurable entrepreneurial outcomes, but does not rule out that small amounts of capital may have beneficial impacts. In column (5), we show that contract size is not strongly correlated to days spent in the contract. This provides some evidence in favor of the possible benefits of managerial experience in a contract for firm creation outcomes. We add the caveat that not all measures of managerial experience would be invariant to contract size because many of the more daily management tasks would be larger with a larger contract size. There are also numerous mechanisms that are part of the bundle of benefits of a managerial job, such as positive updating about self, that we cannot directly test.

We also show that effects of financial capital on becoming a firm owner do not appear to be non-linear in Figure A6. We split the effects of contract size from the regression above across quintiles of contract size. The pattern is somewhat noisy and not indicative of a clear pattern, with some small contract sizes and some medium-sized contract sizes having large effects on firm creation. We cannot reject that the effects are homogeneous across the quintiles ($p = 0.59$).

To provide greater evidence on these channels of program impacts, we show that individuals have had new managerial experiences as a result of contract receipt and have positive but insignificant amounts of loans accessed in the future. We hypothesized that experience with a government contract would increase the maximal number of temporary employees ever worked with, and the maximal number of subcontractors ever worked with. Table A4 shows results consistent with these hypotheses. We find lottery winning leads individuals to have managed more temporary employees and subcontractors. These measures are taken over the course of their entire career, indicating that they may have had these experiences on the randomly assigned lottery projects, or otherwise. We do not have evidence that these new managerial experiences lead to differential management practices, due in large part to the difficulty of measuring management practices for those who are not firm owners. To provide additional evidence on the importance of firm financing, we look at survey evidence of the effect of lottery winning on individual's number of loans and loan amounts. Both the number of loans and loan amounts are positive but statistically insignificant. The estimates are likely underpowered to detect effects due to the small survey sample. Any effect of contract access on loan amounts is unclear whether it's related to either greater financial capital or changes to firm production decisions as a function of other changes from managerial decisions.

4.2.4 New firms and quality

The welfare implications of new firm creation are dependent on the quality of the firms themselves. Firm creation may be entrepreneurial and lead to thriving businesses, or, at the other extreme, be undesired and act as disguised unemployment. We assess these two possibilities in this section, but first discuss firm creation types.

The newly created firms in this sample take numerous forms, not exclusively in construction. Table A5 shows that while lottery winners are much more likely to become owners of construction firms, they are also more likely to become owners of firms in commerce and other sectors. Thus, this contract experience potentially lets them expand into other sectors where their skills are useful as well.

Table 6 examines firm quality. The comparisons between firm characteristics are at the firm level, making this a primarily descriptive rather than causal exercise due to the selection effects that are partly the focus of this section. The comparison of lottery winner firms to non-winner firms studies two potential effects. First, new firms may be created as a function of contract experience. Second, firms that would have existed even without contract experience may be different. These forces may be in opposition in the case of negative selection but positive in the case of within-firm changes, or in the same direction if this experiment induces higher quality individuals to become entrepreneurs. We assess these effects jointly.

To make this firm comparison, we restrict our analysis to firms created by entrants in the same event-block and same year, to control for basic selection and time confounders. We follow the main measures of newly created firm quality used in Hombert et al. (2017), namely any firm hiring and firm survival in the first two years of the firm's existence. The rationale for these measures is that early firm actions, such as hiring, are predictive of firm hiring in the future. In the firms created by lottery non-winners, 27% have hired within the first two years and 72% have survived.[17] Firms created by lottery winners are 4.8 percentage points or 18% more likely to have hired ($p < 0.10$) and are 5.1 percentage points or 7% more likely to have survived ($p < 0.05$) in their first two years. Revenues of firms created by lottery winners are insignificantly different from firms created by non-winners. Overall, firms created by lottery winners show positive signs of early firm development, even with the potential for negative selection to arise from creating new entrepreneurs.

4.2.5 Robustness to potential concerns

Bias in official statistics: This paper relies heavily on administrative records collected by the DGII. If winners have become more formalized or feel a need to report business records more thoroughly than non-lottery winners, the measured results may be spurious. One piece of evidence against informal firms formalizing is that individuals exit wage employment and move into firm creation at similar rates. To provide further evidence against these possibilities, we collect a second dataset on firm ownership records from ONAPI. They maintain records of registered firms that trademark their names. These records are not part of the Dominican tax system and reflect greater formalization in the economy that is unrelated to taxation.

Table A7 shows that our main result, that lottery winners are more likely to start firms, is confirmed in this dataset as well. Lottery winners are more likely to ever have started a firm, have more firms and have a higher number of firm shares. In these specifications, firm shares are defined only based on the share of the year that the individual is an owner rather

[17] Average firm size is 2 in the firm's second year.

than their ownership share because that information is unavailable here. The point estimates and control group mean in this dataset are lower than in the main dataset because we are only able to match part of the sample (68%), and not all firms register in these records.

5 Evaluating selection into the program

The treatment effects previously analyzed are conditional on the sample of those that applied. This framework leaves key questions unanswered. First, do potential entrepreneurs self-select into this program based on their relative likelihood to start businesses? Second, are the strong age-based effects previously seen indicative of differences in underlying entrepreneurial potential in the population or a function of differences in selection into the lotteries by age group?

Reduced-form evidence motivates these questions. Table A8 examines the effect of lottery winning based on the number of times that an individual has previously entered a lottery event. Individuals who have entered fewer events appear to be less likely to become a firm owner after winning a lottery. This is indicative of positive selection into the program based on gains, but the reduced-form nature makes it difficult to evaluate this proposition. Regarding the age-based results, Figure 2b shows that older entrants are negatively selected on past income as compared to non-entrants, but younger individuals are not. This suggests that the age-based results may differ across age groups at least in part given differential selection. Evaluating this is important for understanding who to target with entrepreneurship-based interventions.

To answer these questions in an organized framework, we analyze individuals' participation choices into lotteries to recover unobserved participation preferences in the population. We then document how the treatment effects vary based on the observed and unobserved heterogeneity in the decision to participate, and show treatment effects for the population.

5.1 Setup

We model lottery event application choices in order to characterize selection into this government program. Potential applicants i include individuals who have participated and also non-participants from the set of licensed civil engineers and architects, who could have participated. These individuals may apply to the various events $e \in \{1, \ldots, E\}$. Within each event, applicants are required to make a mutually-exclusive choice over the pre-grouped sets of contracts called "blocks", where B_e identifies the number of blocks in event e inclusive of the outside option. We denote blocks $b \in \{0, \ldots, B_e\}$, with $b = 0$ as the outside option of not participating. The number of blocks varies from one to thirty-two. For applicants to a certain block b in event e, the available contracts are randomly provided a government contract with probability π_{eb}. We denote application choices as $A_i = (A_{i1}, \ldots, A_{iE})$. A subsequent choice of contract acceptance is not modeled because almost all applicants accept. The final outcome—specifically whether an individual starts a firm—is Y_i.

5.2 Preferences

The utility of receiving a contract in event and block *eb* takes the form

$$U_{ieb} = X_i\beta + \delta W_{ieb} + \theta_i + \nu_{ieb} \tag{3}$$

where X_i is a vector of time-invariant or predetermined observable characteristics of individual i, W_{ieb} is a vector of characteristics of the event-block that may also interact with the individual (e.g. distance from i to contracts in eb or the average contract size), and θ_i is time-invariant unobserved heterogeneity in individual i's preference for government contracts. The utility of not applying for a government contract in a specific event e is normalized to be mean 0

$$U_{ie0} = \nu_{ie0} \tag{4}$$

As written, unobserved preferences, (θ_i, ν_{ieb}), are assumed to be additively separable and independent from covariates. Individual-specific heterogeneity in preferences, θ_i, is modeled to follow a normal distribution with mean μ_θ and variance σ^2_θ. Application-specific preference shocks, ν_{ieb}, are assumed to be iid extreme-value distribution across alternatives, events and people. This error structure provides the scale-normalization of the model. The inclusion of θ_i relaxes the assumption of uncorrelated preference shocks between contract lotteries within an event and across events as opposed to the outside option of not applying.

5.3 Choices

Applicants make choices in each subsequent lottery event e based on their preferences. All applicants are assumed to pay a fixed cost, C, that includes the the non-monetary costs of learning about the events and offered contracts, preparing and submitting relevant documentation, and attending lotteries. Contracts are awarded with probability π_{eb}, which are considered known and operationalized by including the past lottery probabilities.[18] We model each period as separate choices that are independent conditonal on covariates. The choice facing potential applicants to event e is

$$A_{ie} = \arg\max_b \pi_{eb} U_{ieb} - C\,\mathbb{1}[b \neq 0] \tag{5}$$

5.4 Choice estimation

The unobserved heterogeneity in preferences implies this is a form of mixed logit. The formulation of the model with a single time-invariant random coefficient for the choices of applying to lotteries is analogous to a nested logit with one nest for all contract lottery choices.

[18]The outside option is assumed to have probability 1.

We assume conditional independence of choices after conditioning on X_i, W_{ieb}, and θ_i, which rules out forward-looking behavior.[19]

The likelihood of an individual making a series of application choices $a = \{a_1, a_2, \ldots, a_E\}$ based on observable characteristics is

$$L(A_i|X_i, W_i) = \int \prod_{e=1}^{E} \left[\frac{\exp(A_{iea_e})}{1 + \sum_{j=1}^{B_e} \exp(A_{iej})} \right] dF(\theta) \tag{6}$$

$$= \int q(a|X_i, W_i, \theta) dF(\theta) \tag{7}$$

Since there is no closed-form solution for this integral, we estimate the model by Maximum Simulated Likelihood (MSL). The MSL estimator of $\Lambda = (\beta, \delta, \mu_\theta, \sigma^2_\theta)$ is

$$\Lambda^{MSL} = \arg\max_{\Lambda} \sum_i \ln \left(\frac{1}{R} \sum_{r=1}^{R} q(a|X_i, W_i, \theta_i^r) \right) \tag{8}$$

where R is the number of simulations. We set R = 200.

The above procedure estimates coefficients on observable parameters and coefficients of the distribution of θ_i, namely μ_θ and σ^2_θ. To recover θ_i, we estimate posterior means of θ by simulation that are conditional on an individual's sequence of choices and the relevant choice characteristics[20]

$$\theta_i^* = E[\theta|A_i, X_i, W_i] \tag{9}$$

5.5 Outcomes

From the model of participation choice, we recover both observable and unobservable factors affecting entry. We include these parameters into an outcome equation characteristic of generalized Roy models. The estimating equation takes the form:

$$Y_i = \mu_0 + \mu_0^X X_i + \mu_0^\theta \theta_i^* + \mathbb{1}[\text{Winner}][(\mu_1 - \mu_0) + (\mu_1^X - \mu_0^X)X_i + (\mu_1^\theta - \mu_0^\theta)\theta_i^*] + \varepsilon_i \tag{10}$$

The outcome, Y_i, is whether an individual becomes a firm owner between 2012 and 2018, the full study period. Parameters μ_0 and $(\mu_1 - \mu_0)$ measure the average effect and the population average differential effect of being a lottery winner since X_i and θ_i^* are demeaned. Differential effects of becoming a firm owner as a function of observable characteristic are measured by μ_0^X, and as a function of unobservable characteristics, reflecting unobserved absolute advantage in firm ownership, is measured by μ_0^θ. Differential effects across observable characteristics are measured by $(\mu_1^X - \mu_0^X)$. Unobserved comparative advantage in

[19]Forward-looking behavior in this context is unlikely because of the low probability of winning any one event, and because the existence or characteristics of future events are not known.

[20]While θ is independent of (W, X), they are not conditionally independent given A.

lottery participation is measured by $(\mu_1^\theta - \mu_0^\theta)$. We account for sampling variation in unobservables through a bootstrapping procedure that re-estimates this model using draws of θ_i^* from the distribution of parameters in the choice model.

This model provides a means of estimating various population-level parameters. The ATE is measured directly from Equation 10. The defined parametric model provides counterfactual outcomes across the population. To estimate other population-level parameters, we simulate population-level observable characteristics by resampling and unobservable characteristics from the estimated distribution of θ. Using the distribution of preferences, we estimate the treatment-on-the-treated effect (TOT) and the treatment-on-the-nontreated effect (TNT) by applying marginal treatment weights across the distribution of preferences and implied propensity for treatment from the experiment (Heckman and Vytlacil, 2005).

The model is identified by using instruments that affect participation in the lotteries and are otherwise unrelated to outcomes. In this setting, we exclude the varying event-specific characteristics of the lottery alternatives. As discussed in Walters (2018) and based on the results of Heckman and Navarro (2007), models combining lottery instruments with secondary instruments affecting inclusion can be semi-parametrically identified, although we use a parametric model for estimation. The lottery randomization provide estimates of mean outcomes of non-winners across the distribution of preferences without requiring that a single instrument provide full support in treatment status.

5.6 Selection model results

We begin by showing the results of the choice model. We then document that individuals with higher preferences for participation are most likely to become entrepreneurs. Finally, we show that the reduced-form finding that the youngest individuals benefit the most from contractual experience is not a result of differential selection by age.

Table A9 provides the results from the estimation of participation choices. Covariates pertaining to the choice alternatives enter with the expected sign. The share of contracts relative to the local population and mean contract size, both demeaned at the province by time level, induce greater entry. The distance to the expected worksites discourage entry. All individual characteristics are either time-invariant or are taken from year 2011, the year prior to the beginning of the lotteries. The partial correlation between employment or previous firm ownership and entry are negative. Tax filers, a likely measure of past formality, and age increase entry. We also include event dummies that show learning about this procurement scheme, as these lottery events began as a largely unknown phenomena to potential entrants.

Unobserved participation preference heterogeneity in the population, the standard deviation of θ, is large. The standard deviation of observable individual characteristics affecting participation is only 0.5, as compared to 2.8 for unobservable characteristics. This is even after including detailed observable characteristics likely to affect labor market choices, such as demographic information, important labor market measures, and past firm ownership. On average, individuals prefer not to enter the lotteries, as indicated by the negative mean unobserved tastes, but the wide variation in underlying preferences in the population induces many to enter.

To see how individual's observable and unobservable characteristics affect firm creation with and without treatment, we examine Table 7. The two columns in this table are from the same regression, but are split across columns. In column (1), we see the effect of observable and unobservable covariates on becoming a firm owner for non-winners. Observables explain large amounts of heterogeneity in firm creation. Young individuals are more likely to start firms on average. Females are less likely. Individuals who were employed in 2011 and who had higher incomes are more likely to have started a firm. Past firm ownership is also correlated to new firm ownership. Individuals also select into the program based on unobserved absolute advantage in becoming a firm owner. That is, individuals with higher preferences for participation also have a higher likelihood of becoming a firm owner if they are non-winners.

In column (2) of Table 7, we investigate the differential effect of being a lottery winner on becoming a firm owner. Being a lottery winner insignificantly increases the likelihood of becoming a firm owner by 6 percentage points. The effect is highly heterogeneous by age. While females were much less likely to start firms without this program, females are not less likely to start firms as a function of contractual experience. There is also no differential effect as a function of past income, providing another indication that income may not be the most important channel of new firm creation. Instead, individuals with higher unobserved preferences for participation also are more likely to become firm owners after lottery winning, characteristic of selecting into the program based on unobserved comparative advantage (Roy, 1951). That means that higher preference individuals are more likely to start a firm as a result of this program. Combining both results on unobservables, we see that those who participate in the lotteries are more like to start a firm, even absent winning, but also more likely to benefit from the contract through new firm creation.

Using the model predictions, we show additional population-level parameters in Table 8. Column (1) shows the lottery-based, reduced-form estimated treatment effects by including event-block fixed effects. Column (2) shows the model-based simulated predictions. We focus first on Panel A showing the full sample results. The estimated treatment-on-the-treated (TOT) coefficients are large, indicating a 10 percentage point increase. The population average treatment effect and treatment-on-the-not-treated effects are both much lower, closer to 6 percentage points. This is indicative of selection on the relative likelihood to start businesses from the population, and shows that the program structure targets along the dimension of future firm creation. The close relationship between the average treatment effect and the treatment-on-the-not-treated parameters is reflective of few contracts being awarded as compared to the size of the population.

The TOT estimate are larger than the estimate from the reduced-form design, at 0.08, but the latter effect is a result of inverse-variance weighting across the lottery event-blocks. The weights are a function of both the number of entrants and the share of entrants who win a lottery in each of these separate contract allocation groups. Hence, the reduced-form design doesn't correspond to a treatment effect for any particular population. We use the simulated model to predict the reduced-form lottery effect and find that the model prediction, 0.084, is similar to the reduced-form in the data.

We previously saw in Table 4 that the youngest individuals in the sample are much more likely to become firm owners than those who are older. One concern is that there may be differential selection by age group. The youngest may, for example, exhibit a selection on gains pattern, whereas those who are older may have the opposite pattern since the more entrepreneurial individuals may already have successful businesses. We evaluate this possibility in Panels B-D in Table 8. We find that the youngest are unambiguously most likely to use this opportunity to become firm owners. Both young individuals and individuals who are mid-career select in on gains, but the ATE in the population of young individuals is 0.10 as compared to 0.00 for mid-career individuals. Thus, the young are much more likely on average to start a firm as a result of lottery winning. The oldest individuals in the sample are consistently unlikely to become entrepreneurs. Taken together, the results are unambiguous that the young benefit the most from managerial experience.

Overall, individuals select into the program based on future gains to firm creation and the age-based results are not the result of differential selection. The current program design of allocating contracts across a series of lottery events does already achieve selection by requiring individuals to incur participation costs across events. Given the positive selection in based on gains, the model estimates suggests that reducing the number of contracts allocated in the same event or increasing application costs would further select more individuals who will use this contractual experience to start new firms.

6 Local labor market impacts on non-winners

In this section, we study the effects of government hiring on local labor markets. Large-scale, nationwide hiring by the government may affect non-contract recipients through a number of channels. Simple models of public sector employment suggest that employment for non-recipients will fall due to increases in wages (Cahuc et al., 2014). In these models, the net effects of public hiring on job creation are ambiguous because increased wages decrease the number of vacancies, and can even cause aggregate decreases in jobs. There are reasons to be skeptical of the applicability of these models in this context. Many of these models are contingent on hiring out of unemployment. That is not true for this government program, and for other prominent government labor market interventions as well (Crépon et al., 2013).[21] Increased vacancies due to the government poaching workers may leave vacancies for non-winners. Additionally, government hiring in monopsonistic models of the labor market in which firms face upward sloping labor supply curves may increase wages and employment for all individuals. Finally, there could be greater employment effects through firm creation or other big-push type mechanisms as well.

[21] 44.9% of their sample was employed at the time the intervention started.

6.1 Empirical strategy

We model the effect of the share of individuals hired by the government on non-winners in each province through the following:

$$y_{ipt} = \beta(\text{share employed})_{pt} + \phi_i + \delta_t + \varepsilon_{ipt} \tag{11}$$

for individual i who is from province p in time t. Provinces of individuals are assigned based on their province of tax registration. Our outcome of interest, y_{ipt}, includes income and employment. This panel fixed effects estimator controls for time-invariant characteristics of the individuals such as their fixed ability or past education. We include macro-region by time fixed effects to control flexibly for aggregate time trends and show robustness by including linear province-specific time trends in a separate specification. There are three macro-regions in the country. Our regressor of interest, $(\text{share employed})_{pt}$, measures the number of individuals from a province p who received a contract by time t over the number of licensed engineers and architects from these provinces.[22]

The identifying assumption is that non-winners who are in areas with high relative shares of lottery winners would covary in the same way as those from areas with low relative shares of government contracts. A visual representation of the variation for our regressor of interest can be seen in Figure A4. By 2015, some provinces have 16% of engineers who received a contract whereas others had only 4%. This estimator is most akin to Lalive et al. (2015), who also study the market externalities of another government program, in their case unemployment insurance, on non-recipients using a panel fixed effects design.

6.2 Results

We examine the effect of the program on non-winner employment and income by labor market in Table 9. In all regressions, we control for time-invariant individual characteristics, age, and time by region temporal variation. In columns (1-2), we see that an additional one percentage point of individuals in the labor market who received contracts increased employment by 0.6 percentage points. This is a relatively large effect on non-winners and is robust to including province-specific linear time trends. Since average treatment levels in the country are 8 percentage points, it suggests that on average non-winners received an additional 4.8 percentage points of jobs due to the program. The point estimate is still considerably lower than the estimated treatment effect of 12 percentage points that does not account for any spillover effects.

We see null results on income gains. In columns (3-4), an additional one percent of treated individuals in a province has no statistically significant effect on incomes. Again, interpreting coefficients literally, these estimates are still much lower than the unadjusted treatment effects seen in our analysis of direct impacts.

[22] This population of engineers in the denominator is calculated from the exequatur records. We were able to match and consequently find the province for only 92% of observations. Thus, we may be slightly overstating the share of licensed individuals who received a contract.

The generally *positive* employment effects on non-winners is the opposite of other estimates of the effect of government labor market assistance programs (e.g., Crépon et al. (2013)). One possible reason is that non-winners may have filled job vacancies that opened as a result of winners leaving their past employment. Other explanations include monopsonistic models of the labor market or new firm creation. A final possibility is that non-winners work directly for lottery winners on the contracts. This is possible, but we think it is unlikely to be driving the results. Lottery winners would be unlikely to pay people who work for them through formal employment contracts; much more common are temporary work contracts that would be accounted through VAT transactions. Thus, this would likely not contribute to the estimated effects.

7 Conclusion

Substantial theoretical and empirical literature has focused on the importance of managerial experience to generate firm development in developing countries. In this paper, we provide new evidence on the opportunity to work in managerial roles. Through a fully randomized government program, we show that lottery winners who are given this opportunity shift out of formal wage employment and start new firms. We interpret these effects as most consistent with the bundle of benefits from managerial job experience, rather than increases in government contract access, increases in business networks, or increases in financial capital. The effects are driven primarily by the young, who are the most likely to benefit from new labor market experience. We then build a selection model to assess program targeting. We find that individuals self-select into the program based on future gains to firm creation. As individuals with higher marginal benefits are willing to incur higher costs to participate, increasing costs of participation will select more individuals along this dimension. Finally, we show that greater allocation of contract shares can increase the employment of non-recipients.

The results suggest that incentivizing new labor market experience in the labor market, especially in managerial jobs, may lead to new firm creation, and also finds evidence in favor of the ability for costly admission procedures to target those with high potential for entrepreneurship. Each of these results are potentially important for developing country firm creation and labor markets. First, the results suggest a clear rationale why managerial job experience may be underprovided by firms—individuals may start their own competing firms. Public employment programs in developing countries are common, but are often focused on low-skill employment. These programs may alleviate temporary income shortfalls, but are less likely to provide skill accumulation that will be helpful for workers in the long-term. In terms of more advanced job experience, programs to incentivize the young to acquire more skills may be the most beneficial. Finally, in regards to targeting, recent programs to target high-potential entrepreneurs, such as business plan competitions, have seen beneficial results, potentially in part because they induce self-selection from those with high marginal benefits. Greater evidence on mechanisms to induce self-selection in intervention targeting seems potentially fruitful.

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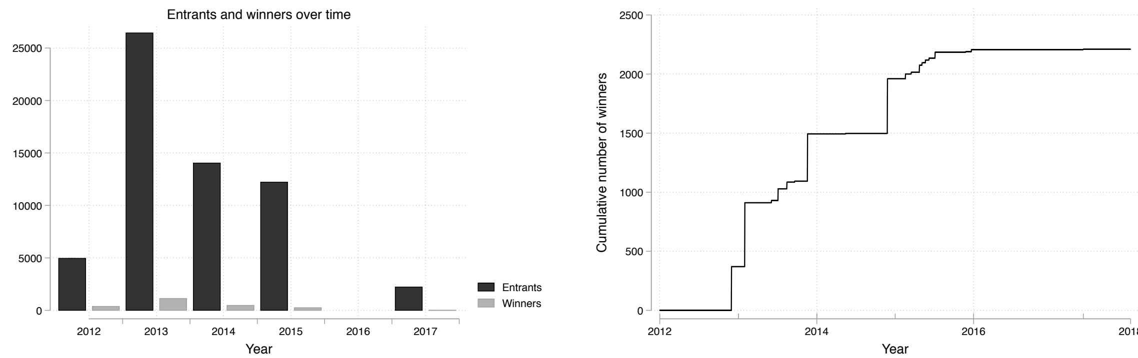
8 Figures

Figure 1: Lotteries over time

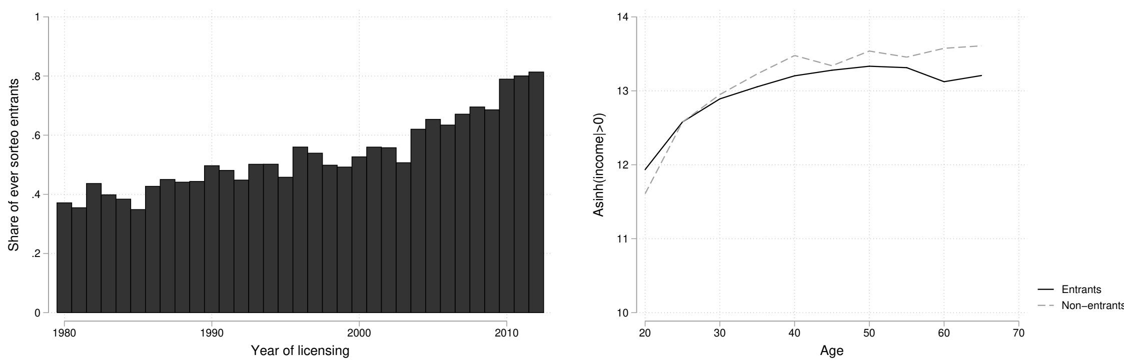
(a) Entrants and winners **(b)** Number of winners



The figures show the lotteries over time. Panel (a) shows the number of entrants and winners by year of lottery event. Panel (b) shows the cumulative number of winners by date of lottery event.

Figure 2: Comparison of entrants and non-entrants

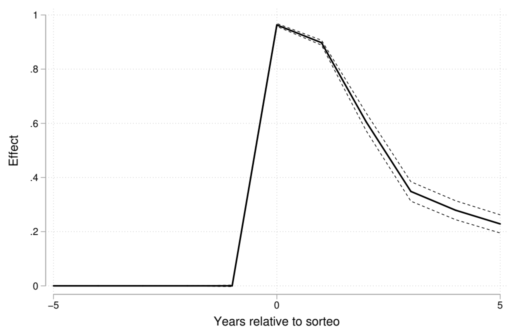
(a) Share of entrants by licensing year **(b)** Asinh(income) by age



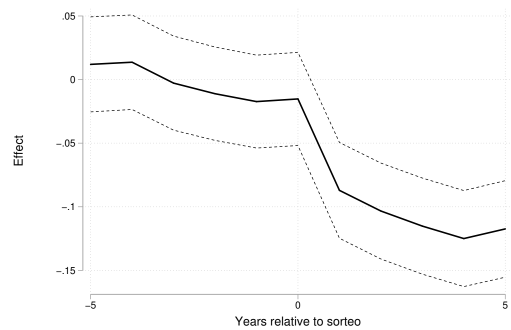
The figures show patterns of lottery entry by age. Panel (a) shows the share of individuals ever entering a lottery by year of civil engineering or architectural licensing. The full sample is taken from Exequatur records, the registry of all licensed engineers and architects in the country. Panel (b) shows Asinh(income) for ever entrants and non-entrants taken from the same registry. Income records are from the years of 2012 and earlier, which are prior to any lottery payments. The sample is further restricted to taxpayer statements with positive reported earnings.

Figure 3: Sector participation for individuals

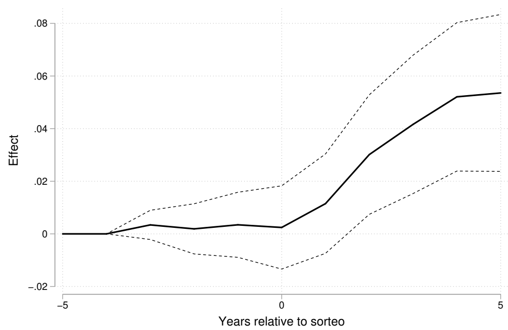
(a) Open contract



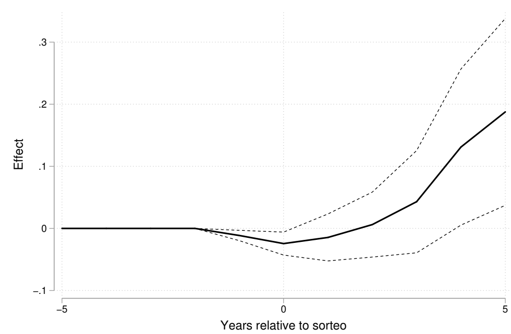
(b) Wage employment



(c) Number of firms owned, scaled by share



(d) Owned firm size, scaled by share



The figures display regressions of the outcome in the panel header on lottery winning. The sample includes individual entrants. Regressions are estimated separately for each year relative to the lottery event date and include event-block fixed effects. Dotted lines represent 95% confidence intervals for standard errors clustered at the individual level.

9 Tables

Table 1: Placebo tests for winning allocation

	All sorteos			Analysis sample		
	N	Var. Mean	Winner	N	Var. Mean	Winner
Number of sorteos, t=-1	59864	2	-.0052	19469	.8	.044
			(.055)			(.11)
Firm	59864	.14	.0047	19469	.13	-.0073
			(.0075)			(.011)
Female	59340	.29	.000048	19177	.27	-.012
			(.0096)			(.014)
Filed taxes, t=-1	59864	.82	.0089	19469	.82	.013
			(.0083)			(.013)
Gross income, t=-1	59864	2,132,715	109,122	19469	2,052,089	-317,242
			(222,953)			(343,721)
Asinh Gross income, t=-1	59864	10	.025	19469	10	.026
			(.13)			(.2)
Employees, t=-1	59864	.84	-.21	19469	.93	-.45
			(.17)			(.31)
Ever won non-sorteo, t=-1	59864	.028	-.003	19469	.016	-.006
			(.0036)			(.0041)
Ever firm owner, t=-1	59864	.13	.009	19469	.097	.0074
			(.0074)			(.0098)
Joint p-value			.24			.42
Joint p-value, Individuals			.86			.66

Columns (1-3) correspond to all lottery events. Columns (4-6) correspond to the analysis sample. Columns (1, 4) report the sample size, columns (2, 5) report the sample mean, and columns (3, 6) reports the point estimate of an OLS regression of the entrant characteristic on lottery winning. Joint p-value comes from an F-test of joint significance of the variables in the rows on lottery winning. Controls include lottery event-block fixed effects. Heteroskedasticity robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Individual firm ownership and aggregated firm outcomes

	Any firm	Num firms		Income	Profits	Employees
	(1)	(2)	(3)	(4)	(5)	(6)
	Ever	Ever	2018	2018	2018	2018
Winner	0.078***	0.15***	0.060***	4187.4**	-55.7	0.17**
	(0.017)	(0.032)	(0.014)	(1822.1)	(92.7)	(0.076)
Control Mean	.35	.49	.18	7224	152	.39
N	16855	16855	16855	16855	16855	16855

This table reports estimates of the effect of lottery winning on firm ownership and owned firm outcomes. Sample is restricted to individuals. Columns (1-2) report becoming a firm owner at some point after 2010. Columns (3-6) report aggregated firm results at the individual level for year 2018 and are scaled by participation and ownership period shares. Controls include lottery event-block fixed effects. Standard errors clustered at the individual level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Effects on individual's income and net income

	(1) Total inc.	(2) Net inc.
Winner * Year 0	97388.0***	10050.6***
	(4537.5)	(1246.3)
Winner * Year 1	243343.0***	16717.7***
	(7773.6)	(2370.5)
Winner * Year 2	194231.3***	9329.3***
	(8251.0)	(2095.2)
Winner * Year 3	74259.6***	2915.8**
	(4570.4)	(1141.9)
Winner * Year 4	39240.2***	4255.6***
	(3282.9)	(1220.6)
Winner * Year 5	21327.4***	-735.8
	(2642.1)	(911.3)
Control Mean	22288	11410
N	100164	100164

This table analyzes the effect of lottery winning on individual entrant income and net income. Regressions on each period are estimated jointly. Controls include lottery event-block by period fixed effects. Standard errors are clustered at the individual level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Employment and firm ownership by age

	Employment	Firm ownership			
	(1)	(2)	(3)	(4)	(5)
	Employed	Num firms	Income	Profits	Employees
Winner, 20-34	-0.19***	0.13***	8050.5**	130.0	0.46**
	(0.032)	(0.028)	(3912.1)	(204.2)	(0.20)
Winner, 35-49	-0.11***	0.043*	3328.7	-112.1	0.12
	(0.030)	(0.024)	(2861.8)	(155.1)	(0.11)
Winner, 50-64	-0.066**	0.025	3146.5	-174.3	0.020
	(0.032)	(0.022)	(2949.1)	(150.6)	(0.084)
Joint p-value	0.02	0.02	0.55	0.47	0.12
Control Mean	.58	.18	7224	152	.39
N	15869	15869	15869	15869	15869

This table shows estimates of the effect of lottery winning on employment and firm ownership by age. Sample is restricted to individuals. All outcomes are from 2018. Column (1) registers formal employment. Columns (2-5) report aggregated firm results at the individual level for year 2018 and are scaled by participation and ownership period shares. Joint p-value is from an F-test on joint equality of treatment variables. Controls include lottery event-block by age bin fixed effects. Standard errors clustered at the individual level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Effects of contract size on entrepreneurship, income, and management

	Entrepreneurship (2018)			Income (Cum.)	Management
	(1)	(2)	(3)	(4)	(5)
	Owner	Firm income	Firm size	Indiv. net	Days in contract
Winner	0.099***	7189.8**	0.38***	-1932.9	1068.4***
	(0.038)	(3414.3)	(0.14)	(8073.9)	(47.6)
Winner * Size ($200k)	-0.0065	-930.4	-0.067**	11002.6***	5.14
	(0.010)	(830.3)	(0.030)	(2672.1)	(12.6)
Control Mean	0.35	7223.98	0.39	65368.54	0.00
N	16855	16855	16855	16855	16855

Sample is individuals. The treatment variables are winner and winner interacted with contract size. We assign contract size as 0 for control individuals and do not control for contract size. Controls include lottery event-block fixed effects. Standard errors clustered at the individual level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Created firm quality

	Hire		Survival		Revenue	
	(1)	(2)	(3)	(4)	(5)	(6)
Winner	0.048*	0.052*	0.051**	0.052**	-3580.3	-4473.0
	(0.028)	(0.028)	(0.022)	(0.022)	(4590.3)	(4566.3)
Firm type FE		X		X		X
Age tercile FE		X		X		X
Control Mean	.27	.27	.72	.72	44913	44913
N	4647	4591	4647	4591	4647	4591

This table compares firms created by lottery winners and those created by non-winners. The sample includes created firms after the date of the sorteo from participants in the analysis sample. Hire is a dummy equal to 1 if the firm has hired any employees within the first two years. Survival is a dummy equal to 1 if the firm continues to exist after 2 years as measured by filing tax records. Revenues come from the second year of firm existence. Controls include lottery event-block and year fixed effects. Standard errors two-way clustered at the firm and individual level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Selection model estimates of program effects

	Became owner	
	(1) Non-winners	(2) Winner effect
Constant	.29***	.06
	(.004)	(.041)
Age	-.0073***	-.022***
	(.0018)	(.0078)
Age Sq.	.000031	.00016*
	(.00002)	(.000084)
Female	-.078***	.05
	(.0097)	(.044)
Employed	.055***	-.053
	(.0071)	(.035)
Income (10K USD)	.0054***	-.00032
	(.00097)	(.0036)
Prev. owner	.16***	-.075
	(.013)	(.064)
θ^*	.0086***	.024*
	(.0017)	(.014)

Estimates come from a single regression with column (2) showing interaction terms between winner and covariates. All individual characteristics are taken from the year prior to the sorteos, 2011. Standard errors are bootstrapped to account for sampling variation in θ. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Effects on firm ownership:
reduced-form and structural model estimates

	Reduced-form	Str. Model
	(1)	(2)
	Panel A: Full sample	
LATE	0.080	0.084
TOT		0.100
ATE		0.062
TNT		0.060
	Panel B: Age < 35	
LATE	0.196	
TOT		0.236
ATE		0.101
TNT		0.098
	Panel C: 50 > Age > 34	
LATE	0.052	
TOT		0.084
ATE		-0.003
TNT		-0.004
	Panel D: Age > 49	
LATE	0.002	
TOT		-0.012
ATE		-0.041
TNT		-0.044

This table compares our lottery fixed effect reduced-form estimation with model-based estimation of treatment effects on being a firm owner after 2011. The panels are split for the full sample and by age groups. Column (1) conditions on sorteo-block fixed effects. Column (2) shows model-based effects of the treatment on the treated (TOT), the average treatment effect (ATE), and treatment on the nontreated (TNT).

Table 9: Effect of government hiring share on non-winners

	Employed		Income	
	(1)	(2)	(3)	(4)
% contracts	.0063***	.006**	3201	6055
	(.0022)	(.0026)	(8524)	(9789)
Sample Mean	.62	.62	610850	610850
N	155419	155419	124828	124828

This table analyzes the effect of government contract share on non-winner wage employment and income. The sample includes non-sorteo winner individuals. The regressor of interest is the share of government contracts in this province by province engineering and architecture population. The sample mean is taken from 2012 and later. Odd columns include age, individual, and macro-region by year FE. Even columns also include a linear trend by province. Standard errors are clustered at the province level and in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix

A1 Matching between datasets

Linking between all administrative datasets in this project is completed by using the two unique administrative identifiers that apply to this population: the RPE, an identifier assigned to all registered state providers, and the national ID. The dataset on state providers includes both the RPE number and the national ID. We have access to one of these two identifiers across almost all of the outcome datasets. The main work in matching the other datasets, most notably the datasets on lottery event participation, is to assign identifiers when they are omitted. Since 72.5% of participation records come with the identifiers, the matching is only pertinent to the other 27.5%.

When administrative identifiers are missing from a dataset or an observation, we attempt to assign one of the two identifiers using the entrant's full name. Luckily, full names of individuals and firms in the DR are often unique. In the universe of registered taxpayers in the country, 90.5% of individuals have unique names.[1] The reason for the high name uniqueness in the Dominican Republic is that full names usually contain at least four parts, including first and middle, plus the father's and mother's last name. We are further usually matching into the set of 70,000 state providers from the list of over three million registered taxpayers in the DR, thereby mitigating concerns about name uniqueness.

To assign the identifiers using name, we usually start by trying to match to the registry of state providers. Our approach is automated matching with human assistance. For all matches, we standardize names by converting special characters to ASCII format and removing honorifics if included. We then assign Jaro-Winkler string distances between all names within these datasets. Jaro-Winkler scores are commonly used in string matching procedures similar to this one (e.g. Feigenbaum (2016)).[2] The maximum value of a Jaro-Winkler score is 1. We use two criteria to define automatic matches. First, if two names have a Jaro-Winkler score of 0.99, and there is no other possible match with a Jaro-Winkler score within 0.03 units, we call this a match. Second, if the Jaro-Winkler score is above 0.95, and there is no other possible match within 0.07 units, we define this as a match. Each of these criteria is very restrictive; we use them to define matches when we have a high degree of confidence the match does not need to be reviewed. Finally, if there are no matches that fit these criteria, we output the five names with the highest Jaro-Winkler score to see if we can find a match. We then try to assign identifiers when obvious, or do a further set of matching using the full dataset of taxpayers, either manually or by a second application of the same method.

This parsimonious procedure uniquely assigns identifiers to almost all observations. Of the 59,861 observations in our event participation datasets, 43,428 come with both full name and either an RPE number or a national ID number. They did not require further matching.

[1] Name overlap is more common in the United States (e.g., in Florida, only 83.9% of individuals have unique full names).

[2] For more information on Jaro-Winkler distances, please see Winkler (1994).

Overall, we were not able to match 526 observations to a national ID number, and hence, have no tax information for these observations.[3] All observations that are matched uniquely or matched to multiple identifiers are kept for the analysis sample. Since a relatively small sample is matched non-uniquely, we randomly choose one of the identifiers for our analysis sample. This generates a small amount of potential measurement error that we could correct by using probabilistic matching and associated regression methods, but our results would change little.

We modify the matching procedure for a few datasets. We use information from exequatur records to identify the universe of licensed engineers and architects in the DR. Since we are specifically interested in individuals who may not be state providers, we follow the procedure above but match directly to the DGII taxpayer records to recover the national ID. Of the 22,879 individuals who are licensed as civil engineers and architects, we match 92% to a *Cédula*. For records on *sorteo* completion status where we are only provided the name of the project, we use hand-matching assisted by an application of the Hungarian algorithm documented in Pecenco (2019). This is a simple algorithm that defines optimal assignment globally instead of observation by observation.[4]

Finally, we include a second dataset of firm creation from ONAPI. We also follow the procedure above but with slight modifications to the match requirements since we did not hand match any of the observations.[5] We match 67.9% of the original dataset to a record in the DGII taxpayer universe. 97% of these matches are to unique taxpayers.

A2 Dynamic treatment effects

In this setting, individuals are permitted to enter multiple lottery events. This presents the potential for lottery winning to affect the likelihood of entering and winning future events. This effect may partly be behavioral, but is also deterministic—contract recipients are not allowed to enter future events until they finish their contract. Figure A5 shows that the combination of these mechanisms allows lottery non-winners to win additional lotteries in subsequent years. Although these effects are modest, the results indicate that our main estimates are a function not just of the effects of lottery winning on subsequent outcomes but also their effects on future lottery winning. Following the parlance of Gelber et al. (2015), we call these our "static" estimates.

A separate, and equally valid, treatment effect is to measure the effect of lottery winning on outcomes after removing any effect of lottery winning on subsequent lottery winning.

[3] They are not differential by treatment and control ($p = 0.92$), so we set them to 0.

[4] Assignment problems such as this are more common in economics, but usually require the additional property of stable matching. Matching pieces of data clearly does not require stable matching. This matching algorithm should be used when matching dataset A (the completion records) to dataset B (the full set of projects) when all records in A should be in B.

[5] We did not want to do hand matching here because of the size of the dataset and its minor importance. We define a match if two names correspond with a Jaro-Winkler score of 0.985 and there is no other possible match with a Jaro-Winkler score within 0.015 units, or if the names correspond with Jaro-Winkler score of 0.95 and there is no other possible match within 0.04 units.

To do so, we adopt the method of Cellini et al. (2010). They develop a recursive, two-step estimator that takes current period static treatment effects and removes the dynamic component by subtracting the combined future winning probabilities and estimated period by period dynamic treatment effects. We call them the dynamic estimates. Formally, let τ represent the relative number of years since event entry, π_τ be the effect of lottery winning on lottery winning in year τ, β^S_τ be the static treatment effect in year τ, and β^D_τ be the dynamic treatment effect in year τ. The estimator is

$$\beta^D_\tau = \beta^S_\tau + \sum_{h=1}^{\tau} \pi_h \beta^D_{\tau-h}. \tag{1}$$

To operationalize this approach, we first estimate π_τ and β^S_τ in a seemingly unrelated regression. Second, we solve for the β^D_τ estimates and construct standard errors using the delta method.

The dynamic estimator imposes two major assumptions that are potentially difficult to satisfy in this context. First, the estimator requires treatment effects to be the same across lotteries in our sample and those not in the main sample. Because each lottery imposes different requirements and is overseen by a different government institution, the time to completion and their subsequent impact on future outcomes may differ. Second, the estimates are required to not differ for those who win in the earlier events, our main sample, and those who continue to enter and win later. This is potentially unpalatable because those who continue to enter and win a lottery may have different treatment effects.

Table A10 shows the effects of lottery winning on future lottery winning, along with the static and dynamic estimates on employment and firm ownership. First, lottery winning has a modest effect on non-winning. Non-winners are 5.9 percentage points more likely to have won a contract one year after the lottery, with these effects quickly fading away. Intuitively, since the non-winners are more likely to become lottery winners, our dynamic treatment effect estimates will be larger than the static ones. Columns (2-3) show the static estimates, and columns (4-5) show the dynamic treatment effects. The estimates are quite similar throughout the study period, although they begin to diverge somewhat 4-5 years after the original lottery event. By period 5, the dynamic treatment effects suggest that lottery winning leads to an 18 percentage point reduction in formal wage employment and the ownership of .068 firms as compared to our static estimates of 12 percentage points and .054 firms, respectively. These estimates do not appreciably change the conclusions that lottery winners leave formal employment and own more firms.

A3 Effect of lottery winning on firm entrants

In this section, we describe the effects of winning a lottery for firms who apply. This description mirrors the analysis in Section 4.2.2, which analyzes effects for the individuals who entered the lotteries.

Figure A7 shows the main results on entrant firms shifting between sectors (private and government) and results on firm size. Panel A shows the effect of winning a lottery contract on the likelihood of having a lottery contract being open or unfinished. The results for firms are similar to those of individuals. The likelihood of having an open contract is highest in periods 0 and 1 and falls to .39 by period 3. The rate of contract completion is slower in the final 2 periods, with 30% of projects remaining unfinished.

Panel B shows extensive-margin sales to the private sector. In periods 0 and 1, the first two years of the contract receipt, firms report insignificant decreases in participation in the private sector. In periods 2-4, as contracts are being finished, winning firms move into selling to the private sector more than non-lottery winners. By period 5, there is a positive but insignificant effect of firms having any sales to the private sector.

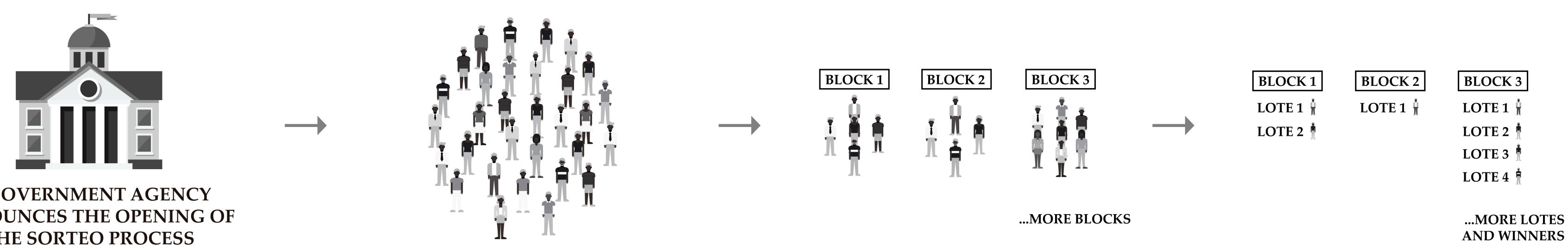
Panel C documents firms as having ever won a non-lottery contract bid by quarter. Few firms had ever won a contract with the government previously (3%), making the margin of ever winning a non-lottery contract particularly relevant. In the periods directly after receiving the contract, lottery winners see statistically insignificant decreases to this measure of firm participation with the government. Over time, this effect grows to be positive, but remains insignificant in this reduced sample.

In Panel D, we analyze the effects of lottery winning on firm size. Firms grow to have 1.5 more employees in period 1. Throughout periods 1-5 firms stay at least .9 employees larger, but the results are insignificant at the 95% level from period 2 onwards.

In general, we find some evidence of increases in firm size, but these results are underpowered due to the much smaller sample for firms than for individuals, the much larger heterogeneity in firm outcomes, and the analysis sample restriction we make in order to document project completion.

A4 Figures

Figure A1: *Sorteo* chronology



A *sorteo* process begins by a government agency making a public call, which includes information on the blocks (if any), number of contracts, and minimum requirements to participate.

All interested participants (individuals and firms) present their documentation to the government agency making the call.

If there are multiple blocks in the lottery event, participants choose one to enter at the time of document submission. In the same event, a unique individual can only be listed in one block.

In public events and per block, the government agency draws the names of the winners for each contract. In a block, there can be multiple winners or just one, depending on the event.

Figure A2: Lottery randomization

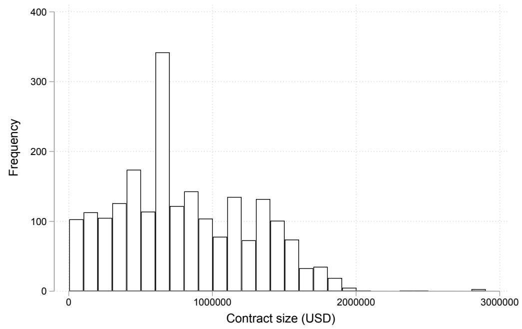
(a) Selection of lottery winners

(b) Winner celebrating

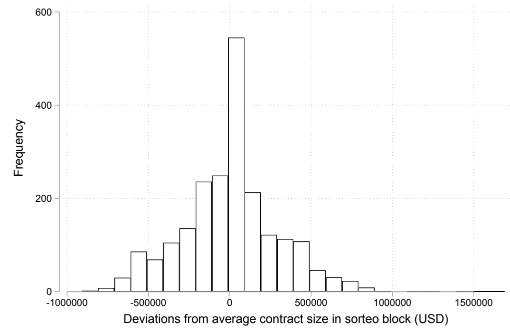
Source: Ministry of Education. The images show the public lottery events. Panel (a) shows entrant cards in the transparent cylinder being spun/randomized. Panel (b) shows a winner whose card was pulled.

Figure A3: Contract sizes and deviations

(a) Unadjusted contract size

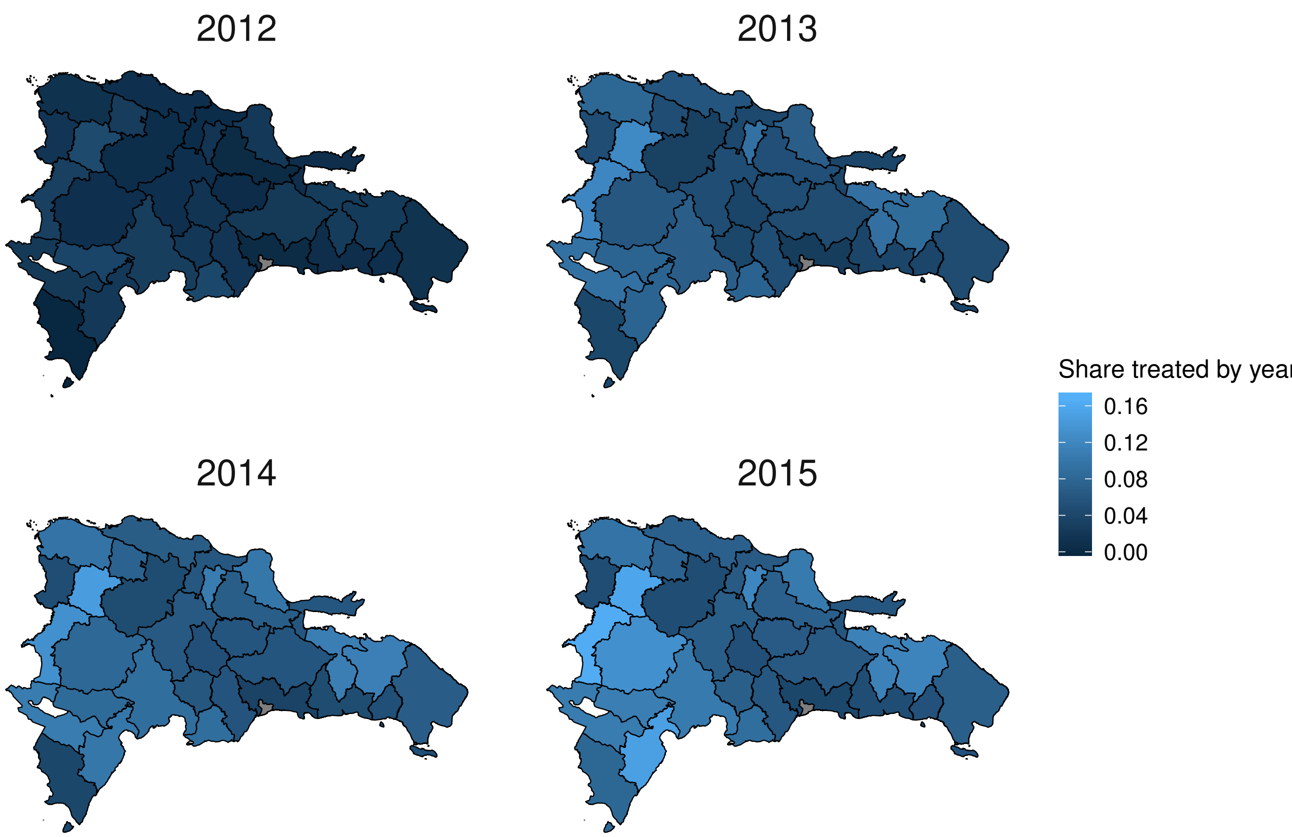


(b) Size deviations from lottery event-block average



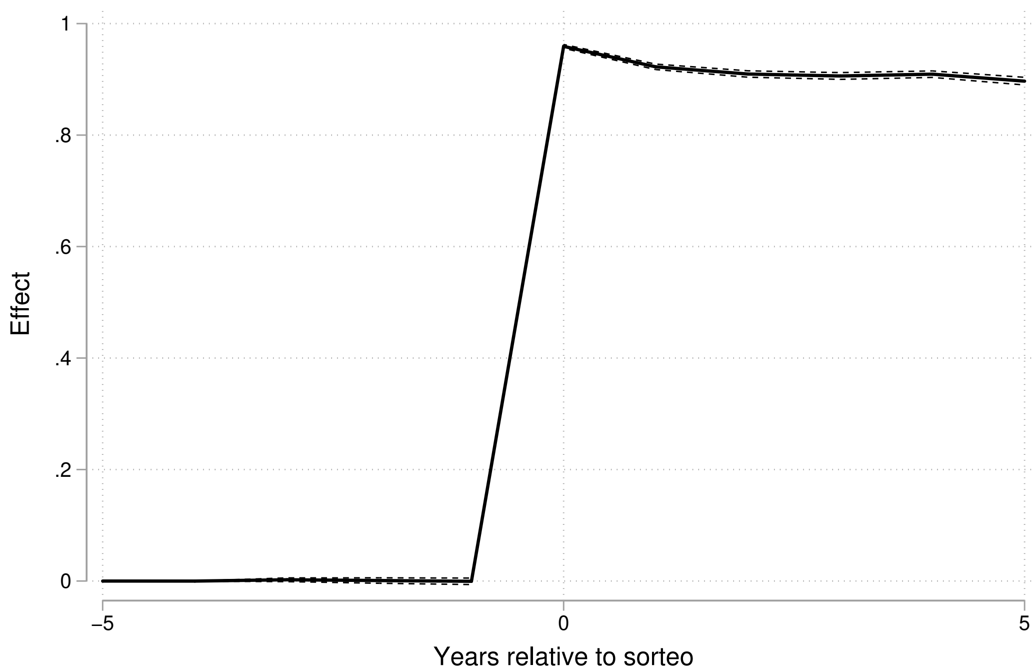
The figure shows contract sizes in USD. Panel (a) shows unadjusted contract sizes. Panel (b) shows deviations from lottery event-block average.

Figure A4: Share of lottery winners by province



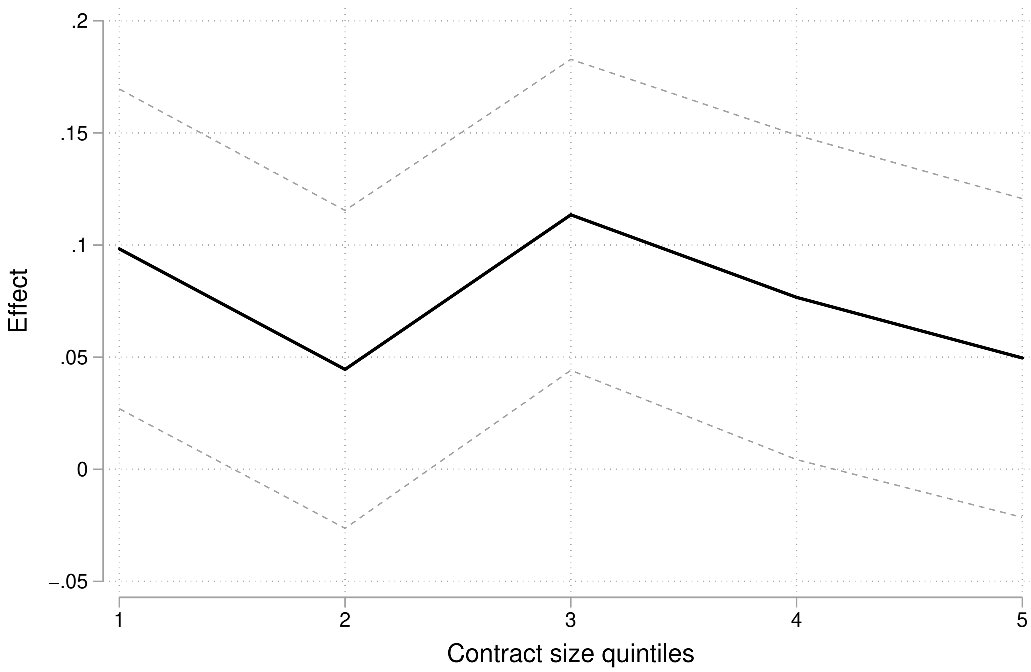
The figure shows the share of individuals by province who have received a contract up to year t. The denominator in the share is taken from linked Exequatur records.

Figure A5: Ever winner by whether won in this year



The figure displays regressions of whether an entrant has won a lottery up to year t on lottery winning. Regressions are estimated separately for each year relative to the lottery event date and include event-block fixed effects. Dotted lines represent 95% confidence intervals for standard errors clustered by individual.

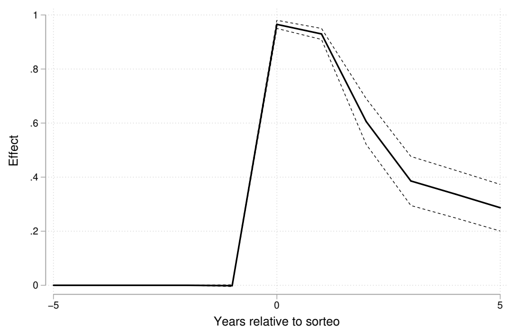
Figure A6: Effect on firm ownership by contract size quintiles



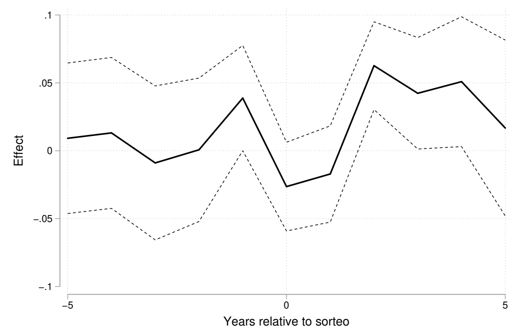
The figure displays estimates from a regression of ever firm ownership on lottery winning interacted with quintiles of contract size. Regressions include lottery event-block fixed effects. Dotted lines represent 95% confidence intervals for standard errors clustered by individual.

Figure A7: Sector participation for firms

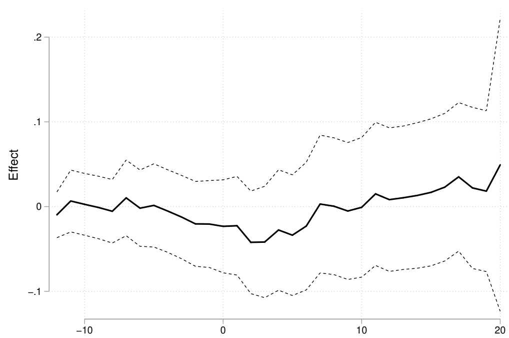
(a) Open contract



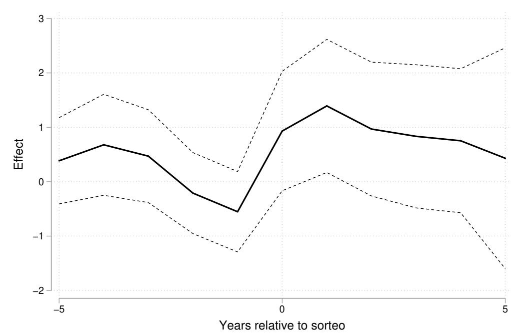
(b) Any work in the private sector



(c) Ever winning a non-sorteo contract



(d) Employees



The figures display regressions of the outcome in panel header on lottery winning. The sample includes firm entrants. Regressions are estimated separately for each year relative to the lottery event date and include event-block fixed effects. Dotted lines represent 95% confidence intervals for standard errors clustered at the individual level.

A5 Tables

Table A1: Placebo tests for project size (RD$10,000,000s)

	N	Var. Mean	Value
Number of sorteos, t=-1	2130	1.51	-.0554
			(.043)
Firm	2130	.14	.00294
			(.005)
Female	2110	.27	-.000927
			(.006)
Filed taxes, t=-1	2130	.83	.00359
			(.006)
Gross income, t=-1	2130	1937002.93	95,100
			(145617.170)
Asinh Gross income, t=-1	2130	10.43	.00195
			(.087)
Number of employees, t=-1	2130	.63	-.0168
			(.050)
Ever won non-sorteo, t=-1	2130	.02	-.000928
			(.002)
Ever firm owner, t=-1	2130	.12	-.00108
			(.005)
Joint *p*-value			.94

Controls include *sorteo*-block fixed effects. Heteroskedasticity robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: Characteristics of entrepreneurship

	Control Mean	Separate Interactions	Joint Interactions
Age	42.84	-0.0062***	-0.0065***
		(0.0014)	(0.0014)
Female	0.28	0.022	0.0013
		(0.040)	(0.040)
Ln income, t-1	10.62	-0.0032	0.00073
		(0.0030)	(0.0035)
Formal employed, t-1	0.55	-0.040	-0.064
		(0.034)	(0.040)
From capital	0.45	-0.033	0.0023
		(0.035)	(0.036)
N			16418

The sample is individuals. The dependent variable is whether the individual ever became a firm owner. Column (1) is the mean of the heterogeneity variable in the control group. Column (2) are coefficients of the interaction terms *Winner* $*$ *Variable* estimated in separate regressions. Column (3) are the same interaction terms estimated jointly. Controls in all regressions include the full set of interacted variables and lottery event-block fixed effects. Standard errors clustered at the individual level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Firm co-ownership networks

	Firm co-ownership		
	(1)	(2)	(3)
Same block	.00014***		
	(.000033)		
Winners	.000061**	.000064**	
	(.000028)	(.000028)	
Same block * Winners	.0026	.0022	.0028
	(.0019)	(.0019)	(.0017)
Block and block FE	X		X
Block-block FE		X	
Sample Mean	.00	.00	.00
N	140564194	140564194	390286
Sample	All	All	Winners

This table analyzes firm co-ownership with other entrants in the same *sorteo*-block. Sample is from most completed sorteos. An observation is at the entrant by entrant link level. Same block means the links come from the same *sorteo*-block. Winners means that both linked entrants were winners. Fixed effects vary by regression. Block-block fixed effects are full interactions between all pairs of entrant *sorteo*-blocks. Block by block fixed effects are separate fixed effects for each entrant. Column (3) is restricted to only lottery winners. Standard errors are clustered at the block-block level and in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Managerial experiences and loans

	Experience		Loans	
	(1)	(2)	(3)	(4)
	Temporary employees	Sub contractors	Number	Amount
Winner	6.1**	.82*	.14	.46
	(3)	(.46)	(.091)	(.73)
Control Mean	31.51	5.55	.66	5.14
N	691	691	691	691

This table reports estimates of the effect of lottery winning on managerial experiences and loans received. Sample is restricted individual survey respondents. Columns (1-2) report the most temporary employees managed and most subcontractors worked with in their career. Columns (3-4) refer to the number of loans and amount of loans the individual currently has. Loan amount is transformed by the inverse hyperbolic sine function. The regressions are weighted by the inverse probability of sampling. Controls include lottery event-block, age bins, and surveyor by month of survey fixed effects. Standard errors clustered at the individual level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Becoming a firm owner, by ownership and firm type

	(1) All	(2) Starting owner	(3) Majority owner	(4) Constr-uction	(5) Comm-erce	(6) Other sector
Winner	0.078***	0.069***	0.051***	0.064***	0.024**	0.028**
	(0.017)	(0.017)	(0.013)	(0.015)	(0.0097)	(0.013)
Control Mean	.35	.32	.11	.2	.058	.15
N	16855	16855	16855	16855	16855	16855

This tables shows estimates of the effect of lottery winning on entrepreneurship by type. Sample is restricted to individuals. Outcome is whether an individual ever became a firm owner, defined by panel header. Starting firms is defined as becoming an accionista within 6 months of firm registration. Majority owner is defined as having a firm ownership share above 50%. Controls include lottery event-block fixed effects. Other sector firms are neither construction nor commerce firms. Standard errors clustered at the individual level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Effects of lottery winning on created firm government contracts

		Sorteo	Non-sorteo		
	(1)	(2)	(3)	(4)	(5)
	Num firms	Ever won	Ever won	Num won	Asinh(value)
Winner	0.15***	-0.00049	0.0055	0.0044	0.00086
	(0.032)	(0.0030)	(0.0061)	(0.013)	(0.015)
Control Mean	0.49	0.01	0.03	0.05	0.05
N	16855	16855	16855	16855	16855

This table reports estimates of the effect of lottery winning on created firm interactions with the government. Column (1) reports all firm creation. Columns (2-5) report firm government contracts aggregated up to the individual level. *Sorteo* contracts refer to the randomized procurement scheme, while Non-*sorteo* contracts are non-randomized procurement schemes. Controls include lottery event-block fixed effects. Standard errors clustered by individual. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Registering as a firm owner (ONAPI)

	Any firm	Num firms	Num firm shares
	(1)	(2)	(3)
Winner	0.033**	0.036*	0.034*
	(0.014)	(0.020)	(0.018)
Control Mean	0.16	0.20	0.18
N	16855	16855	16855

This table shows estimates of the effect of lottery winning on firm creation using data from ONAPI. Controls include lottery event-block fixed effects. Standard errors clustered at the individual level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Heterogeneous effects based on number of past lottery entries

	Ever firm owner
	(1)
Winner * Prev. event == 0	.051
	(.032)
Winner * Prev. event == 1	.097***
	(.031)
Winner * Prev. event == 2	.15*
	(.082)
Sample Mean	.32
N	12399

This table reports estimates of the effect of lottery winning based on past number of lottery entries. We exclude the first lottery event. Standard errors clustered by individual. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: Basic choice

	(1) Choice
Mean	
Dem. Contract %	24***
	(1.2)
Dem. Contract % Sq.	-103***
	(7.5)
Dem. Avg. Value	.052***
	(.011)
Distance (km)	-.038***
	(.00026)
Age	.33***
	(.016)
Age Sq.	-.0032***
	(.00018)
Female	-.22***
	(.065)
Employed	.35***
	(.059)
Income	3.7e-08***
	(1.1e-08)
Firm owner	-.1
	(.11)
Sorteo ID 1	1.7***
	(.045)
Sorteo ID 2	1.3***
	(.045)
App. Cost	-.011***
	(.00044)
θ	-3.1***
	(.045)
SD	
θ	2.8***
	(.045)
LL	-57167
Chi2	8045
N	1425368

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A10: Effects on employment and firm ownership, static and dynamic specifications

		Static β		Dynamic β	
	(1)	(2)	(3)	(4)	(5)
	Won lottery	Employed	Num firms	Employed	Num firms
Winner * Year 0	.97***	-.015	.0025	-.015	.0025
	(.0046)	(.019)	(.011)	(.02)	(.0089)
Winner * Year 1	-.059***	-.088***	.011	-.089***	.011
	(.0046)	(.019)	(.011)	(.021)	(.011)
Winner * Year 2	-.011**	-.1***	.029**	-.11***	.03**
	(.0046)	(.019)	(.011)	(.022)	(.013)
Winner * Year 3	.0012	-.12***	.041***	-.13***	.044***
	(.0046)	(.019)	(.011)	(.024)	(.016)
Winner * Year 4	-.00018	-.12***	.051***	-.15***	.058***
	(.0046)	(.019)	(.011)	(.027)	(.019)
Winner * Year 5	-.000093	-.12***	.053***	-.18***	.067***
	(.0046)	(.019)	(.011)	(.034)	(.024)
Control Mean	.021	.68	.091	.68	.091
N	100182	81852	100182	100182	100182

This table analyzes the effect of lottery winning on entrant employment and firm ownership for both static and dynamic treatment effects. Column (1) shows the probability of winning a lottery in year t on winning in this sorteo. Columns (2-3) show the standard, static treatment effects. The dynamic treatment effects estimated in columns (4-5) follow the method of Cellini, Ferreira, and Rothstein (2010). Num firms is the number of firms owned, scaled by participation and ownership period shares. The control group mean is from period 0. The sample is restricted to individuals. Regressions on each period are estimated jointly. Controls include lottery event-block by period fixed effects. Standard errors clustered by individual. The dynamic treatment effect standard errors are estimated by the delta method. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.