

# The Political Nature of Entrepreneurship in Developing Countries

Experimental Evidence from Tunisia and Senegal

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We examined the role of political connections in the choices of young prospective entrepreneurs by conducting original surveys and field experiments in Tunisia and Senegal, countries where interest in entrepreneurship is high despite the complex political obstacles facing entrepreneurs. We first administered surveys with embedded conjoint experiments to 1,110 young people to assess the determinants of entrepreneurial intention. Next, we recruited a subsample of survey respondents to participate in entrepreneurship training sessions in Tunis and Dakar where government officials offered advice to young entrepreneurs. The survey results reveal that young people are very uncertain about whether connections will affect their career prospects, yet rate connections to political parties as the most valuable attribute a prospective entrepreneur could possess. Randomized training sessions increased entrepreneurs' intention to create businesses with between 10 and 17% of this effect mediated by an exogenous increase in political connections. Long-term followup shows that although the treatment's effect on connections dissipated over time, the success of entrepreneurial efforts continued to be mediated by political connection levels. By demonstrating the powerful impact of political connections on desires to enter the business world, these results suggest that entrepreneurship promotion efforts will work better when conducted in tandem with high-level government engagement.<sup>1</sup>

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# 1 Introduction

Developing economies are changing at a breakneck pace. Fueled by increased competition, innovation, and international and domestic policy changes, a new class of entrepreneurs is entering the private sectors of the Global South (Amorós et al. 2019). Especially in postcolonial Africa, where the formal economy has historically been the domain of the wealthy and well-connected, these recent shifts have spurred optimism that entrepreneurship could serve as an engine for unlocking more equitable economic development. Many developing economies' private sectors, however, remain dominated by clientelism and favoritism (Dutta and Sobel 2016; Frye 2017). In this paper, we examine the additional barriers to entrepreneurship, already a risky economic behavior, that the lack of political connections cause in developing countries.

Because stimulating entrepreneurship can provide sustainable development to countries that struggle to provide meaningful employment to their citizens, it is important to understand the sources of entrepreneurial intention. Yet, in the rapidly shifting business environments of developing countries, the determinants of formal entrepreneurial intention remain largely unknown. Specifically, in these institutional environments of selectively enforced rule of law, how political connections moderate entrepreneurial intentions remains an open-ended question despite increased attention in the study of entrepreneurship and connections (Autio and Fu 2015; Urbano, Aparicio, and Audretsch 2019; Sun et al. 2020; Ge, Carney, and Kellermanns 2019). This is partly because of the historical difficulties in separating connections and private-sector outcomes. Connections are not distributed exogenously, and thus selection issues affect much of the research on determinants of entrepreneurial success. More problematically, measures of connections are not often included in existing datasets of entrepreneurs (Reynolds, Hay, and Camp 1999) despite the importance of such connections in low-income countries.

In this paper, we provide causal evidence for the impact of connections on entrepreneurial intention and make strides toward generalizability by conducting extensive fieldwork in two countries: Senegal and Tunisia. Both countries have experienced booms in entrepreneurial interest in recent years, yet both also have private sectors historically characterized by favoritism,

connections, and uneven enforcement of the rule of law.

In both Senegal and Tunisia, we use a combination of original surveys and field experiments to understand how connections impact entrepreneurial intention. First, we conducted online surveys targeted at prospective young entrepreneurs to measure respondents' existing connections as well as their entrepreneurship intentions. We also administered a conjoint survey experiment in which respondents judged the likelihood of entrepreneurial success based on randomized attributes, including connections. The survey thus served to measure important baseline covariates as well as to understand, in an experimental context, whether respondents considered political connections to be a meaningful predictor of success.

Second, to move beyond the survey context, we tested the effect of connections by implementing field experiments in both Senegal and Tunisia with random samples drawn from our surveys. We offered access to a training on entrepreneurship to this random sample. In total 83 respondents, representative of the overall population of entrepreneurial hopefuls, attended training sessions in the capital cities of each country. Included in some training sessions were discussions with local government officials directly involved in business development. By taking pre and post-surveys, and comparing them to our broader sample of respondents, we can identify the effect of a shock to the respondents' perceived connections on their intention to create businesses in the future. We thus gain causal traction on a question that has long stymied researchers due to selection bias.

The results of the conjoint experiments show that connections are one of the most important factors in the perceptions of young people about the success of businesses. Respondents believed political connections were as important as wealth or previous business ownership in determining success. Descriptive results also show that the sample was relatively unconnected at baseline: less than 10% of prospective entrepreneurs in the sample had access to strong connections to government officials or parliamentary representatives who could provide meaningful access to state resources.

The field experiments' results show that the training sessions increased the sample's intention to create businesses by between 10% and 20%. Crucially, we also show that training increased their perceived political connections, which mediates approximately 10-17% of the training's

effect. Interacting with government officials during training increased respondents' optimism about embarking on the arduous task of entering the otherwise inhospitable private sector.

Follow-up surveys conducted between six months to one year after treatment show that the treatment effect on entrepreneurship intentions and activity remained relatively robust. However, the effect of the treatment on political connections dissipated and even turned negative for some subjects, suggesting that, while the introduction to government officials increased connections over the short term, true relationships would require additional investment. We do still find strong mediation effects of the treatment in our follow-up surveys, which shows that connections continue to condition the effect of the treatment over the long term.

By providing descriptive and causal evidence on the interplay between political connections and intention to participate in the formal economy, this paper seeks to make several contributions. First, these results reaffirm the importance of possessing political connections in developing economies. Political connections can smooth the entrepreneurial process for those that have them. The survey also reflects the difficulty that young people face in obtaining connections. The sample was largely unconnected at baseline, and, even in an experimental setting, self-selection prevented many respondents from making use of political connections that could benefit their careers. Given that young people have limited opportunities to obtain connections apart from their families, these results reflect the inequalities inherent to developing economies where connections are paramount.

Second, these results show that absent reforms that break the linkage between connections and private-sector growth, entrepreneurship is unlikely to be the panacea that some policymakers suggest. Rather, increasing entrepreneurship opportunities may reproduce or enhance existing inequalities. The results suggest that policies intended to increase private-sector entrepreneurship should be coupled with reforms that aim to equalize access and render connections moot.

Third, our studies allow us to circumvent the selection problems that affect much research on political connections and economic outcomes. By showing that an exogenous increase in political connections, even when boosted in a relatively subtle manner, can motivate citizens' desires to participate in the formal economy, this paper hints at the massive impact that

political connections continue to play in the economies of the Global South.

Finally, this paper makes strides toward generalizability by conducting original surveys and field experiments in two countries where limited data has existed to date. Especially as interest in entrepreneurship in the Global South explodes, we expand the descriptive evidence base in the business and politics literature in these understudied contexts. By conducting the same studies in both the MENA region and sub-Saharan Africa, we also demonstrate the broad applicability of our findings.

## 2 Theory

Political scientists have argued for some time that a close or “embedded” relationship between private firms and government officials is necessary for economic development to achieve its potential (Evans 1989; Haber, Razo, and Maurer 2003). Political institutions are ultimately a reflection of political coalitions and sustained economic development of the kind observed in industrial Europe and postwar East Asia came about through political incentives that aligned with growth (Waldner 1999; Slater 2012; Kang 2002). These macro-level arguments, though, can be difficult to apply to the micro-level of firm creation, which is necessarily a function of an individual’s aspirations, access to resources, skills, and creativity (Schlaegel and Koenig 2014). For these reasons, entrepreneurship promotion efforts tend to abstract away from the political environment, modeling their training on Western MBA programs that try to build an “entrepreneurial spirit” among students (Neal and Finlay 2008).

While we believe that such training can be very valuable for prospective entrepreneurs, we also argue that this type of entrepreneurship education can be deficient if it falsely presents a narrative in which only the talent and ambition of the individual determine success. In so-called predatory states (Acemoglu, Johnson, and Robinson 2004), government officials may actively target successful businesspeople, and investment of personal resources into new ventures is tinged with risk not only in terms of fickle consumer tastes but also the actions of bureaucrats who can shut down or obstruct the operation of companies. Operating a business in these environments requires protection via alliances with those who hold power, or lacking these

connections, influential foreign patrons (Markus 2015; Raza et al. 2020).

Research on the impact of political connections overwhelmingly argues their value. For those who have them, political connections can smooth the process of operating a business. Among other benefits, connections can help to circumvent the red tape in applying for permits (Bhandari 2023), ease the process of acquiring vital capital (Khwaja and Mian 2005), and lead to preferential contract enforcement (Lu, Pan, and Zhang 2015). Yet selection issues affect much of the work on connections. Because political connections are more associated with wealthy or socially privileged citizens, untangling the role of connections specifically in the business world is difficult. Furthermore, because much observational work takes existing firms as the unit of observation, it might understand the value of connections by not considering the would-be firms that never came to be or that fizzled out prematurely. While research has used quasi-experimental methods (Fisman 2001; Szakonyi 2018) and more recent work attempts to randomize political connections (Bhandari 2022), in terms of understanding the selection process into the actual creation of businesses, research remains scant.

In this article, we want to understand how early access to connections for young people affects their willingness to engage in entrepreneurship. To do so, we broaden our frame to the population of people with some interest in entrepreneurship in a given country; we want to then understand what leads some of these potential entrepreneurs to take steps toward this type of risky economic behavior. We hypothesize that access to political connections can affect this choice; young people make strategic choices based on their access to connections and their assessments of the plausibility of succeeding in entrepreneurship.

Because these decisions are strategic, we face a difficult inference problem. If we were to observe a cross-section of the general population, we might find a negative correlation between connections and successful entrepreneurs if highly-connected people opt for careers in government or politics. This type of selection or collider bias would induce associations that might not exist if people could make decisions about careers without having to take connections into account. Conversely, there are also over-time dynamics as entrepreneurs without connections may succeed at lower rates, leading to a positive over-time relationship between entrepreneurship and connections. For this reason, we want to start relatively early

in the selection process with young people and employ some experimental techniques to see if we can exogenously shape some of the environment that determines political connections.

We thus hypothesize that an increase in a young entrepreneur's political connections should cause an improved self-assessment of the possibility of engaging in entrepreneurship by potential entrepreneurs:

Hypothesis 1: An increase in a young entrepreneur's political connections should cause an improved self-assessment of the possibility of engaging in entrepreneurship by potential entrepreneurs.

Hypothesis 2: An increase in a potential entrepreneur's political connections should make their entrepreneurial ventures to be more likely to survive and more profitable.

Hypothesis 3: Increased interactions with government officials will result in higher self-reported political connections among potential entrepreneurs.

Hypothesis 4: Increased interactions with government officials will result in higher self-reported political connections among potential entrepreneurs if the entrepreneurs have full-time employment.

These hypotheses were included in our [pre-registration](#). The pre-registration also calls for exploratory analysis of subgroup effects, including political affiliation, religious values, ethnicity, and family background. We note that for reasons of space we do not test H4 in the main results section.

### **3 Research Design**

Because we are interested in mediation and indirect effects of our treatment, we need to be careful to try to understand under what conditions we can estimate mediated effects of the treatment given that, while we can randomly assign the treatment, we cannot directly

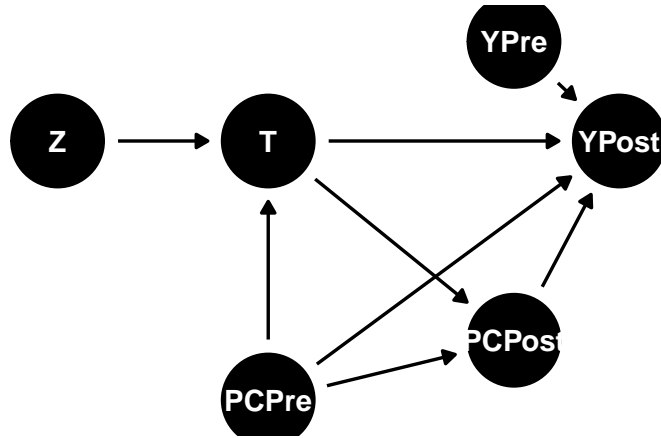


manipulate a subject’s political connections. To understand how to best identify these effects, we put forward a proposed causal graph in Figure 1 panel A. The graph includes nodes for our treatment  $T$ , treatment invitation  $Z$ , entrepreneurship outcome  $Y_{post}$ , pre-treatment level of interest in entrepreneurship  $Y_{pre}$ , pre-treatment level of political connections  $PC_{pre}$ , and post-treatment level of political connections  $PC_{post}$ . As the diagram makes clear, we implemented a form of an encouragement design by inviting people to the training. Our instrument  $Z$  is unconfounded as it is randomly assigned, and for these reasons, we intend to implement two-stage models to account for the selection process into treatment. However, we need to be aware that the prior level of political connections  $PC_{pre}$  could affect whether people show up to the treatment  $T$  and as such should be included as an adjustment covariate in the first-stage and second-stage models to account for selection into treatment. We incorporate  $Y_{pre}$  in the second-stage model of the treatment on the outcome  $Y_{post}$  as it is an exogenous covariate that can reduce the variance of  $Y_{post}$ —in other words, doing so allows us to estimate the over-time changes in the outcome rather than solely the cross-sectional effect.

In addition, we need measures of pre-existing connections  $PC_{Pre}$  to identify the effect of the mediator, the post-treatment increase in connections  $PC_{Post}$ , because the treatment is likely to be more efficacious among people who already have pre-existing connections and who also may be more likely (or possibly less likely) to show up for the training. As a result, we need to obtain estimates for all of these variables in order to have some confidence that we can not only estimate the effect of the treatment on entrepreneurship intentions but also the mediated effect of the treatment on entrepreneurship through increasing  $PC_{Post}$ .

To obtain the necessary measurements for Figure 1, we designed a multi-stage research plan in Tunisia and Senegal to maximize both internal and external validity. These two countries are former members of France’s colonial empire and share much in common in terms of both the institutional foundations of government and elite cultural attitudes toward economic development. In addition, both countries are ranked as some of the highest in the world for the proportion of people with a strong interest in entrepreneurship according to the Global Entrepreneurship Monitor (Reynolds, Hay, and Camp 1999). Finally, there is ample reason to believe that political connections matter to the success of both established and prospective

A Causal Diagram



B Adjustment for Mediation

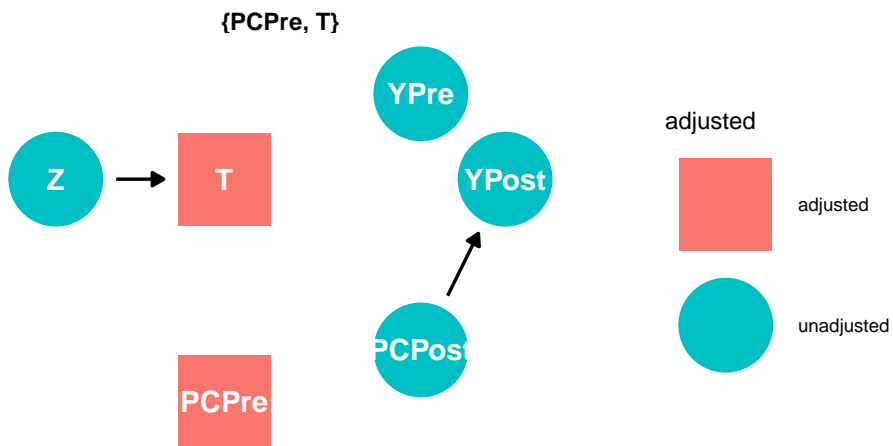


Diagram A shows causal process for our invitation to the experiment Z, treatment T, pre-treatment political connections PCpre, post-treatment political connections PCpost, pre-treatment entrepreneurship interest Ypre, and post-treatment entrepreneurship interest Ypost. Diagram B shows the necessary adjustments to identify the effect of PCpost on Ypost for the purposes of mediation analysis.

Figure 1: Causal Process for Entrepreneurship Intentions and Political Connections

businesses (Rijkers, Freund, and Nucifora 2014).

In both countries, we administered an online survey with embedded conjoint experiments and measures of political connections to a sample of potential entrepreneurs from both urban and rural areas. Second, we invited a randomized subset of these hopeful entrepreneurs to training sessions in the countries' capital cities where they were given the opportunity to develop political connections, and we collected further post-treatment data for variables  $Y_{post}$  and  $PC_{Post}$ . We intend to also perform follow-up surveys in six months to one year's time following the treatment in both countries that will give us additional measures of  $Y_{post}$  and  $PC_{Post}$  for as much of the original sample as we can re-contact.

One limitation of our research design is that we do not obtain post-treatment measures of  $Y_{post}$  and  $PC_{Post}$  for the control groups until follow-up six months later while we obtain such measures for the treatment group immediately before and after the experiment. For these reasons, to model the effect of the treatment on the outcome, especially in terms of mediation, we have to re-use our  $Y_{pre}$  and  $PC_{pre}$  measures from the survey as our measures  $Y_{post}$  and  $PC_{post}$  for the control group. Doing so requires us to make the assumption that these variables did not change differentially with the control versus the treatment, but we think it unlikely that these variables would change in any meaningful direction in the time window between survey implementation and field experiment implementation. Furthermore, any differences are most likely orthogonal to the field experiment, and as such would only factor in as random noise.

To check for the reasonableness of this assumption, as we further specify below, we will also consider cross-sectional models in which we pool our data from the field experiment with the original survey data to ascertain the effect of  $T$  on  $Y_{post}$ . However, we cannot check for mediation effects of  $T$  on  $PC_{post}$  without explicitly modeling the pre/post nature of the causal graph.

### 3.1 Outcomes: Political Connections and Entrepreneurial Activity

While  $Y_{post}$  and  $PC_{post}$  can be measured in many ways, in the context of potential entrepreneurs, *perceptions* of both variables tend to be the most important factors. For example, the perception of being politically connected may drive a young entrepreneur to create a business, despite the reality of that connection or the strength of that connection in the overall distribution of connections in the country. If young people perceive connections as being necessary to become a successful entrepreneur, then they will opt for other careers if they lack these connections. Similarly, a high, self-perceived intention to create a business usually predicts the creation of that business.

To measure connections  $PC_{post}$  and  $PC_{pre}$ , we asked respondents detailed questions about the types of connections they have and how they were able to obtain these connections (see Figure 2 and Figure 4). Respondents were asked to rate their total level of political connections on a scale of 0 to 10, what we call the general scale, and were also asked a more specific question of how likely on a scale of 0 to 10 that they could arrange a meeting with a member of parliament relative to an average citizen.<sup>2</sup>

#### 3.1.1 Entrepreneurship

We have a variety of measures of entrepreneurship included in our pre-registration. We include those two below that we included as main outcomes in the study and were included in all waves of the study, including the pre/post questionnaires. There are additional outcome measures which we report using the baseline and endline surveys for the entire samples.

##### 1. Entrepreneurial Intention

To measure entrepreneurial intention  $Y_{post}$  and  $Y_{pre}$ , we asked respondents their likelihood of creating a business with the following question: “How likely are you, alone or with others, to start a new business, including any type of self-employment, within the next three years?”

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<sup>2</sup>Members of parliament in Senegal perform regular constituent services and are often ingrained in clientelistic networks.

Responses were ordinal on a 4-point scale, corresponding to “not very likely,” “a possibility, but a small one,” “more likely than not,” and “I have strong intentions of starting a business.”

## 2. Attempts to Start Businesses

We measure actual attempts to start a business as a binary response Yes/No to the question, “Over the past year, did you, alone or with others, try to start a new business?”

### 3.2 Sample recruitment and baseline connections

We used online recruitment techniques to target a sample of young people. We targeted young people for several reasons. First, young people comprise the group most likely to contain hopeful entrepreneurs in Tunisia and Senegal, and by targeting this group we maximized our chances of recruiting a representative sample of people considering entering the private sector via entrepreneurship. Second, focusing on a young cohort enables us to gain a better understanding of the origins of political connections. Because political connections are endogenous to progressing in the private sector, targeting young people before they begin their careers allows us to exogenously impact their connections and to study the role that political connections play over the course of their career.<sup>3</sup> Because a sample of young people has had relatively little opportunity to obtain political connections through career or political advancement, most of their political connections stem from people they know in their family or broader ethnic or kin group.

We recruited our sample using Facebook advertisements targeted at Senegalese and Tunisian young people from 18 to 30. Respondents first filled out a screener survey which included their career preferences and their educational background. From this screened list, we selected those who had either completed higher education or were currently enrolled in higher education and who either ranked entrepreneurship as one of their top two career interests or who ranked government careers as a top career interest. The government careers group was included to

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<sup>3</sup>We intend to continue to work with this sample over the course of their careers to track connections and economic outcomes.

allow us to make comparisons to a population that would likely be distinct from prospective entrepreneurs, though we did not invite these subjects to participate in the field experiment.

The selected respondents were then invited to complete a much longer survey that included our conjoint survey experiment. Respondents were offered a small, approximately 1 USD, mobile credit incentive for completing the first screener survey and a larger, approximately 10 USD, mobile credit incentive for completing the second survey. We then invited random samples of respondents from our surveys to complete a one-day entrepreneurship training in both countries.

The final sample skewed towards male respondents at 62% male and 38% female; this bias was expected as interest in entrepreneurship among males is generally higher than among females (Shinnar et al. 2018). In total, we obtained 501 completed surveys from Tunisia and 600 completed surveys from Senegal. In line with expectations for a sample of young citizens at the onset of their careers, our sample had relatively few political connections as measured on several dimensions. As Figure 2 and Figure 3 demonstrate, the modal respondent had a low level of self-reported political connectivity, with wealthier respondents reporting higher levels of parliamentary connections though no difference in overall perceived political connections. For those that do have connections, Figure 4 shows that they tended to be toward mid-level bureaucrats.

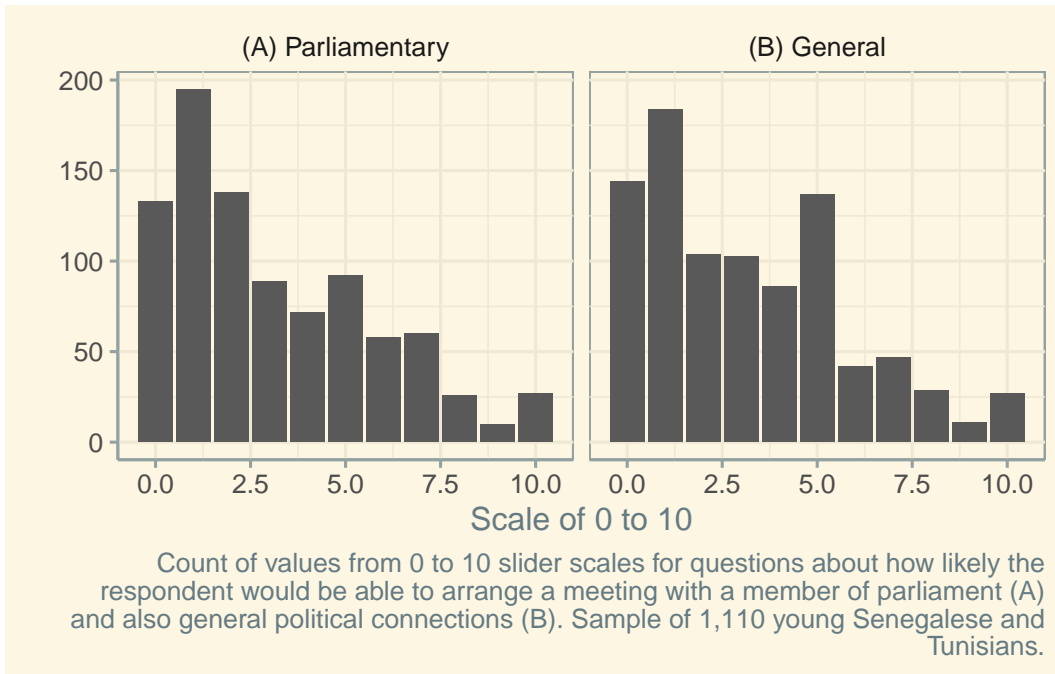


Figure 2: General Political Connections and Connections to Parliament Members among Survey Respondents

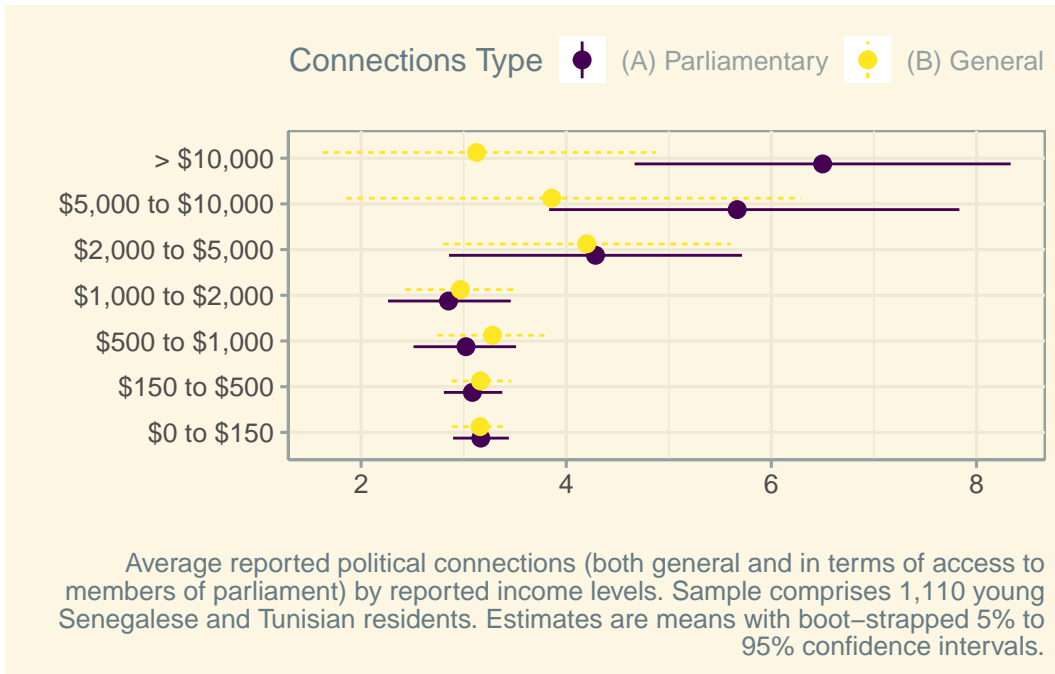


Figure 3: Average Political Connections by Household Income

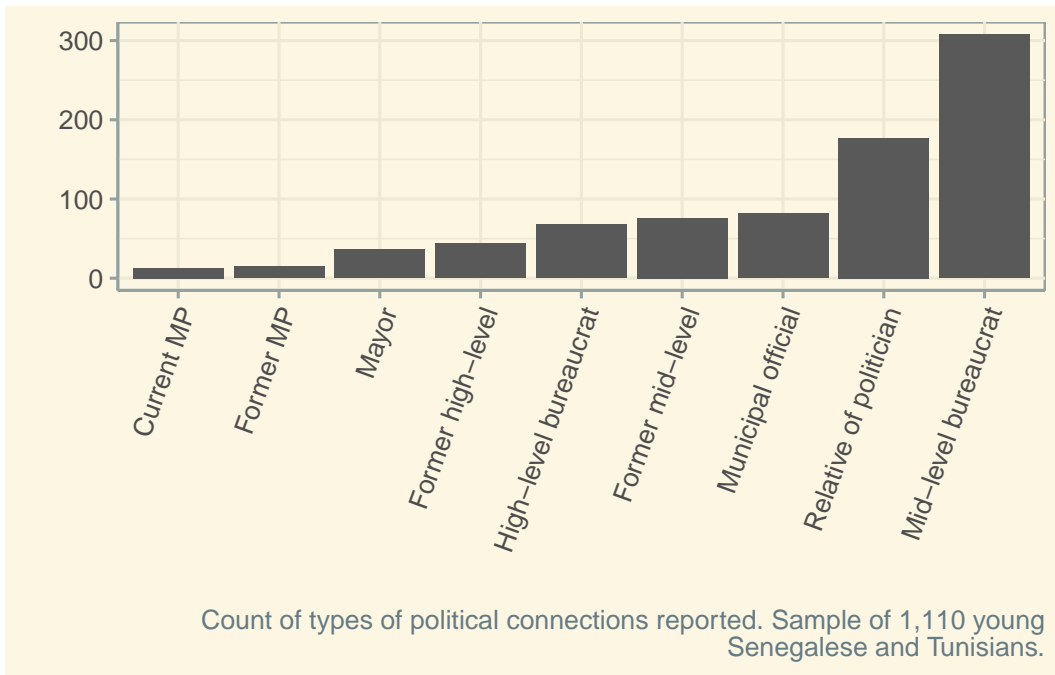


Figure 4: Political Connections Specifics



### 3.3 Conjoint design

We included a conjoint experiment in the survey as a sophisticated way of measuring what our respondents thought about the potential determinants of entrepreneurship. While we could theoretically use this data to test our causal graph in Figure 1, doing so would require us to make significant assumptions about hypothetical versus actual responses to entrepreneurship opportunities, and in addition the conjoint does not change respondents' connections in any way. Instead, we value this data as providing excellent descriptive information about what respondents thought about connections and entrepreneurship before the field experiment, permitting us to infer possible mechanisms that the field experiment could affect. Compared to the field experiment, which could only affect a subset of factors that might affect entrepreneurship interest, the conjoint experiment can examine a wide variety of factors, including access to credit and skills training.

Respondents saw two hypothetical profiles of entrepreneurs at a time and selected which profile they believed would be more successful. Table 1 shows the attributes included in the conjoint experiment and Figure 5 shows an example of the experiment in the survey. To estimate average marginal component effects (AMCEs), we use the `cjoint` package in R which implements multiple regression estimation. Each respondent completed 4 choice tasks and our conjoint sample totaled 8156 profiles evaluated. When estimating AMCEs, we cluster standard errors at the respondent level to account for multiple choice tasks per respondent.


Attribute	Randomized traits
Political party	Ruling party, opposition party, no party
Geography	Urban, rural/interior
Family wealth	From a rich family, from a middle-class family, from a poor family
Father occupation	Government manager, government bureaucrat, farmer, unemployed factory worker, restaurant owner, journalist, business owner, banker
Gender	Male, female
Highest level of education	High school, college, professional degree
Prior business experience	Entry-level employee, manager, current business owner
Religion	Secular/atheist, Christian, Muslim, Islamist
Sector	Manufacturing, IT, data science, retail store, transportation tourism, import/export, restaurant

*Notes:* Asterisks indicate the reference trait of the attribute, used for estimating treatment effects.

Table 1: Attributes and their trait values

We will now present you with the profiles of two hypothetical entrepreneurs. The characteristics of each entrepreneur are shown below.

Randomized Attributes



	<b>Entrepreneur A</b>	<b>Entrepreneur B</b>
<b>Family wealth</b>	From a poor family	From a poor family
<b>Background</b>	Urban	Urban
<b>Political party</b>	Not a member of a party	Member of the leading opposition party
<b>Father occupation</b>	Government Manager	Unemployed
<b>Gender</b>	Male	Male
<b>Education</b>	High school	Professional degree
<b>Experience</b>	Current Business Owner	Entry-level Employee
<b>Religion</b>	Muslim	Muslim
<b>Startup Sector</b>	Import/Export	Transportation

Which of these entrepreneurs do you believe is more likely to successfully start a business?

Entrepreneur A

Entrepreneur B

Figure 5: Example of Conjoint Experimental Treatment

## 3.4 Field experiment design

### 3.4.1 Treatment

To directly test our hypotheses, we invited a randomized subset of the survey sample (represented by  $Z$  in Figure 1) to in-person training sessions in each country’s capital city. In Tunisia we held a one-day training on May 22, 2022, and in Senegal we completed training sessions on March 18, March 19, and May 20 of 2023. As we discuss in our pre-registration, our projected recruitment size for each training was 30 students, but we generally did not meet these goals for recruitment as each training had 16, 25, 19 and 22 respondents participate. We thus added the May 22 training to increase treatment enrollment. We were also able to increase sample size by recruiting more people into the online surveys; all of the respondents who were not invited to the training sessions comprise the control group.

We re-contacted all respondents approximately one year after the training in Tunisia and six months after the training in Senegal to have them retake the baseline survey. We were able to obtain a 57% follow-up response rate in Tunisia and a 75% follow-up response rate in Senegal. The difference in follow-up response rates is likely due to the increased time since follow-up in Tunisia and the fact that Tunisia’s mobile companies sometimes failed to transfer credits in a timely fashion.

We consider power analysis in the [pre-registration](#). The power of the design largely depends on the effect size as we were limited in how many people we could attract to training sessions given our desire to keep class sizes small and the expense of compensating subjects and trainers. We did have a reasonable expectation that the effect size would be relatively large as our subjects had an entire day together with meals and ample social interaction. Furthermore, while the experimental group was relatively small, the control group was quite large, which increased the power of the design. Given that this study is the first to combine government officials and entrepreneurship as an experimental treatment, we believe that our emphasis on quality over quantity is justified.

The training sessions in Tunis were held at a downtown hotel and in Dakar at a business incubator. Our implementing partners in both cases were local entrepreneurs who used their

local connections to recruit other entrepreneurs for panels and to recruit government officials with relevant expertise. The structure of our Senegal training sessions was different from our Tunisia training as the latter had a more open-ended structure with panels while the Senegal training sessions used a curriculum developed by the implementing partner, Pitch Palabre.

All training sessions—with the important exception of March 19—included government officials with a vested interest in promoting their agency’s work in encouraging entrepreneurship. In Tunisia we had senior officials from the government’s employment training organization and in Senegal senior officials from the Ministry of Finance attend the training and engage in open-ended question-and-answer sessions with respondents. We encouraged the officials to interact as much as possible with respondents, and they provided considerable information about support programs for young entrepreneurs. Respondents were engaged and participated actively in sessions. Indeed, the information was so valuable that the implementing partners informed us that they became aware of potential programs to apply for because of the sessions.

Attending the training was a non-trivial endeavor for many of our respondents as our samples included all districts within Senegal and Tunisia, and respondents traveled as much as 12 hours by bus. These problems were particularly acute in Senegal due to its larger size and poorer interior infrastructure relative to Tunisia, and so in Senegal, we additionally provided respondents with hotel rooms for those traveling several hours to the capital. In Tunis, respondents were able to travel back to their home district given Tunisia’s ample transportation links to the capital. We provided each respondent a payment of approximately 20 USD to cover these costs and in Senegal, we provided additional funding to those who had to travel from considerable distance. After travel expenses, all respondents received an equal amount of remuneration.

While we went to considerable expense and effort to recruit respondents from across the country, we consider sample diversity a key strength of the design. NGO-funded programs related to entrepreneurship are common in both countries but are often only found in urban areas where NGOs and universities are located. By bringing in young people from marginalized rural areas, we are able to understand the effect of the treatment on those who might never receive such opportunities.

As mentioned previously, for one training, March 19, we did not recruit a government official and instead invited more local entrepreneurs to present. This treatment was intended to be a placebo to test whether the government official’s presentation was the cause of any increased political connections as opposed to entrepreneurship education in general. When estimating general effects of the treatment, we include this arm, but when estimating mediation effects of connections, we exclude it.

### 3.4.2 Estimation

Our primary estimand of interest is the effect of a shock to respondents’ political connections on entrepreneurship intentions, opportunities, and activity. That is, we measure the indirect effect of the experimental treatment involving access to government officials on entrepreneurship that is mediated through increased within-subject political connections. Because the treatment was an invitation to the training sessions, results throughout should be considered intent-to-treat effects. Our specifications listed below conform to the causal diagram we propose in Figure 1.

For our main treatment specification, we use the following two-equation model for the total intent-to-treat (ITT) effect where  $g(\cdot)$  is the inverse logit function:

$$T \sim \text{Bernoulli}(g(\alpha_T + \beta_{T1}Z + \beta_{T2}PC_{pre})) \quad (1)$$

$$Y_{post} \sim \text{Ordlogit}(g(\alpha_Y + \beta_{Y1}T + \beta_{Y4}Y_{pre})) \quad (2)$$

And we use the following three-equation specification for our mediation model:

$$T \sim \text{Bernoulli}(g(\alpha_T + \beta_{T1}Z + \beta_{T2}PC_{pre})) \quad (3)$$

$$PC_{post} \sim \text{Normal}(\alpha_P + \beta_{P1}T + \beta_{P2}PC_{pre}) \quad (4)$$

$$Y_{post} \sim \text{Ordlogit}(g(\alpha_Y + \beta_{Y1}T + \beta_{Y2}PC_{pre} + \beta_{Y3}PC_{post} + \beta_{Y4}Y_{pre})) \quad (5)$$

For our experimental outcomes  $Y_{post}$ , we use Bayesian ordered logit models to account for

the ordinal nature of our intentions outcome and regular logit models for all binary response outcomes with weakly informative priors on  $\beta_i$ . We calculate ITTs using the g-formula method, i.e., by predicting the value of the outcome for each ordinal category and treatment and control separately and then subtracting to obtain an estimate of the ITT. To calculate indirect and direct effects of mediation involving political connections, we use the `marginalEffects` package in R to calculate the marginal effect of the treatment on connections and the marginal effect of connections on our main outcome. By doing so we can obtain indirect effects that are in the scale of the outcome despite using non-linear models.<sup>4</sup>

In addition, we consider as well the following cross-sectional model in which we pool our pre/post data from the field experiment to avoid re-using data from the control group as post-treatment measures:

$$T \sim \text{Bernoulli}(g(\alpha_T + \beta_{T1}Z)) \tag{6}$$

$$Y_{post} \sim \text{Ordlogit}(g(\alpha_Y + \beta_{Y1}T)) \tag{7}$$

In this specification,  $Y_{post}$  is equal to the original survey data for the control group and the post-treatment survey for the treatment group. The pre-treatment survey for the treatment group is included as an additional measure but is not explicitly modeled.

To clarify these two estimands, we refer to our within-subject ITT in (2) as  $ITT_W$  and the cross-sectional ITT in (7) as  $ITT_{CS}$ .

In addition, with our endline survey data collected approximately six months to a year following each treatment, we are able to estimate an ITT that is in essence a difference-in-differences effect as we have pre/post measures for all respondents. We denote this ITT as  $ITT_{DiD}$ . For these estimations,  $Y_{post}$  is equal to our end-line survey rather than the post-treatment survey.

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<sup>4</sup>This approach is quite similar to the `mediation` package in R except that we apply this method to Bayesian models which have jointly estimated mediation and outcome models.

## 4 Results

### 4.1 Political Connections

We first explore some descriptive findings relating to respondents' perceptions of the utility of political connections before we turn to the experimental treatments. While the sample was relatively unconnected, respondents still reported that political connections would be useful to have. Interestingly, Figure 6 shows that at baseline, there was no relationship between existing political connections and overall interest in entrepreneurship, whereas connected people had slightly more interest in government jobs. However, when asked whether a lack of connections could hold back their aspirations, nearly all respondents stated that it would cause problems or that it is a possibility, as Figure 7 shows. We believe that these descriptive statistics serve to highlight some of the issues with inference from cross-sectional samples: we might expect that political connections would powerfully condition reported interest in entrepreneurship, but we find only a weak relationship.



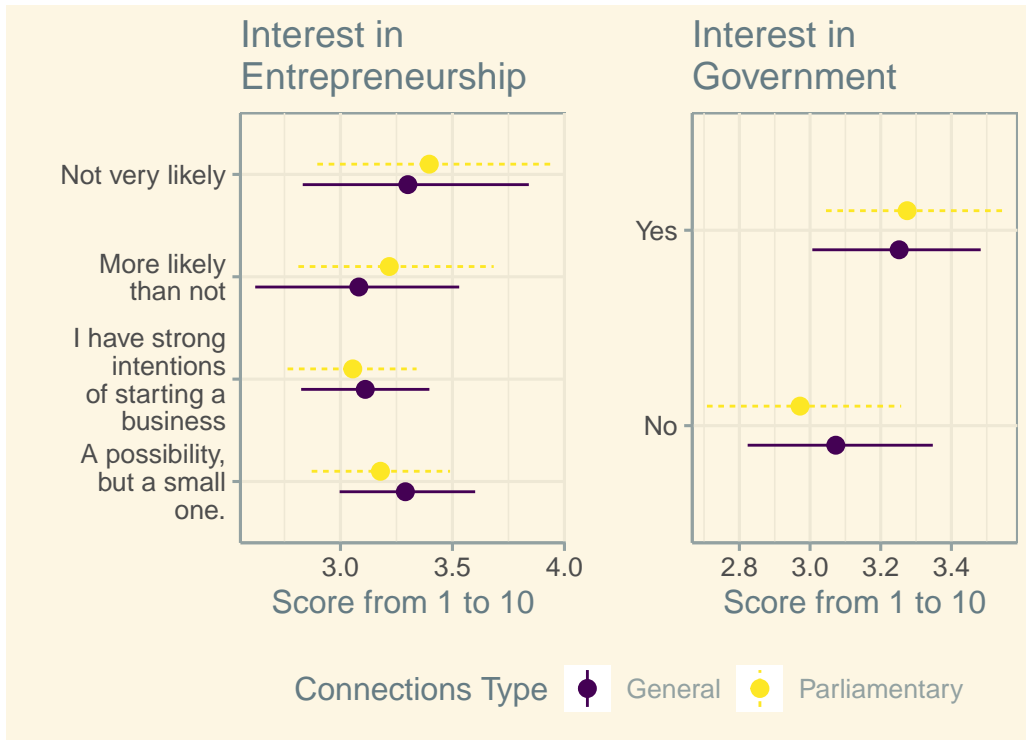


Figure 6: Average Political Connections by Future Career Interest

At the same time, it is interesting to note that the modal category in Figure 7 is being unsure about whether political connections might be a problem. This answer suggests that the uncertainty about political connections is a form of Knightian uncertainty—that is, respondents do not even have adequate information to put an estimate on the risk that not having connections would have on a business venture. This type of uncertainty may be particularly vicious because young entrepreneurs cannot account for what they do not know, and consequently, the outcomes in terms of behavior may be sub-optimal for the individual. The conjoint experiment results in the next section help clarify these issues.

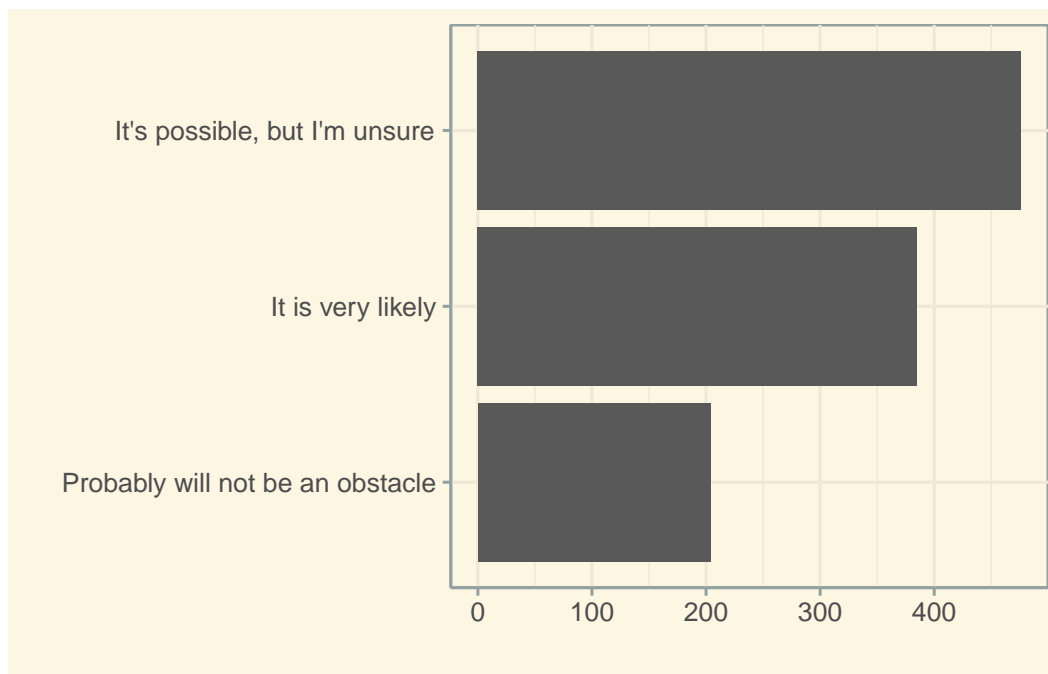


Figure 7: Will A Lack of Connections Cause Trouble for Starting a Business?

## 4.2 Conjoint Experiment

We next report the combined results of the conjoint experiment in both Senegal and Tunisia in Figure 8. These marginal treatment effects show a typical range for a forced-choice conjoint design with the largest effects equaling a +/- 10 to 15 pp change in the probability of a conjoint profile being favored. What is immediately obvious is that the two attributes with the strongest positive effect are both related to political connections: the single largest effect is for being a member of the ruling party (between +5 and +10 pp) and the second is having a father who was a government manager (between +3 and +12 pp). These two attributes have an even greater effect than that of having an entrepreneur who comes from a wealthy family background (between +3 and +8 pp).

The important caveat with these results is that AMCEs are direct effects of treatments; it is quite possible that wealthy family background could be stronger in combination with other factors we include. Nonetheless, the prominence of these political attributes and their effect

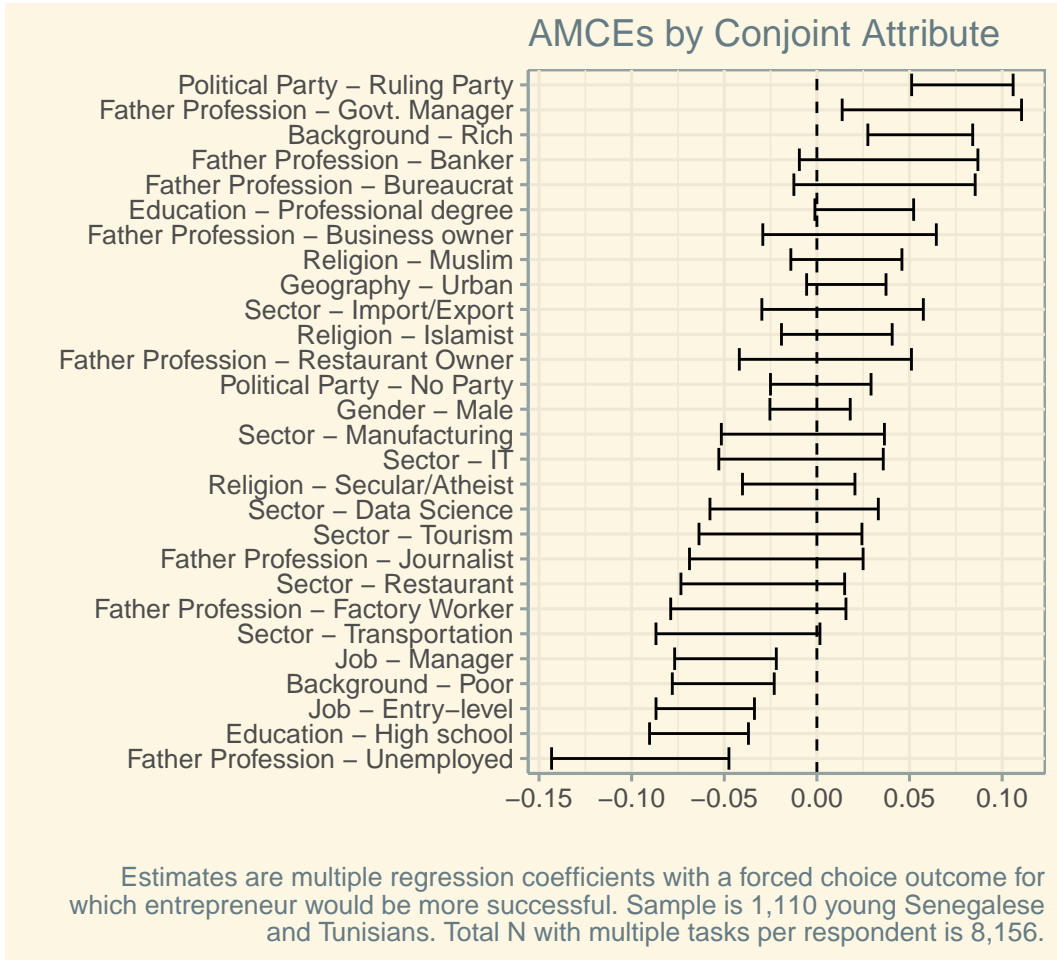


Figure 8: AMCEs for Entrepreneur Selection Conjoint

on the prospective success of entrepreneurs in the minds of Senegalese and Tunisian young people suggests that connections could well influence whether or not a young entrepreneur would take the risk to invest capital and time to start a business.

We also find it interesting to compare the results in Figure 8 with the descriptive statistics in Figure 7 where the modal survey respondent reports that they are unsure if connections would matter for entrepreneurship success. This survey response would suggest that connections may not be a primary barrier to entrepreneurship, but when we ask respondents to evaluate connections along with other attributes, they come out as some of the most important factors. There could be a variety of mechanisms that underpin this contrasting relationship, though the one we find most plausible is that the uncertainty surrounding political connections leads to a “flight to safety” effect where respondents view connections as one of the most dependable assets for young entrepreneurs. In other words, connections might not always help young entrepreneurs, but they are unlikely to hurt.

It might seem from the results in Figure 8 that young people with connections should be more likely to pursue entrepreneurship. We know from our survey data reported previously in Figure 6 that this is not true: there is a very weak relationship between reported political connections and the pursuit of the public sector or entrepreneurship as careers. The best explanation for this divergence is the nature of selection or collider bias—while an entrepreneur with connections might fare better than one without connections, young people with connections might prefer government careers over entrepreneurship due to less risk involved. These two contrasting strategic incentives would obscure cross-sectional relationships as reported in Figure 6 and are part of the reason we needed to implement a field experiment to gain a better understanding of how these variables are related to each other.

### **4.3 Field Experiment**

We first report the effect of the treatment on our main outcomes of entrepreneurship intentions and attempts at entrepreneurship. It should be noted that these results are based on the post-treatment survey that was completed within one to three days following the training, and so

changes in attempts at entrepreneurship should be seen as aspirational at this stage. Figure 9 shows the within-subject estimate of  $ITT_W$  and cross-sectional estimate of  $ITT_{CS}$  for the three ordinal categories of our entrepreneurship intention outcome (panel a) and our binary entrepreneurship attempts outcome (panel b). These results are adjusted for non-compliance using instrumental variable regression that takes into account respondents who received an invitation but did not attend the training as we described in equations (1) - (7).

As can be seen, the estimates of  $ITT_{CS}$  and  $ITT_W$  are quite similar but not identical, and in general, the within-subject  $ITT_W$  estimates have lower variance, as would be expected. In either case, the entrepreneurship training had a pronounced effect on encouraging entrepreneurship both by increasing the number who reported strong interest in entrepreneurship (panel a) and the number who reported that they were in fact attempting to start a business (panel b). In fact, the effect size appears to just as large for panel a as for panel b.

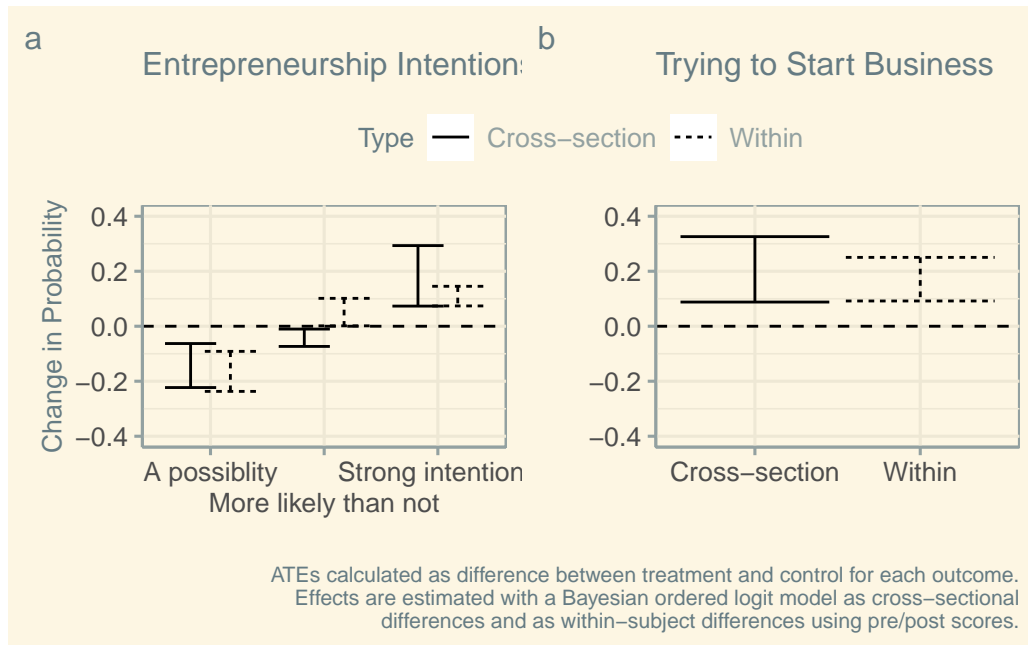


Figure 9: Pre/Post Treatment Effect on Entrepreneurship Intentions and Activity

While our main aim with the field experiment was not to design a compelling new technique for encouraging entrepreneurship, we do believe that our results show very strong effects of the

treatment on the young people who attended. This result is important for the validity of our analysis as the shock to entrepreneurship intentions needed to be strong enough to enable us to check for an indirect effect via political connections. We also know that the treatment was very valuable to participants, who were effusive in their thanks for being able to participate and who also tried to circumvent our experimental protocol by inviting friends and relatives to participate as well.

Having demonstrated that the treatment was effective at increasing the entrepreneurship intentions of our subjects, we turn next to mediation analysis. Because we employ non-linear models, we report marginal mediation effects by first calculating the effect of the treatment on the mediator and the mediator on the outcome as marginal effects and then multiplying these two estimates to obtain an indirect effect. Taking this step allows us to obtain indirect and direct effects that are in the scale of the outcomes (i.e. both the mediator as an outcome and entrepreneurship intentions and attempts). We report these effects for mediation of the  $ITT_W$  estimate in Table 2 based on equations (3) - (5).

The Indirect effects in Table 2 show that there are positive effects on entrepreneurial intentions (outcome 1) and starting businesses (outcome 2), though the indirect effect on starting businesses is weaker and not statistically significant. These positive indirect effects mean that the treatment increased connections *and* connections increased the probability of the outcome. We would note that the treatment only has an indirect effect via general connections rather than parliamentary connections. We would have expected this to be the case given that we introduced the subjects to government officials, not MPs.

Given these indirect effects, we can calculate the proportion of the treatment effect that is plausibly mediated through connections. Table 2 shows that approximately 17% of the treatment effect on entrepreneurship intentions is mediated through general political connections, while 10% is mediated by parliamentary connections. The mediation effect on starting businesses is positive but much smaller and not statistically significant. Furthermore, we note that while the effect of the treatment on connections is identified via randomization, the relationship between connections and the outcome requires the identification assumptions in Figure 1, namely that adjusting for pre-treatment connections identifies the effect of post-treatment

Table 2: Pre/Post Indirect and Direct Effects of Treatment and Political Connections

Outcome	Mediator Type	Effect	5%	Median	95%
Intentions	General	Direct	0.029	0.062	0.098
Start Business	General	Direct	0.112	0.176	0.250
Intentions	Parliamentary	Direct	0.050	0.089	0.124
Start Business	Parliamentary	Direct	0.120	0.180	0.264
Intentions	General	Indirect	0.001	0.004	0.009
Start Business	General	Indirect	0.000	0.002	0.006
Intentions	Parliamentary	Indirect	-0.003	0.000	0.002
Start Business	Parliamentary	Indirect	-0.001	0.000	0.001
Intentions	General	Proportion Mediated	0.065	0.179	0.346
Start Business	General	Proportion Mediated	-0.005	0.034	0.084
Intentions	Parliamentary	Proportion Mediated	0.028	0.102	0.202
Start Business	Parliamentary	Proportion Mediated	-0.018	0.019	0.054

<sup>a</sup> Table shows the quantiles of the posterior distribution for mediation and treatment effects calculated as sample average marginal effects. Mediators are the 1 to 10 scales for both general political connections and parliamentary connections (obtaining a meeting with an MP). It should be noted that the Tunisian field experiment did not include the parliamentary connections measure and thus the total effects are not equivalent for both types of connections.

connections on the outcome. Given that connections were measured immediately before and after the treatment, this assumption is relatively mild.

Table 3 replaces the treatment with our measure of interactions with government officials. We model this ordinal variable as a single monotonic predictor so that we can identify a single effect given that there are three possible categories for reporting interactions with government officials (Very Little, A Handful of Conversations, Multiple In-Depth Conversations). This table’s results are quite similar to those of the treatment, providing additional validation that the treatment’s effect on connections happened through these conversations. The proportion mediated of the treatment for entrepreneurship intentions via general political connections is approximately 11 percent.

What the findings in Table 2 and Table 3 show is that the treatment is stronger in terms of increasing entrepreneurship intentions when its effect on political connections is taken into account. If the treatment did not move a respondent’s political connections, it would not have

Table 3: Pre/Post Indirect and Direct Effects of Government Interactions and Political Connections

Outcome	Mediator Type	Effect	5%	Median	95%
Intentions	General	Direct	0.043	0.107	0.167
Start Business	General	Direct	0.154	0.254	0.383
Intentions	Parliamentary	Direct	0.075	0.117	0.166
Start Business	Parliamentary	Direct	0.163	0.272	0.394
Intentions	General	Indirect	0.007	0.016	0.028
Start Business	General	Indirect	-0.006	0.005	0.016
Intentions	Parliamentary	Indirect	0.001	0.010	0.020
Start Business	Parliamentary	Indirect	-0.007	0.001	0.010
Intentions	General	Proportion Mediated	0.050	0.110	0.245
Start Business	General	Proportion Mediated	-0.018	0.016	0.057
Intentions	Parliamentary	Proportion Mediated	0.004	0.062	0.144
Start Business	Parliamentary	Proportion Mediated	-0.022	0.003	0.033

<sup>a</sup> Table shows the quantiles of the posterior distribution for mediation and treatment effects calculated as sample average marginal effects. Mediators are the 1 to 10 scales for both general political connections and parliamentary connections (obtaining a meeting with an MP). It should be noted that the Tunisian field experiment did not include the parliamentary connections measure and thus the total effects are not equivalent for both types of connections.

had as strong an effect on increasing the subjects' interest in entrepreneurship. This finding would appear to support the contention that political connections are related to entrepreneurship choice as we found in our conjoint experiment. They also provide realistic bounds for the treatment effect: we did not expect political connections to increase dramatically, nor did we think that the all of the effect of the treatment would be mediated by connections. The knowledge transfer about how to be an entrepreneur should have a greater effect on expressed interest in entrepreneurship than a meeting with a government official, even if that official was knowledgeable about policy issues related to entrepreneurship.

#### 4.3.1 Long-Term Effects

We next consider the effects of the treatment from endline surveys conducted one year following treatment in Tunisia and six months later in Senegal. In this analysis, we use the online panel



surveys to measure outcomes and connections rather than the pre/post treatment surveys for all respondents. In total, we received 287 completed endline responses in Tunisia and 453 in Senegal, providing us with considerable power even at endline. Furthermore, our ability to use within-subject inference increases the power of this outcome even with some attrition.

Figure 10 examines the same outcomes as Figure 9 but with the endline and baseline survey data, including both with and without the placebo arm. As can be seen, even after considerable time the treatment had a noticeable effect both in terms of increasing entrepreneurship intentions and in encouraging respondents to attempt to start a business. We consider this long-term nature of the effect to be remarkable, especially as evidence for the durability of entrepreneurship training is mixed (Quinn and Woodruff 2019).

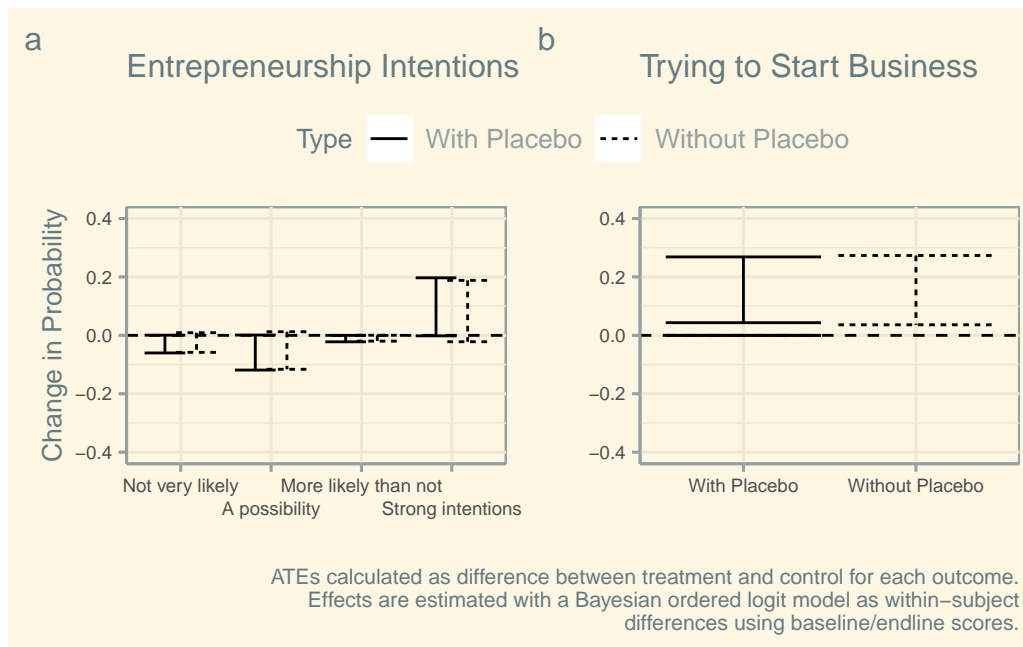


Figure 10: Endline Treatment Effect on Entrepreneurship Intentions and Activity

However, when we look for mediation of political connections in the endline survey, we do not find that the treatment has a positive effect on political connections at this late stage. Table 4 shows the direct and indirect effects of the treatment (excluding the placebo arm) on the outcomes. While the direct effects are still present and even quite large—more precise than

the total effects reported in Figure 10 –the uncertainty intervals indirect effects all include zero. There is a substantively large and positive indirect effect of general connections on starting businesses, but again there is some probability mass on negative values in the 5% to 95% posterior interval. We do observe a very high proportion of the treatment that is mediated by connections on starting businesses—especially for parliamentary connections—because the indirect effects are imprecisely estimated, we cannot conclude that the treatment is driving that mediation. Indeed, after six months to a year it would be surprising to observe persistent effects on connections from a workshop with a government official even if the workshop provided useful information and some direct access.

Table 4: Endline Indirect and Direct Effects of Treatment and Political Connections

Outcome	Mediator Type	Effect	5%	Median	95%
Intentions	General	Direct	0.001	0.116	0.225
Start Business	General	Direct	-0.014	0.107	0.223
Intentions	Parliamentary	Direct	-0.121	-0.002	0.112
Start Business	Parliamentary	Direct	0.051	0.176	0.267
Intentions	General	Indirect	-0.014	0.003	0.021
Start Business	General	Indirect	-0.007	0.014	0.038
Intentions	Parliamentary	Indirect	-0.006	0.004	0.022
Start Business	Parliamentary	Indirect	-0.022	0.000	0.020
Intentions	General	Proportion Mediated	-0.168	0.022	0.172
Start Business	General	Proportion Mediated	-0.125	0.096	0.541
Intentions	Parliamentary	Proportion Mediated	-2.344	0.131	1.869
Start Business	Parliamentary	Proportion Mediated	0.012	0.135	0.347

<sup>a</sup> Table shows the quantiles of the posterior distribution for mediation and treatment effects calculated as sample average marginal effects. Mediators are the 1 to 10 scales for both general political connections and parliamentary connections (obtaining a meeting with an MP). It should be noted that the Tunisian field experiment did not include the parliamentary connections measure and thus the total effects are not equivalent for both types of connections.

We examine additional outcomes in Table 5 to understand how the increase in entrepreneurial activity and interest among the treatment group fared over time (again, excluding the placebo arm). While there are some intricacies in terms of how these outcomes may be related to the treatment given the amount of time that has elapsed, we do observe

that are more likely to both own and have quit businesses than the control group (though the former result is not statistically significant). There is also some evidence that the companies in the treatment group are somewhat less likely to have started paying salaries, which could be simply a function of firm age.

What is very interesting to note is that there are again very strong mediation effects of owning businesses and quitting businesses from political connections. Again, we cannot conclude that the treatment is still affecting these levels of connections, but it is clear that the success of the entrepreneurship training does depend on these levels of connections. Those with higher connections from baseline are more likely to both own businesses and have quit businesses, and there is some evidence that they employ fewer people while also being more likely to pay salaries (though is latter evidence is not statistically significant).

Table 5: Effect of Treatment on Business Success

Outcome	Type	5%	Median	95%
Employ People	Direct Effect	-24.200	-7.115	8.191
Employ People	Mediation General	-2.231	0.279	3.158
Employ People	Mediation Parliamentary	-5.413	-2.514	0.536
Own Business	Direct Effect	-0.012	0.106	0.245
Own Business	Mediation General	0.007	0.025	0.048
Own Business	Mediation Parliamentary	0.002	0.019	0.037
Pay Salary	Direct Effect	-0.371	-0.198	0.032
Pay Salary	Mediation General	-0.003	0.027	0.053
Pay Salary	Mediation Parliamentary	-0.006	0.029	0.064
Quit Business	Direct Effect	0.058	0.213	0.374
Quit Business	Mediation General	0.011	0.031	0.048
Quit Business	Mediation Parliamentary	0.018	0.041	0.064

<sup>a</sup> Table shows the quantiles of the posterior distribution for mediation and treatment effects calculated as sample average marginal effects. Mediators are the 1 to 10 scales for both general political connections and parliamentary connections (obtaining a meeting with an MP). It should be noted that the Tunisian field experiment did not include the parliamentary connections measure and thus the total effects are not equivalent for both types of connections.

As a result, while the effect of the treatment on political connections does not persist as

long as six months to a year, it is clear that the success of entrepreneurship efforts is strongly mediated by both general and parliamentary connections (and especially the latter).

#### **4.4 Mechanisms and Placebos**

Demonstrating this mediation effect is an important part of the puzzle concerning political connections and willingness to engage in entrepreneurship. We can go farther than this statistical form of evidence by examining details about the experiment designed to determine why some people became more or less connected. First, we consider a question we fielded that asked respondents how much they interacted with government officials. In Figure 11 we show the pre/post differences in general and parliamentary connections for those in the treatment groups (excluding the March 18th group that did not have a government official speaker). As can be seen, there is a clear increase in political connection scores for these groups even when accounting for pre-test connections scores, which suggests (though does not prove) that these conversations had a formative effect on subjects' perceived connections. Moving from having very few if any conversations with government officials to multiple conversations is associated with an increase in self-perceived connections of approximately 1.5 points on our scale of 1 to 10. This is a reasonable increase that we might expect from a meeting with a government official.

The reason we cannot conclude that these conversations were responsible for the political connections increase is that, as mentioned in the research design section, we cannot force subjects to engage in these conversations. There very well could be other baseline differences such as extroversion that might predispose some subjects to take advantage of these opportunities to obtain connections. Even so, it would still be true that the subjects would not have obtained connections without the opportunity provided by the experiment.

##### **4.4.1 Alternative Outcomes**

We next examine an additional pre-registered outcome that sheds light on another way the treatment may have influenced how our respondents perceived political connections. In Fig-

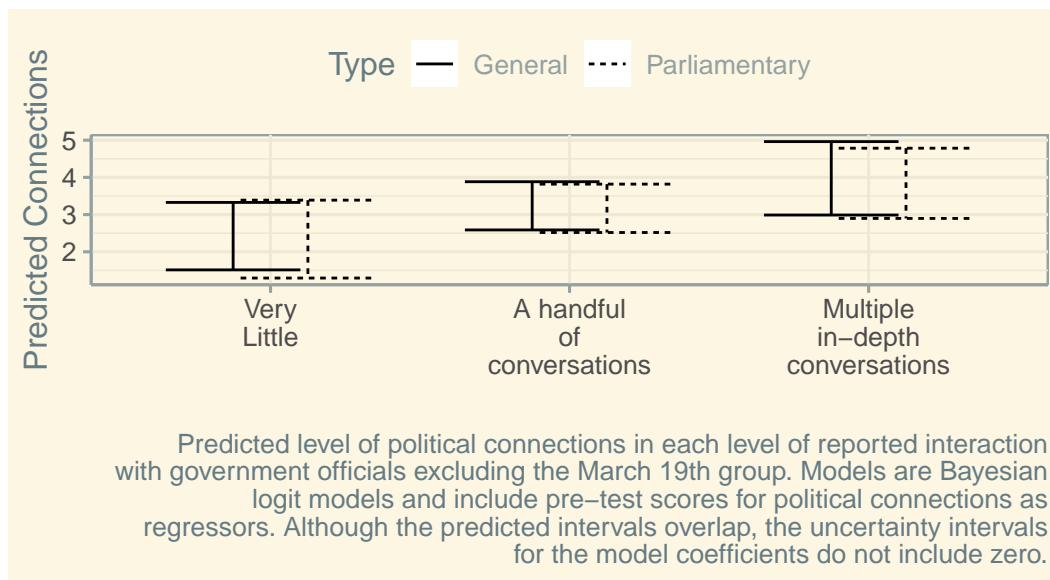


Figure 11: Predicted Level of Political Connections by Interactions with the Government Speakers (Excluding Placebo Treatment)

Figure 12, we show the  $ITT_W$  and  $ITT_{CS}$  estimates for an outcome that asked subjects whether they thought it was necessary for entrepreneurs to have connections in order to be successful. As the figure shows, our treatment reduced respondents' concerns that connections are necessary in order to succeed. We believe this effect is derived from the entrepreneurship training and conversations with entrepreneurs that stressed risk-taking, innovation, and hard work. Counterintuitively, this type of "empowerment speak" may have the side effect of causing young people to believe that connections are less important than their ingenuity and willingness to take big risks.

We do not know, of course, how this change in beliefs about connections could affect future behavior. We may be able to learn more as we study these young people in future years and their beliefs about the importance of connections.

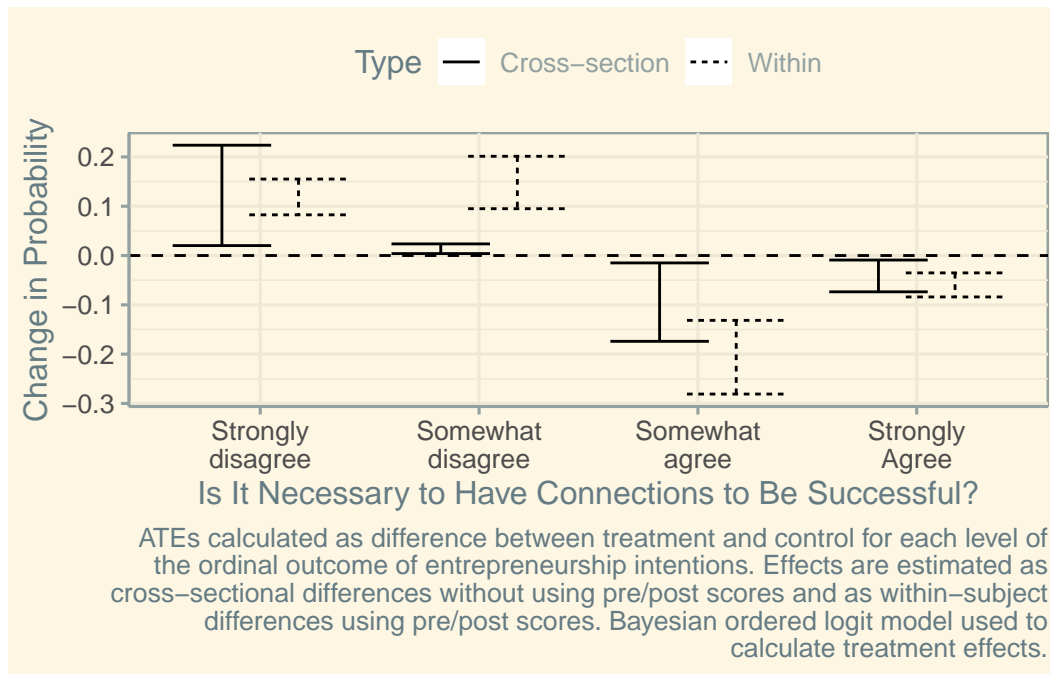


Figure 12: Treatment Effect on Whether It Is Necessary to Have Connections to be Successful

## 5 Conclusion

Our survey and experiment in Senegal and Tunisia reveal the critical nature of ties to government officials and political parties as barriers to youth pursuing careers in entrepreneurship. At the same time, our results suggest that there is much to learn about how young people might obtain connections. Our experimental treatment exposed them to a government official willing to discuss their future career plans and offer advice, but we see significant variation in terms of how much the respondents took advantage of this opportunity. Although the treatment's effect on political connections dissipated over time, reported connections remained a powerful mediator of the treatment at six months to a year following treatment.

Table 6: Mean Difference in Political Connection Scores by Treatment Group

Arm	General Connections	Parliamentary Connections
18th March, 2023	-0.120	0.320
19th March, 2023	-0.842	0.579
20th May, 2023	1.273	1.045
Control	0.000	NaN
22nd May, 2022	-0.188	0.846

<sup>a</sup> Table shows average differences in political connection scores by each treatment group from the pre- and post-surveys conducted immediately before and after the treatment. March 19th is the placebo treatment group that did not have government officials appear.

## 6 Appendix

Table 7: Mean Difference in Political Connection Scores by Government Interactions and Treatment Group

Arm	Interactions	General Connections	Parliamentary Connections
18th March, 2023	Very Little	-2.250	-1.750
18th March, 2023	A handful of conversations	0.000	1.077
18th March, 2023	Multiple in-depth conversations	0.750	0.125
<b>19th March, 2023</b>	<b>Very Little</b>	<b>-0.842</b>	<b>0.579</b>
20th May, 2023	Very Little	0.500	0.333
20th May, 2023	A handful of conversations	0.800	0.700
20th May, 2023	Multiple in-depth conversations	2.833	2.333
Control	Control	0.000	NaN
22nd May, 2022	Very Little	-0.333	0.667
22nd May, 2022	A handful of conversations	0.250	0.000
22nd May, 2022	Multiple in-depth conversations	-0.333	2.333

<sup>a</sup> Table shows average differences in political connection scores by each treatment group and level of interaction with government officials from the pre- and post-surveys conducted immediately before and after the treatment. March 19th is the placebo treatment group that did not have government officials appear.



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