Access Denied: How Bureaucratic Partisanship and Gender Bias Prevent Private-Sector Development[*]

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Abstract

Can bureaucratic bias generate inequality? The private sector is often promoted as a driver of equitable growth, yet bureaucratic discretion can determine who gains access to it. When public officials control key permits, private-sector opportunities may accrue to already privileged groups. I test how copartisanship and gender bias shape access to the private sector by creating a business in Senegal and conducting a field experiment that randomizes applicants' partisanship and gender during real permit applications at municipal councils. Copartisan applicants were substantially more permitted to submit their applications, while suggestive evidence indicates that women faced more roadblocks. These biases operated primarily during initial face-to-face interactions rather than in later stages of processing. Structured text analysis and qualitative data further identify the mechanisms underlying discrimination. The results demonstrate how political and social privileges shape access to economic opportunity and suggest that curbing bureaucratic discretion can promote more equitable development.

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1 Introduction

How does bureaucratic bias generate inequalities? "Street-level" bureaucrats often have leeway to meaningfully alter the lives of citizens (Brierley 2020; Lipsky 1980).[1] Their personal biases, which are well documented across the social sciences, can distort governmental outcomes ranging from service provision to regulatory enforcement (Nathan and White 2021; White, Nathan and Faller 2015; Wilson 1989). Yet we know far less about how bureaucratic discretion can quietly reproduce inequality—not through denying public services, but by deciding *who even gets a chance to compete*.

The private sector is one arena where such discretionary gatekeeping may generate inequalities, with exceptionally high stakes for development in the Global South. International organizations and domestic governments alike tout the importance of the private sector for sustainable economic growth (La Porta and Shleifer 2014; IFC 2015; UNDP 2020). The promise, it is argued, is that private markets expand opportunities to segments of the population beyond those historically favored by state-led development: that entrepreneurship enables broad participation in economic advancement.

Yet advancement in the private sector frequently requires government-controlled licenses and permits. In settings where the rule of law is weak or selectively enforced, bureaucratic discretion over these authorizations can determine *who* can participate in the economy at all (Brierley 2020; Brierley et al. 2023).[2] Because public officials control the permits necessary for entrepreneurs to operate, their personal biases can ultimately affect whether a business operates or not. These biases—whether subconscious or deliberate—can shape who succeeds in business and who is shut out. Such biases may advantage those with political and social privilege over the historically marginalized. Bureaucratic bias thus has the potential to reproduce political and social inequalities in the economy. Furthermore, if the benefits of private-sector development accrue primarily to already advantaged groups, the private sector may not serve as the engine for equitable and sustainable growth that policymakers claim. Quite the opposite, it could further entrench existing inequalities.

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[1] Per Lipsky (1980), street-level bureaucrats are those that deal with citizens directly to implement state policy.

[2] Work in the social sciences often treats formalization as the key barrier to entering the formal private sector (e.g., De Mel, McKenzie and Woodruff 2013; van Elk and de Kok 2014). While business registration has been historically difficult, reforms across the Global South have streamlined this step and made formalization substantially less onerous. Rather than eliminating barriers, I argue, these reforms have simply shifted where the political bottlenecks occur. The same roadblocks as before now lie in obtaining the licenses and permits required to operate, which rest at the discretion of frontline bureaucrats.

Two forms of bias are particularly consequential in this regard: partisan bias and gender bias. Both have far-reaching implications for markets, growth, and inequality. Existing research has demonstrated the value of partisan connections for businesses when the rule of law is not enforced equally (Bhandari 2022; Fisman 2001; Szakonyi 2018). Political networks lead to better access to credit (Khwaja and Mian 2005), preferential access to courts (Ang and Jia 2014), and ultimately result in more highly valued companies (Faccio 2006). Gender bias, meanwhile, has been shown to stymie women's advancement in the formal sectors of developing countries (Kabeer 2021; Lince 2011; Razavi 2012). Despite comprising a large share of the rising entrepreneurial class, women encounter higher barriers to entry and progression than men (Lyness and Thompson 2000).[3]

Research often takes as given that political and social privilege leads to preferential access to state institutions. Indeed, this is an assumption that grounds much of the literature on clientelism and cronyism: knowing the right people yields preferential personal outcomes, especially in business. Yet we know little about the magnitude of these biases or the precise stages at which they operate. Part of the reason is endogeneity: it is hard to separate political and social privilege from business outcomes because they are so deeply intertwined. It is unclear, for example, how partisanship and gender bias affect the obtaining of business permits because non-copartisans and women may simply be opting out of the private sector in the first place. Observational data thus risks understating the true impact of these biases because only the most connected and confident entrepreneurs may be subjecting themselves to the process.

To circumvent these issues and gain causal traction on the impact of partisanship and gender on private-sector entry, I conducted a field experiment in Senegal using a real business environment. Senegal provides a useful setting for this analysis in light of recent policies that have lowered the barriers of entry for registering firms, as well as the complex interaction of formal and informal influences in its business environment (Bhandari 2023). I registered a real sales firm and hired employees to apply for business permits at municipal councils in Dakar.[4] Municipal councils are especially relevant institutions in which to situate this work, as they hold substantial power in Dakar over business permits. Bureaucrats at these councils are critical gatekeepers to advancement in the private sector. These permits—for authorized construction and signage—are essential for new businesses seeking to operate publicly after formalization.[5] For each application, I randomized (a) the gender and (b) the partisanship of the applicant, with partisanship signaled by wearing the official polo of the ruling or opposition coalition, depending on which led the municipal council in question. To estimate the precise stage at which bureaucrats' biases operated, I tracked outcomes across the application and follow-up processes.

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[3] Whether due to hostile sexism or benevolent sexism (Glick and Fiske 2001), the end result is a status quo in which men have an easier time advancing in the private sector. These terms come from the literature on *ambivalent sexism* in social psychology. Hostile sexism captures antagonistic reactions toward women who enter or disrupt traditionally male spheres of power. Benevolent sexism involves seemingly supportive behaviors that nevertheless reassert traditional gender norms.

The results of the field experiment confirm that both partisanship and gender bias affect entrepreneurs' interactions with the state and their likelihood of success in securing necessary permits. The *magnitudes* of both treatments were substantial: copartisan applicants were more than 20 percentage points more likely than non-copartisans to be allowed to submit their applications for consideration, and male applicants were approximately 10 percentage points more likely than women to do so. The temporal *location* where these biases emerged was also revealing. They operated primarily during the initial application stage—the face-to-face, direct encounters between my employees and bureaucrats at municipal councils—rather than during the later back-office processing behind closed doors. Bureaucrats did not appear to discriminate once applications were submitted. Note, however, that compared to the statistically robust results for partisanship throughout, the results for gender are weakly significant and subject to model specification. In this context, partisan bias appears to be the stronger driver of private-sector success.

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[4] Throughout, I use "employee," "applicant," and "enumerator" interchangeably.

[5] While partisanship and gender biases could in theory also shape formalization itself, Senegal passed reforms in 2016 in response to international and domestic economic pressures that routinized registration and largely eliminated bureaucratic discretion at the formalization stage. I argue here that bureaucrats' biases now play a larger role *after* formalization, in the permitting stage. See Conclusion for a broader discussion of this implication.

To unpack the mechanisms behind these patterns, I use an exploratory mixed methods approach. First, I estimate outcomes during the application process that hint at the form bias took, such as the number of bureaucrats applicants were referred to and whether they were directed to male or female staff. Second, I analyze qualitative narratives that applicants wrote immediately after leaving the councils. I use structured text analysis to examine how treatment conditions differentially shaped the nature of interactions during the application process, and draw on the content of the narratives to parse the mechanisms. These narratives document each interaction in sequence—who they met, what was said, and how bureaucrats responded—providing unusually rich visibility into the mechanics of bureaucratic discretion.

The mechanism results show that bureaucrats exerted more effort and behaved more prosocially when interacting with copartisans and with men.[6] Rather than explicitly excluding out-group applicants, bureaucrats appeared to respond to prosocial and gendered expectations: they devoted more time and effort assisting applicants they related to, and less on those statistically less associated with the private sector.[7] Council staff went out of their way to connect copartisans to the right officials and facilitate progress in the application. By contrast, women encountered more dead ends and were more likely to be questioned or not taken seriously.[8] Excessive bureaucratic discretion thus creates space for political and social biases to shape who gains entry to the private sector, even when formal rules appear standardized. These patterns hold even after accounting for other in-group motivators, including coethnicity, coreligion, and cogender.

By demonstrating how bureaucratic bias shapes inequalities in access to economic opportunity, this study makes several contributions. First, it provides rare experimental evidence from a real-world setting that unsupervised bureaucrats generate inequalities that extend well beyond the public institutions they control. When bureaucrats act as gatekeepers to the private sector, their political leanings and social biases are not inconsequential; they produce distributional consequences with material stakes in the economy.

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[6] Prosocial behavior is defined as "a broad category of acts that are defined by some significant segment of society and/or one's social group as generally beneficial to other people" (Penner et al. 2005, 366). It includes cooperating and helping because of intrinsic motivation.

[7] This is in line with *taste-based discrimination* and *statistical discrimination*, which are defined in Section 2.3. See Altonji and Blank (1999) for a rich overview.

[8] Dealing with female bureaucrats did not improve outcomes for female applicants, as Section 5.3 shows.

Second, this paper helps to identify *where* such biases operate. It shows that discretion during routine, face-to-face interactions can stymie economic development even without more overt forms of discrimination at later stages. Even in what could be considered an "optimistic" case—bureaucrats do not appear to be maliciously excluding marginalized citizens behind closed doors—the consequences are significant. These seemingly small frictions can segment markets, introduce inefficiencies, and undermine the prospect of equitable development.

Third, the findings complicate dominant narratives that portray private-sector growth and entrepreneurship as inherently inclusive engines of development. If access itself is shaped by status and connection, then policies promoting formalization and entrepreneurship without addressing underlying inequities may inadvertently reproduce them. Furthermore, a large literature focuses on encouraging firms to formalize, yet doing so misses much of the locus of bureaucratic bias. The permits and licenses that determine whether one can begin working in the private sector—processes managed entirely at the discretion of bureaucrats—are the chokepoints now that formalization itself has been streamlined in many developing economies. Promoting equitable development requires addressing these discretionary pathways for discrimination.

2 Theory

2.1 Partisanship and the private sector under bureaucratic discretion

While politicians attempt to exert control over their agents—creating well-documented agency problems in politician-bureaucrat relations (Brierley et al. 2023; Golden 2003; Hassan 2020)—in many cases the problem runs in the opposite direction. In developing countries, the state often lacks the capacity or incentive to effectively monitor its bureaucrats.[9] On the capacity side, weak states lack the administrative infrastructure, training systems, and oversight technology necessary to constrain bureaucratic discretion. This produces unprofessionalized bureaucracies in which outcomes depend as much on individual officials' preferences or personalities as on codified rules (Lipsky 1980; Olken 2007; Pritchett and Woolcock 2004).

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[9] Bureaucratic discretion is not unique to developing countries, of course; a foundational literature in American politics explores the dynamics of bureaucratic agency problems (e.g., Calvert, McCubbins and Weingast 1989; Epstein and O'Halloran 1994).

The incentive problem compounds the capacity one. Bureaucratic appointments in many developing countries serve as political rewards in patron-client networks (Arriola 2009; Grindle 2012; Kimenyi 2006; Van de Walle 2007). Because such officials owe their positions to political favor rather than professional merit, politicians have little interest in ensuring rule-bound behavior. The very logic of their appointment implies that bureaucratic authority carries a non-legal, partisan quality.

In such environments, bureaucrats exercise substantial discretion over who gains access to the state (Slough 2024). The implementation of policy varies widely across administrative units. What is permissible in one office may be prohibited next door, creating a patchwork of informal procedures (McDonnell 2020). High rates of bureaucratic turnover—common in clientelistic systems where employment is allocated as a reward for loyalty rather than merit—further weaken institutional memory and procedural consistency (Grindle 2012; Hassan 2020). Decisions about licenses, permits, or other administrative authorizations thus do not become routinized or mechanical. Instead, they reflect the preferences and whims of officials operating in systems of selective enforcement, where laws are applied unevenly across citizens (Holland 2016). When bureaucrats themselves are appointed along partisan lines, this discretion can translate into systematic favoritism toward copartisans. This is especially true of low-level bureaucrats (Brierley 2021), precisely the kind who supervise basic administrative procedures like licenses.

Research in American politics has long shown the pervasive influence of partisanship (Bartels 2000, 2002), including in bureaucratic responsiveness (Porter and Rogowski 2018). Though research on ideological partisanship is scarcer in sub-Saharan Africa, studies of ethnic favoritism point to similar mechanisms. In many African settings, scholars proxy for partisanship using ethnicity, as ethnic groupings often capture underlying political alignments and social identities (Fearon 2003; Ekeh 1975; Posner 2004). Biases of this sort have tangible effects on private-sector outcomes. In Ghana, for example, Michelitch (2015) finds that businesspeople give preferential rates to copartisans during periods of heightened political awareness.

Partisan favoritism is closely related to the broader literature on political connections in the Global South, which shows how alignment with ruling parties enhances firms' access to contracts, credit, and regulatory forbearance (Fisman 2001; Frye 2004; Khwaja and Mian 2005). Specific to bureaucracy, Szakonyi (2018) provides evidence from Russia demonstrating that political connections can increase access to bureaucrats. While political connections offer more direct access than copartisanship, some of the mechanisms by which they operate are similar: namely, that political familiarity eases access when rule of law is selectively enforced.

These literatures combine to suggest that when bureaucrats enjoy high discretion and low oversight, and where partisan identity structures political competition, copartisan applicants should enjoy privileged access to state-controlled economic opportunities.

Hypothesis 1. *Copartisanship increases access to bureaucratically controlled business permits.*

2.2 Gender bias and access to the private sector

Research has established that members of marginalized groups are subject to the biases of bureaucrats. Slough (2024) finds, for example, that bureaucrats in Colombia neglect lower-class citizens seeking to access social welfare programs. Meanwhile, in the United States, White, Nathan and Faller (2015) find that street-level bureaucrats are less responsive to information requests from Latino constituents and expend less effort on their behalf.

In highly patriarchal societies where gender biases permeate formal and informal institutions, women constitute a marginalized group. In Africa, a rich literature documents the impact of gendered norms on political outcomes. On the demand side, gendered expectations can affect political participation (Ahikire 2004; Tripp 2006), representation and engagement (Barnes and Burchard 2013; Burnet 2011; Clayton 2015), and voters' preferences and evaluations of candidates (Clayton et al. 2020; Gottlieb, Grossman and Robinson 2018). On the supply side, an experimental literature finds that politicians are often more responsive to men than to women, with the magnitude of this bias varying across contexts (Dhima 2022; Kim and Michelitch 2021; Magni and de Leon 2021).

These biases are likely to extend to bureaucratic decision-making processes that shape the composition of the private sector. However, causal evidence of gender bias in the private sector, as Bertrand and Duflo (2017) argue, remains relatively scarce in comparative political economy.

Audit experiments in political economy reveal discrimination against women in labor markets (Booth and Leigh 2010; Carlsson 2011; Riach and Rich 2002), but far less work examines whether gender affects access to *state-facilitated* opportunities for private-sector development. This is a likely possibility, as we know that gendered hierarchies are likely to shape bureaucratic decision-making in areas where discretion is greater (Durose and Lowndes 2024). Especially in highly patriarchal societies, bureaucrats' personal beliefs about women's roles can influence whether female entrepreneurs are taken seriously, treated respectfully, or granted equal procedural access.[10]

Whether driven by explicit discrimination or implicit gender stereotypes (Eagly and Karau 2002; Glick and Fiske 2001), there is ample evidence that gender bias extends into the private sector more broadly (Brock and De Haas 2019; Fay and Williams 1993; Fielden, Dawe and Woolnough 2006; Trentham and Larwood 1998; Salman, El Abboubi and Henda 2012). In many settings, including Senegal, women make up only a small share of the formal sector (International Labour Organization 2020), report facing systematic barriers in pay, advancement, and professional recognition (Blau and Kahn 2017), and have more limited property rights (Hallward-Driemeier and Hasan 2012). They are also subject to harassment and questioning of their credentials and legitimacy at higher rates (McLaughlin, Uggen and Blackstone 2012). Gendered business norms, in short, are pervasive and present in the background when women seek bureaucratically issued business permits. High administrative discretion and weak oversight heighten opportunities for bias to affect outcomes.

Taken together, existing theory and evidence suggest that when bureaucrats exercise high discretion under selectively enforced rule of law, women are likely to face barriers in accessing the formal requisites of entrepreneurship.

Hypothesis 2. *Women are less likely than men to obtain bureaucratically controlled business permits.*[11]

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[10] Note, however, that even in highly patriarchal societies, women frequently succeed in business, though this success tends to concentrate in the *informal economy*—where women are overrepresented (Chen 2001)—rather than in the formal economy (Meagher 2011). The present article examines access to formal, state-regulated opportunities that are increasingly the focus of international and domestic policymakers seeking to develop the private sector.

[11] Hypothesis 1 and 2 are pre-registered as one-sided tests.

2.3 Types of discrimination

Having established that partisanship and gender bias likely shape access to the private sector when bureaucrats act as gatekeepers, a natural question arises: *Why* would bureaucrats discriminate? What motivates them to treat applicants differently based on partisan identity or gender? Although discrimination can take many forms (Onuchic 2022; Romei and Ruggieri 2014), I outline four likely contenders from the political economy literature that can explain bureaucratic behavior.[12]

First is *taste-based discrimination*. Under this form of discrimination, bureaucrats might simply prefer interacting with certain groups over others (Becker 1957). They have a "taste" for—and derive utility from—dealing with politically or socially favored individuals. Out-group members, by contrast, must compensate for the negative utility they create for officials. Bureaucrats may prefer spending time with in-group members, which can manifest in granting favors, devoting more time and effort, or acting friendlier with these individuals (Brewer and Kramer 1985). Sexism is often categorized within this family of mechanisms: when bureaucrats dismiss or undervalue women regardless of competence, it reflects a distaste for the group rather than a rational assessment of productivity. In contexts where women are marginalized, bureaucrats may exhibit a general distaste for women in positions of power because it goes against gendered expectations for behavior (Clayton et al. 2020). "Political homophily," the tendency to gravitate toward and favor copartisans, also fits within this category (Zuckerman 2005).

Second, bureaucrats might be acting upon *statistical discrimination* (Arrow 1972; Phelps 1972). Rather than attaching positive utility to the interaction, bureaucrats here might be using group membership as a heuristic for unobservable traits. For example, if female applicants are rare in the formal sector, bureaucrats may infer that they are less experienced or less likely to succeed, and thus might adjust their effort accordingly.[13] Alternatively, bureaucrats may assume that women are less likely to complain or demand accountability due to gender norms, and thus allocate less effort to their application than they would to a man's (Slough 2024). In both cases, group-based generalizations guide bureaucrats' decisions under limited information, attention, or capacity.

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[12] For an excellent overview, see Michelitch (2015).

[13] The opposite could also be true: *because* female applicants are so rare, women might signal high capability and experience.

Third, bureaucrats' discrimination could stem from *other-regarding preferences*. Individuals may attach positive utility to the welfare of their group and negative or no utility to the welfare of out-groups (Tajfel 1970). While this mechanism has typically been studied in the context of ethnicity and public service provision in Africa (Habyarimana et al. 2007), it could also apply to partisanship or gender. For instance, a female bureaucrat might favor a female applicant out of solidarity (especially if female applicants are rare), or a partisan bureaucrat might privilege copartisan applicants to advance the welfare of the party. At the same time, the partisan bureaucrat may gain utility from excluding a non-copartisan, especially when access to permits is limited or zero-sum.

Fourth, *norm-based discrimination* could influence bureaucrats. This type of discrimination does not depend on utility maximization but rather on conformity to deep-seated social norms and expectations (Michelitch 2015). Entrenched gender norms, for example, may produce reflexive bias toward women even when bureaucrats do not consciously intend to discriminate (Cikara and Van Bavel 2014). This relates to role congruity theory, which proposes that incongruities between gender roles and leadership roles (in this case, in business) can lead to biases against women in practice (Eagly and Karau 2002). Such biases are often internalized through socialization and may manifest in variation in bureaucrats' application of rules and procedures.

Across these forms of discrimination, the common thread is that bureaucrats use mental shortcuts based on identifiable features when allocating their scarce time and effort. These heuristics are especially influential in low-capacity contexts where bureaucrats have limited time to help everyone that requests it. Importantly, the operative bias need not be the same for partisanship and gender bias, and multiple forms can operate simultaneously. For example, statistical discrimination and norm-based heuristics may shape gendered outcomes, while other-regarding or taste-based discrimination may drive partisan favoritism.

In this paper, I adopt a mixed-methods approach to explore which mechanisms are most consistent with observed patterns of bias. I leverage both the experimental design and qualitative data to assess the form that discriminatory behavior takes. These analyses are exploratory but serve as an essential first step toward identifying the impact of bureaucratic discrimination on private-sector development in weakly institutionalized settings.

3 Context

3.1 Business, partisanship, and gender in Senegal

Senegal represents an apt context in which to examine questions related to informal channels of bias in a formal business environment. Like many other low-income countries, Senegal has championed reforms to ease entry into the private sector in recent years. The registration process has been simplified into a "one-stop-shop" process (Gainer, Chan and Skoet 2016), and the government has introduced a favorable tax regime for firms that formalize (Gottlieb 2024). There has been a sizable increase in newly registered firms as a result, and, compared to the private sector of Senegal's past, more and more entrepreneurs without political and social privilege are entering the formal economy. There is thus a growing class of entrepreneurs who seek access to the permits and licenses required to advance in Senegal's private sector.

Despite the growth in Senegal's formal economy, informal influences continue to abound, and there is a widespread perception that state institutions do not work for all entrepreneurs equally. Citizens in Dakar report relatively low trust in the bureaucratic institutions undergirding private-sector enforcement—including courts, police, and councils—especially when political privilege is at play (Bhandari 2022). Entrepreneurs believe that political connections are necessary to advance in business, and that they can make or break success in the private sector (Kubinec et al. 2024).

Partisanship in Senegal is relatively high compared to other sub-Saharan African countries (Mattes and Krönke 2020), yet the fluid nature of its party system—and the coalitions that unite to stand election—leads to significant variation in the party controlling particular administrative units over time (Kelly 2020). Senegal is also a stable democracy, especially relative to the region, and it has experienced substantial institutional continuity despite occasional political threats from its hyperpresidential system (Thomas and Sissokho 2005). In short, Senegal is a place where partisanship matters and where power is consistently fluctuating, creating variation in who is the political in-group and who is the political out-group over time.

Social influences are also important in Senegal, including in its private sector, where they have been shown to affect economic behavior (Bhandari 2023). This includes gender, as women constitute a historically marginalized group in Senegalese politics and business. Gender-based expectations of women in power have been extensively documented in Senegal, where such norms result in weak political representation (despite gender quotas), lower wages and offers of formal employment, and limited corporate representation (Diallo 2025; Fabry, Van den Broeck and Maertens 2022; Koopman 2009; Mackintosh et al. 1989; Ndour and Kante 2025). In a recent Afrobarometer survey round, one-third of Senegalese respondents stated they agree or strongly agree that "men make better political leaders than women, and should be elected rather than women" (Afrobarometer 2023). In the business world, women make up only a small share of the formal sector in Senegal and report facing systematic barriers in pay, advancement, and professional recognition (Dia et al. 2018; Sarr and Fall 2021; Simen and Diouf 2013; Sylla 2009). Overall, these patterns suggest an environment in which gendered norms continue to constrain women's participation in the formal economy and where bureaucratic discretion could amplify existing inequalities.

3.2 Business permits and the bureaucrats who control them

Though the process of registering a business in Senegal has been simplified in recent years, firm owners must still obtain a series of post-formalization permits to truly launch their business operations. These permits are not centralized in any one institution. They are administered by local councils, urban services departments, the Chamber of Commerce, the tax department, and prefectures, among others. Business owners seek permits such as approvals for construction, for establishing the address of one's firm, authorizations to apply for public procurement contracts, and import-export licenses, among many others. As in other developing countries (e.g. De Soto 2000), navigating Senegal's dense and fragmented bureaucracy can be a difficult task involving numerous steps for new firm owners, a task made more arduous due to the procedural formalization inherited from French colonial administrative institutions (Joireman 2001; Kondylis and Stein 2023).

This project focuses on two permits that most firms in Senegal must secure early in their business activities: the occupation permit (*permis d'occuper*) and the public signage permit (*autorisation pour poser de panneaux*). The occupation permit grants the right to legally occupy state-owned land, upon which the vast majority of businesses in Dakar operate.[14] It is thus required for any business intending to operate a storefront, as operating without one exposes businesses to fines, closure, or potential seizure of assets. The public signage permit authorizes businesses to display signs or advertisements on public property. For firms seeking visibility and growth, this permit is essential. Without it, business signs are often removed and owners fined or reprimanded.

Both permits fall under the authority of municipal councils (*mairies*, or town halls) in Dakar. Councils are an ideal institutional focus for this study because they play a central role in local development, are numerous enough to support an experiment, and share broadly comparable organizational structures.[15] Councils are also the level of government with which most Senegalese citizens are likely to interact, including new business owners. Dakar is subdivided into 19 communes d'arrondissement, each with its own council, and its densely populated suburbs of Guédiawaye and Pikine constitute an additional 21 communes d'arrondissement and councils. In addition to the two permits examined here, councils manage construction permits, communal market permits, and permits related to the physical operation of businesses.

Permit-granting positions within councils are often staffed by patronage-linked political appointees, thus heightening opportunities for political and social biases to influence decision-making. The political nature of these jobs also contributes to high staff turnover, producing ambiguity and inconsistency in permit-granting procedures both across and even within councils. For example, the national government's website lists an official occupation permit fee of 2,000 FCFA (approximately 3 USD),[16] yet applicants in this study were quoted prices as high as

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[14] In Senegal, 95% of land belongs to the state (Boone 2014).

[15] The staff at councils across Dakar perform relatively similar roles and occupy similar positions and offices. Comparing a permit process at a council to, for example, the Chamber of Commerce, would introduce other confounding variables that may cloud inferences.

[16] See `https://senegalservices.sn/demarche/demander-un-permis-doccuper`

300,000 FCFA. Within the same council, one applicant was quoted 30,000 FCFA for the permit while another was quoted 50,000 FCFA.

Administrative instability was particularly acute during the period of fieldwork (spring 2022), following local elections held only months earlier.[17] Two major political coalitions contested these elections: Benno Bokk Yakaar (BBY) and Yewwi Askan Wi (YAW). While BBY, headed by Senegal's then-president, Macky Sall, fared well in much of rural Senegal, it lost key urban strongholds in Dakar and Ziguinchor, including the mayorship of Dakar itself. Of the 40 councils in this project's sample, 24 were controlled by YAW and 16 by BBY.[18] Such frequent political turnover and decentralization create an environment in which bureaucratic discretion is both extensive and consequential. Permit officers—often new to their posts—must navigate loosely codified rules and competing political pressures, conditions ripe for bias to shape how citizens access the formal economy.

4 Research design

To measure the impact of partisan and gender biases on access to permits, I implemented a field experiment in Senegal in 2022. I hired employees to apply for real business permits, randomly varying the applicants' gender and a visible signal of political partisanship.

4.1 Business

The field experiment relied on a business that I created and legally registered in Senegal. The business, called *Porte-à-Porte Sénégal* (PAPS for short) was established solely for research purposes at APIX, home to the country's "one-stop shop" for business registration. It received an official business identification number (NINEA, *Numéro d'Identification National des Entreprises et des Associations*), which serves as formal proof of registration. Although PAPS is not a profit-making endeavor, it appeared indistinguishable from an ordinary small business. No bureaucrats contacted the firm to question its authenticity, and all town halls were informed about the project only after data collection concluded.

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[17] While newly elected staff might be particularly susceptible to bias given their lack of experience—or, conversely, lack the experience to discriminate systematically—high turnover itself is characteristic of Senegal's councils and thus an important context for understanding bureaucratic bias.

[18] Interestingly, party switching (*"transhumance politique"*) occasionally occurs after local elections in Senegal. The mayors of Fann–Point E–Amitié and Médina switched affiliations shortly after election, moves widely interpreted as patronage-driven.

Using a real business to test this paper's theory offers several advantages. First, it enables causal estimation in a substantive area where, to date, observational research has been clouded by inference problems: formal sector entry and permit applications are endogenous to political and social networks. Second, it allows testing the hypotheses in a realistic environment among employees who had never previously applied for permits, mirroring the experiences of new entrepreneurs in Senegal's expanding private sector. Finally, it captures the behavior of the actual bureaucrats who control these valuable permits, facilitating direct observation of the bureaucratic gatekeepers to Senegal's private sector.

4.2 Ethical considerations

Though creating a business to test longstanding research questions offers unprecedented researcher control, it also raises important ethical considerations. These include the potential use of limited bureaucratic resources and the displacement of existing entrepreneurial activity. I took several steps to minimize these risks.

First, enumerators verified that municipal councils were not overloaded with applications, and, if council staff asked applicants to return later, enumerators were instructed to agree. However, the median application required about 71 minutes, much of which was spent waiting rather than consuming bureaucrats' time. Second, I limited the number of application days and the total sample size to the minimum required for statistical power. Of the thousands of applications councils process annually in a city with a metropolitan population of 4 million, this project accounted for 159. Third, I partnered with a respected civil society organization in Senegal to ensure that results were shared with local stakeholders to maximize the likelihood that results are used to inform policy to increase equitable access to the private sector. Fourth, all councils were debriefed after data collection and invited to a dissemination event in Dakar during which I presented the results to government, civil society, and academic audiences. Finally, in all published data, council names will be anonymized to prevent any application from being linked to a specific institution.

Table 1: Treatment strength check: Staff mentioned politics to applicants who signaled their partisanship

	Outcome: Staff spoke to applicant about politics
Copartisan applicant	0.456^{***} (0.046)
Control group mean	0.025
Control group std. dev.	0.157
Observations	159

Notes: Specification estimated using OLS. The outcome for the model is a response in the self-administered survey that employees completed after the permit application process, a binary indicator of whether any staff at the council spoke to the applicant about politics. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from a two-sided test.

4.3 Treatments

The experiment included two treatment arms. In one, I randomized the gender of the employee who applied for the permit. In the other, I randomized a signal of political partisanship by providing each employee with polos—one bearing the logo of the ruling coalition and one of the opposition coalition.[19] Given sample-size constraints and the goal of maximizing power, applicants wore the shirt corresponding to the party controlling the council, thereby signaling copartisanship to bureaucrats.[20] Political apparel of this kind is common in Senegal, where supporters often wear party-branded clothing in public settings.[21]

To test whether bureaucrats noticed the political signal, I measured whether applicants wearing copartisan polos were more likely to be engaged in political conversation. As shown in Table 1, copartisan applicants were 45 percentage points more likely to be asked about politics by staff than were control applicants, suggesting that the copartisan signal indeed made an impact.

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[19] I worked with a designer in Senegal to replicate the official polos of each coalition.

[20] Future work could examine discrimination against out-party applicants.

[21] Indeed, political clothing is common across many African contexts (Allman 2004).

4.4 Randomization and data collection

The copartisan treatment was block-randomized by council and permit type. Each of the 40 municipal councils received four permit applications (two permit types × two political conditions [copartisan and control]). The gender treatment was block-randomized by council.[22] In total, the design targeted 160 observations.[23] Eight men and seven women constituted the PAPS employee team, selected to maximize ethnic and religious diversity. This enables testing whether the pro-social, in-group effects theorized to drive numerous outcomes in sub-Saharan Africa moderate treatment effects.

There were two primary stages of data collection. The first stage was the application process itself, which consisted of employees visiting councils to submit permit applications. Some applications required multiple visits. Once an application was either deposited or definitively rejected, the process was considered complete.[24] The second stage of data collection was the follow-up process. Starting one week after submission, follow-up calls to councils were made weekly to check application status until an outcome was reached.

4.5 Measurement of outcomes

The research design enables measuring bureaucratic bias at different stages of the application process. I distinguish between outcomes during the application stage (face-to-face interactions) and those during the follow-up stage (back-office processing).

During the application process, I first measure the overall duration of the visit in minutes. Longer visits could indicate greater bureaucratic hurdles on the one hand, or higher bureaucratic effort on the other; I use supplemental measures recorded by employees to distinguish between these two options. Second, I measure the number of visits required to submit the application. In some cases, applicants were told to return later, even after waiting for several hours. This is a common bureaucratic barrier in Senegal that privileged applicants may circumvent. Finally, as the key outcome of the application stage, I measure whether the council accepted and stamped the application for review. To ensure that enumerators' approaches were standardized and consistent, we conducted extensive training and piloting, recorded randomized audio audits of the application process, and carried out post-project phone calls with the bureaucrats themselves. Together, these steps help demonstrate consistency across the team.

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[22] The gender treatment was not blocked by permit type as I sought to ensure that within councils, some pairs of men and women applied for the same permit type, allowing within-permit estimation of copartisanship, holding gender constant.

[23] Due to an implementation error, the final sample consists of 159 applications.

[24] Rejection was defined as explicit verbal confirmation that the process could not proceed.

I measured two outcomes for the second stage of data collection. First was the number of follow-up calls required to obtain a final status of the application. The second and most important outcome was advancement to the final stage of the process. This is a binary indicator for whether council staff scheduled a site inspection—an almost certain precursor to final approval. This stage marks the experiment's endpoint; stopping here avoided making payments to bureaucrats, a worry since it became clear that some were inflating the fees they quoted to include bribes.[25]

Throughout, I also recorded variables to help parse how bureaucratic biases shaped outcomes. This included waiting time before assistance, the number of officials spoken to, total fees quoted, whether any staff applicants interacted with were women, and applicants' subjective assessments of bureaucrats' professionalism and availability. Further, I collected rich descriptive data. Employees completed self-administered surveys immediately after exiting councils and wrote lengthy narratives that describe every step of the application process, including every interaction with council staff and its content. I make use of this data to qualitatively trace the mechanisms behind observed biases.

4.6 Estimation

I estimate average treatment effects (ATEs) with the following OLS specification:

$$y_i = \alpha + \beta_1 treatment_i + \eta_m + \theta_p + \epsilon_i$$

where y is the outcome of interest, *treatment* is a binary indicator for the copartisan or gender treatment (coded 1 for women), η_m are council block fixed effects, and θ_p are permit-type block fixed effects. Heterogeneous treatment effects are estimated by interacting treatment terms with relevant covariates. Throughout, I conduct one-tailed tests for pre-registered hypotheses and two-sided tests for exploratory analyses.

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[25] Furthermore, by stopping before final approval, we ensured that future entrepreneurs would be able to claim the sites for their own businesses. The study thus avoided influencing actual land or signage allocation.

Because the results could in theory be influenced by particularly strong or weak enumerators, I show in Appendix B that the results replicate with the inclusion of enumerator fixed effects and are robust to tests that reestimate results by excluding one enumerator at a time.[26] Furthermore, because the application process is two-stage, inferences regarding the follow-up outcomes as distinct from the application-stage outcomes may be affected by post-treatment selection, even if the total effect of treatment on permit approval remains causally identified (Slough 2023). I address this issue using the TRACE estimand (Hazlett, McMurry and Shinkre 2025) in Appendix E.

5 Results

5.1 The effects of copartisanship and gender on permit applications

Figure 1 presents the ATEs for both treatments.

Starting with copartisanship, the results reflect an application process that favors copartisan applicants. While there were no effects of copartisanship on the duration of the visit, the results indicate that copartisans enjoyed a smoother application process overall. Relative to the control group, copartisan applicants required fewer visits in order to submit their applications, and, most importantly, were able to successfully deposit their applications at a much higher rate (0.5 standard deviations more likely than control, which corresponds to approximately 20 percentage points). Because submission is a necessary precondition for approval, these results imply a meaningful advantage for copartisans when bureaucrats exercise discretion.

In the follow-up stage, copartisans placed a similar number of calls as the control group, but they were more likely to advance to the final stage of the process (equivalent to conditional approval). However, because the advancement effect is smaller than the submission effect,

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[26] As shown in the results below, the findings on partisanship are more robust to specification checks than the gender results. Accordingly, the gender results should be interpreted as suggestive evidence.

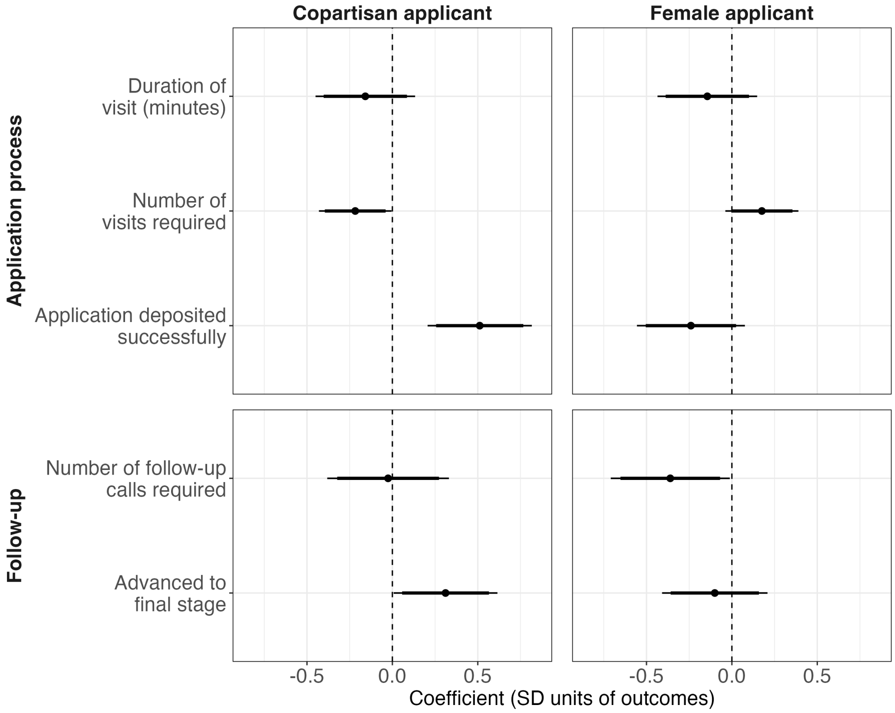

Figure 1: Average treatment Effects: Partisan and gender treatments

Notes: This figure plots estimates for outcomes collected during the permit application and follow-up processes. Outcomes are standardized. Models are estimated using OLS and include fixed effects for randomization blocks. Whiskers display 95% and 90% confidence intervals. The corresponding table output is in Appendix Table A1 and A3, and the results with non-standardized outcomes are in Appendix Table A2 and A4.

the pattern is consistent with advantages concentrated at the application stage rather than during back-end processing. If bias operated mainly during processing, we would expect an advancement effect at least as large as the submission effect.

Figure 1 also reports ATEs for the gender treatment. A consistent picture emerges of the disproportionate barriers facing women in accessing the private sector in Senegal. Despite following the same procedures as male applicants,[27] women had more steps in the application process, needing more visits to councils to complete submission attempts. Importantly, women's applications were rejected outright at higher rates than men; women were less likely to be able to successfully deposit their applications (a 9.7 percentage point decrease, significant at the $\alpha = 0.10$ level). Because these results are statistically fragile, they should be interpreted with caution. While there are process-level disparities in initial bureaucratic encounters for women, the findings fall short of providing clear evidence of women's exclusion from the private sector.

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[27] Significant time was spent during training to ensure that application procedures were standardized across the team. Enumerators' written narratives after submission confirm that practices were uniform.

The follow-up process similarly reflects few gender inequities: women's applications actually required fewer follow-up calls, and, more importantly, advancement to the final stage of the process did not differ by applicant gender. Similar to the results for copartisanship, this pattern indicates that bias primarily manifested during face-to-face encounters rather than in the final processing stage. Once an application was submitted—a hurdle, to be sure—it appears that applications submitted by men and women were treated similarly. Nonetheless, the suggestive evidence of higher outright rejection at the application stage suggests that, in aggregate, women might ultimately secure permits at lower rates.

5.2 Examination of mechanisms

The findings above point to bias during face-to-face interactions rather than during back-office processing. To examine how differential responses during permit applications arose, I conduct an exploratory, mixed-methods analysis.

I first assess intermediate outcomes collected during applications. The results, shown in Figure 2, show key contrasts in the application process between copartisans and noncopartisans and between men and women. Copartisan applicants spoke with more staff members at councils, while women spoke with fewer. On the surface, it may seem beneficial to speak to fewer people, as this might imply a more efficient application process with less bureaucratic runaround. But in newly populated administrative units where staff are still learning their responsibilities—like these municipal councils post-election—more contact is more likely reflective of effortful shepherding toward the appropriate office. Especially in Senegal, where interpersonal ties routinely mediate access, speaking with more people plausibly signals additional assistance; indeed, the qualitative evidence discussed later supports this interpretation. Also, contrary to mechanisms related to gendered sorting, female applicants were not more likely to be referred to female bureaucrats. In fact, women reported fewer interactions with women staff compared to men, in line with women's overall fewer interactions.

The self-reported measures—the bottom three variables in Figure 2—indicate that women

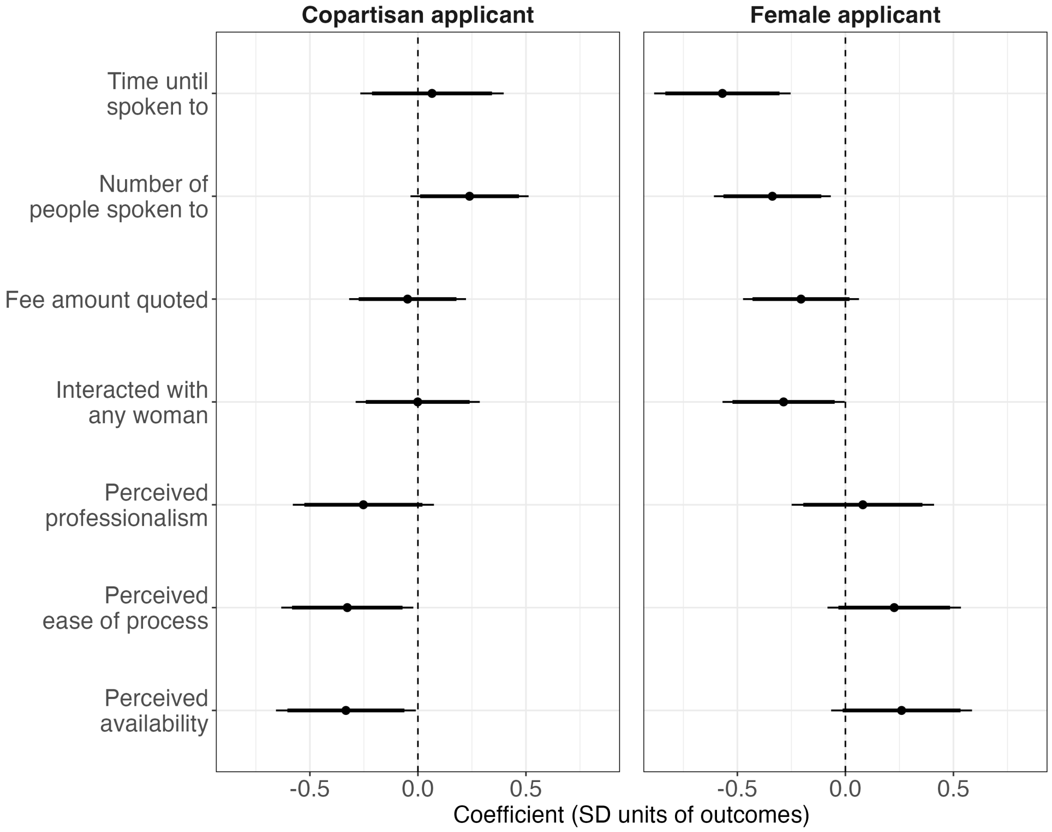

Figure 2: Mechanisms: Partisan and gender treatments

Notes: This figure plots estimates for outcomes collected during the permit application process. Outcomes are standardized. Models are estimated using OLS and include fixed effects for randomization blocks. Whiskers display 95% and 90% confidence intervals. The corresponding table output is in Appendix Table A5 and A6.

rated the process as somewhat easier, while copartisans rated it harder. These perceptions likely reflect differential effort extended to each group: copartisans experienced more steps because more staff were mobilized on their behalf, whereas women encountered fewer contacts and more outright refusals. However, it is difficult to disentangle these self-reported measures from applicants' baseline expectations or from underlying perceptions of ease and availability, both of which are likely gendered. Copartisans' overall experiences were also made somewhat more difficult because they occasionally encountered bureaucrats from the opposing political coalition; because council seats are not uniformly allocated to the winning party (they are allocated via a proportional system), copartisan applicants sometimes experienced resistance that noncopartisans did not. Still, this minor resistance was rarely consequential, as copartisans

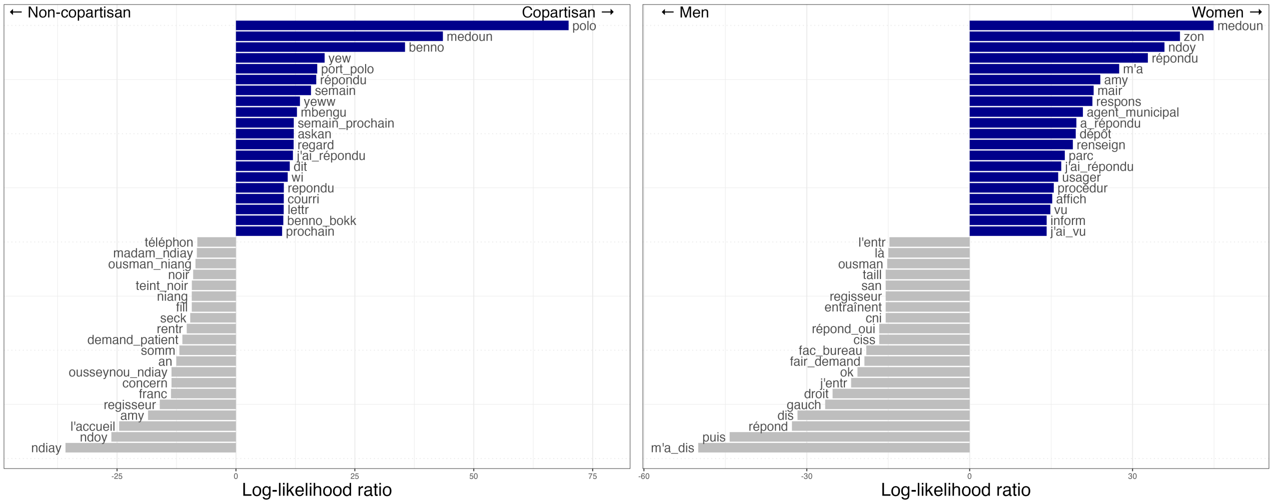

Figure 3: Keyness analysis of employee narratives

Notes: This figure displays keyness scores (log-likelihood ratios) comparing word and bigram frequencies across treatments. Higher values indicate terms that appear disproportionately more often in one group relative to the other. The left panel compares narratives by copartisan vs. non-copartisan applicants (partisanship treatment), and the right panel compares narratives by male vs. female applicants (gender treatment).

successfully submitted their applications at a much higher rate (Figure 1).

To further assess the nature of bureaucratic bias, I leverage the extensive narratives employees documented after each application process. I examine these narratives in two ways: first with structured text analysis, and second by identifying illustrative examples. Figure 3 plots the results of examining the corpus of narratives by "keyness," a measure that reflects words and phrases that disproportionately appeared by treatment group (Gabrielatos 2018; Rayson and Garside 2000).[28]

For copartisanship, narratives unsurprisingly reference treatment cues (e.g., the partisan polo) and coalition names, but also contain more language associated with completing the multi-step process (e.g., "next week," "letter," "courier"). Control narratives more often include phrases like "ask for patience" and "return," as well as mentions of "franc," suggesting longer delays, repeated visits, and more discussion of payment.

Examining the narratives more closely lends additional support that bureaucrats exerted more effort for copartisans. For example, copartisans frequently reported being referred to multiple people after their first point of contact at councils, while noncopartisans were more likely to be told to wait or directed to only one person. An illustrative example comes from a copartisan's narrative:

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[28] I use the `quanteda` package in R to conduct this analysis (Benoit et al. 2018).

> "I met a 'fellow' party member at the door who worked at the office who joked with me and led me to an office with three people who all helped me. Even though he mentioned at the beginning that I would need to fill out a technical form, he ended up not following that procedure and accepted the materials I had."

Contrast this with the lengthy process of a non-copartisan who received no such special treatment:

> "I arrived at the town hall and asked the guard at the door where I could submit my application. He directed me to the first floor to a Mr. Gueye,[29] but when I reached, there were several offices and they were unmarked. I saw what seemed to be a secretary's office and entered, then asked about my application and Mr. Gueye. The secretary hesitated before telling me to take a seat and wait.
>
> [An hour passes and then he enters Mr. Gueye's office]
>
> I sat down and began explaining the reason for my visit. Before I could finish explaining, he interrupted me to tell me that this is a matter for the technical services. He told me to submit a request there and showed me the door.
>
> [After finally submitting the documents and asking for a follow-up number]
>
> The young woman at technical services took me back to Mr. Gueye's office and explained. Mr. Gueye looked at me and said, 'Sir, you don't need a number. We have yours and we'll call you. I already told you that in my office' [he had not]. His face was firmer than before and I could tell it was time to leave."

This experience was successful in that the permit application was ultimately submitted, but, contrasted with the copartisan's experience, it was much more atomistic. The applicant was connected to only one other person at a time, and his experience was tainted by long waits and curt bureaucrats unwilling to go the extra mile for his application.

Turning to the results by gender in Figure 3, certain proper names were more common for women, suggesting they were more likely to interact with particular bureaucrats than men. The figure also shows that men used more directional language, reflective of their longer processes and more intensive navigation of the councils: words like "left," "right," and "next" were much more likely to appear in male applicants' narratives. Even the phrase "responded yes" was more likely to be stated by men, suggesting an ease that was absent for women.

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[29] Name changed for anonymity.

Furthermore, women applicants' narratives align with gendered expectations about participation in the formal economy in Senegal. Relative to men, women were often quizzed about the nature and legitimacy of their work and, when wearing the copartisan polo, questioned about partisan identity (58% of women compared to 36% of men were asked about politics; t-test p-value = 0.572). When a female applicant encountered a non-copartisan while wearing the ruling party's polo, she wrote in her narrative:

> "After reading the application, he told me he shouldn't even accept it since I'm an opponent. On top of that, he started questioning the validity of our business registration document. He said he wanted to throw me out of his office, but that he wouldn't because he said he felt a little sorry for me. He did take the application, but he didn't give me a receipt."

Several female applicants also reported requests for personal contact information unrelated to follow-up as well as other inappropriate remarks from officials. These accounts are consistent with a pattern in which women encountered fewer helpful handoffs and more questioning of credentials. For example:

> "He [the bureaucrat assisting her application] gave me his personal number and insisted I give him mine as well. He also asked me personal questions that made me uncomfortable."

Overall, the qualitative findings suggest gendered treatment and bureaucratic expectations of women in the private sector led to differential application processes.

5.3 In-group social mechanisms: coethnicity, coreligion, and cogender

I additionally test for in-network mechanisms commonly thought to drive political economic outcomes in sub-Saharan Africa. This includes shared ethnoreligious identity—religious networks are especially salient in Senegal—as well as whether the applicant and bureaucrat were both women. Given the body of research that suggests that in-group networks can moderate distributive outcomes (Grimard 1997; Habyarimana 2007; Larson 2017; Larson and Lewis 2017), it is important to ensure that these social factors are not driving the results. These tests also help assess whether the results related to partisanship reflect social homophily or affinity rather than political mechanisms, since co-ethnicity and co-religion may capture some of the underlying social ties in this context. While this strategy cannot fully disentangle political–economic explanations from social affinity, it helps address this concern. Remaining ambiguity is a question for future research.

After each application, employees completed a short survey in which they recorded the ethnic group and religious affiliation of the bureaucrat who assisted them. Because ethnicity and religion are often apparent from names and because introductions in Senegal can be extensive, enumerators reported moderate-to-strong confidence in these classifications.[30] I use these data to compare outcomes for coethnic and coreligious dyads and also to estimate heterogeneous treatment effects.

The results, presented in Appendix D.1, show that these alternative in-group explanations are largely insignificant. In general, there are few meaningful interactions between the treatment variables and shared ethnoreligious identity. The copartisan advantage is not conditioned by shared ethnic or religious ties between applicants and bureaucrats. The gender treatment is similarly unaffected, with one minor exception: women who shared an ethnoreligious identity with the bureaucrat were helped more quickly, but the more consequential outcomes—successfully depositing an application and advancing to the final stage—do not differ.

Finally, I test whether pairs of female applicants and female bureaucrats produced differential outcomes. If gender solidarity were driving results, for example, we would expect higher success rates when women encountered women. While Appendix Table D18 does show an interaction for visit duration, cogendered dyads did not produce differential rates of successful deposit. In fact, heterogeneous mechanism tests show instead that female bureaucrats referred women to *fewer* additional staff members, suggesting that gender bias was not limited to male bureaucrats.

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[30] For example, many employees reported religious imagery, associated with particular Muslim brotherhoods, hung in the offices of bureaucrats. Further, discussing ethnic group and religious affiliation is not taboo in Senegalese society, especially when done in the context of a free-flowing introductory chat.

6 Conclusion

This paper identifies a channel through which bureaucratic bias generates inequality: discretionary gatekeeping at the point of market entry. When frontline bureaucrats control the permits required to operate, political and social biases shape who can participate in the economy. Using a field experiment involving an actual business and real permit applications, I show that bureaucratic discretion advantages copartisans and disadvantages women at the initial gateway to the private sector. Copartisan applicants receive more assistance navigating the bureaucracy, while women face higher rates of outright rejection despite following identical procedures. These patterns operate primarily during face-to-face interactions rather than in later administrative processing, revealing where in the institutional pathway biases emerge. By identifying the magnitude and location of partisan and gender bias, the study informs accounts of how political and social status shape inequality in access to economic opportunity in low-income environments.

These inequalities have direct consequences for citizens seeking economic mobility through the private sector, and the results of this paper carry important implications for policies intended to encourage sustainable and equitable private-sector development. Existing reforms that focus on expanding the formal economy in low-income countries may do so at the continued expense of historically excluded citizens. These interventions do not guarantee equitable development if underlying inequities in *access* remain unaddressed. Furthermore, policymakers and international organizations frequently treat formalization—the act of registering a firm—as the critical barrier to private-sector participation. This focus has driven a push to simplify business registration across the Global South. Yet the permits and licenses required *after* registration—and controlled by street-level bureaucrats—create a distinct and understudied chokepoint, as this paper shows. The bureaucratic discretion that once held up formalization has not disappeared; it has simply migrated elsewhere, preserving the private sector for the privileged. Expanding formalization without reducing discretionary gatekeeping risks widening inequalities: the benefits of private-sector development will continue to accrue disproportionately to those with political or social privilege. Policies that standardize procedures, reduce discretion during initial contact, or anonymize applicant identity could meaningfully reduce these biases.

This study is an initial step toward understanding the impact of bureaucratic bias on access to state-facilitated private-sector development. Three directions for future research are especially promising. First, while the present study focused on formal firms, most firms in Senegal and across sub-Saharan Africa are informal and still require politically controlled approvals (e.g., Holland 2016). Understanding how political and social gatekeeping differentially affect informal versus formal firms is crucial for understanding the aggregate impact of bureaucratic discretion on the economy. Second, this paper identifies a partial equilibrium finding by varying aspects of applicants' identities and observing subsequent outcomes. In a full equilibrium, non-copartisans and women may opt out of permit applications altogether. Future work should aim to quantify this self-removal from the private sector, as observed effects in this paper likely understate the true economic consequences of bias. Third, while this experiment focuses on applications, more research is needed on bureaucrats themselves: how they interpret identity cues, how they perceive rewards and risks to helping the politically or socially privileged, and what shapes their willingness to exert effort. Extensive bureaucrat surveys could help to shed light on these questions.

Although the experiment takes place in Senegal, the mechanisms are likely to extend to other settings where enforcement of the rule of law is uneven and bureaucrats exercise discretion. In places with weaker oversight, greater gender inequality, or more politically captured bureaucracies, the distributive consequences of discretion may be even larger. Tracing these dynamics across and within states will help identify institutional reforms that limit opportunities for bias and support a private sector that promotes equitable growth rather than reproduces inequality.

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A Table form of figure results

A.1 Average treatment effects (Figure 1)

The following two tables contain the table form of the ATEs for partisanship; the first presents the results with the standardized outcomes (depicted in Figure 1) and the second presents the non-standardized outcomes.

Table A1: Average treatment effects: Partisanship (standardized outcomes)

	Application process			**Follow-up**	
	Duration of visit (minutes)	Number of visits required	Application deposited successfully	Number of follow-up calls required	Advanced to final stage
Copartisan applicant	-0.158 (0.149)	-0.218^{**} (0.108)	0.511^{***} (0.156)	-0.025 (0.182)	0.311^{**} (0.155)
Control group mean	0.067	-0.163	-0.305	0.014	-0.144
Control group std. dev.	1.124	0.820	1.170	0.951	0.957
Observations	183	159	159	159	159

Notes: This table displays estimates for outcomes collected during the permit application and follow-up processes. Outcomes are standardized. Models are estimated using OLS and include fixed effects for randomization blocks. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from pre-registered one-sided tests.

Table A2: Average treatment effects: Partisanship (non-standardized outcomes)

	Application process			**Follow-up**	
	Duration of visit (minutes)	Number of visits required	Application deposited successfully	Number of follow-up calls required	Advanced to final stage
Copartisan applicant	−8.993	−0.152**	0.206***	−0.020	0.152**
	(8.440)	(0.076)	(0.063)	(0.146)	(0.076)
Control group mean	92.430	1.225	0.675	1.471	0.312
Control group std. dev.	63.788	0.573	0.471	0.762	0.466
Observations	183	159	159	159	159

Notes: This table displays estimates for outcomes collected during the permit application and follow-up processes. Models are estimated using OLS and include fixed effects for randomization blocks. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from pre-registered one-sided tests.

The following two tables contain the table form of the ATEs for gender; the first presents the results with the standardized outcomes (depicted in Figure 1) and the second presents the non-standardized outcomes.

Table A3: Average treatment effects: Gender (standardized outcomes)

	Application process			**Follow-up**	
	Duration of visit (minutes)	Number of visits required	Application deposited successfully	Number of follow-up calls required	Advanced to final stage
Female applicant	−0.144	0.176*	−0.240*	−0.361**	−0.100
	(0.149)	(0.109)	(0.161)	(0.178)	(0.157)
Control group mean	0.089	-0.359	0.068	0.145	0.062
Control group std. dev.	0.938	0.466	0.949	1.156	1.017
Observations	183	159	159	159	159

Notes: This table displays estimates for outcomes collected during the permit application and follow-up processes. Outcomes are standardized. Models are estimated using OLS and include fixed effects for randomization blocks. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from pre-registered one-sided tests.

Table A4: Average treatment effects: Gender (non-standardized outcomes)

	Application process			**Follow-up**	
	Duration of visit (minutes)	Number of visits required	Application deposited successfully	Number of follow-up calls required	Advanced to final stage
Female applicant	−8.156	0.123*	−0.097*	−0.289**	−0.049
	(8.444)	(0.076)	(0.065)	(0.142)	(0.077)
Control group mean	93.682	1.087	0.825	1.575	0.412
Control group std. dev.	53.252	0.326	0.382	0.927	0.495
Observations	183	159	159	159	159

Notes: This table displays estimates for outcomes collected during the permit application and follow-up processes. Models are estimated using OLS and include fixed effects for randomization blocks. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from pre-registered one-sided tests.

A.2 Mechanisms (Figure 2)

Results for copartisanship:

Table A5: Mechanisms: Copartisanship (standardized outcomes)

	Time until spoken to (1)	Number people spoken to (2)	Fee quoted (3)	Interacted woman (4)	Professionalism (5)	Ease of process (6)	Availability (7)
Copartisan applicant	0.065	0.239^{*}	−0.048	−0.001	−0.252	-0.327^{**}	-0.333^{**}
	(0.169)	(0.139)	(0.138)	(0.147)	(0.167)	(0.156)	(0.165)
Control group mean	0.031	-0.102	0.022	0.004	0.066	0.162	0.165
Control group std. dev.	0.720	0.887	0.960	1.006	0.976	0.907	0.995
Observations	159	183	183	159	159	159	159

Notes: This table displays estimates for outcomes collected during the permit application process. Outcomes are standardized. Models are estimated using OLS and include fixed effects for randomization blocks. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

Results for gender:

Table A6: Mechanisms: Gender (standardized outcomes)

	Time until spoken to (1)	Number people spoken to (2)	Fee quoted (3)	Interacted woman (4)	Professionalism (5)	Ease of process (6)	Availability (7)
Female applicant	-0.569^{***}	-0.338^{**}	−0.205	-0.286^{**}	0.081	0.226	0.260
	(0.161)	(0.138)	(0.137)	(0.144)	(0.168)	(0.158)	(0.166)
Control group mean	0.344	0.178	0.137	0.144	-0.098	-0.115	-0.127
Control group std. dev.	1.296	1.198	1.205	1.065	0.975	1.131	1.111
Observations	159	183	183	159	159	159	159

Notes: This table displays estimates for outcomes collected during the permit application process. Outcomes are standardized. Models are estimated using OLS and include fixed effects for randomization blocks. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

B Enumerator robustness checks

The following two tables display ATEs for both treatments when enumerator fixed effects are included. As the results show, the partisanship results are robust to their inclusion. The results for gender are more statistically fragile, as mentioned in the results section in the manuscript.

Table B7: Effect of Co-partisan Applicant: Enumerator Fixed Effects (Standardized)

	Application process			**Follow-up**	
	Duration of visit (minutes)	Number of visits required	Application deposited successfully	Number of follow-up calls required	Advanced to final stage
Co-partisan applicant	−0.054	−0.202**	0.556***	0.029	0.371**
	(0.116)	(0.120)	(0.166)	(0.189)	(0.160)
Control group mean	0.067	-0.163	-0.305	0.014	-0.144
Control group std. dev.	1.124	0.820	1.170	0.951	0.957
Observations	183	159	159	136	159

Notes: This table displays estimates for outcomes collected during the permit application and follow-up processes. Outcomes are standardized. Models are estimated using OLS and include fixed effects for randomization blocks and enumerators. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from pre-registered one-sided tests.

Table B8: Effect of Female Applicant: Enumerator Fixed Effects (Standardized)

	Application process			**Follow-up**	
	Duration of visit (minutes)	Number of visits required	Application deposited successfully	Number of follow-up calls required	Advanced to final stage
Female applicant	2.396***	0.445	0.567	−0.416	0.024
	(0.336)	(0.353)	(0.509)	(0.683)	(0.479)
Control group mean	0.089	-0.359	0.068	0.145	0.062
Control group std. dev.	0.938	0.466	0.949	1.156	1.017
Observations	183	159	159	136	159

Notes: This table displays estimates for outcomes collected during the permit application and follow-up processes. Outcomes are standardized. Models are estimated using OLS and include fixed effects for randomization blocks and enumerators. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from pre-registered one-sided tests.

As a second robustness check, I verify that the results are not driven by the behavior or reporting style of any single enumerator. To do so, I implement a leave-one-out procedure in which I iteratively drop all observations from one enumerator, re-estimate the main models on the remaining sample, and record the treatment estimates. Figure B1 plots the resulting estimates—one per enumerator dropped—for each outcome and treatment. The dashed line in each panel indicates the full-sample estimate. If a single enumerator were driving the results, we would expect the estimate to shift substantially when that enumerator's observations are removed. In practice, the estimates are stable across all 15 drop-outs, clustering tightly around the full-sample values, which provides reassurance that no individual enumerator is responsible for the main findings. This is especially true of the "application deposited successfully" outcome for partisanship, which is encouraging for the primary conclusions of the manuscript. The exception is for enumerator 15 for the overall duration of the visit. However, this is not the primary outcome, and, if anything, pushes the direction of the coefficient to be even more negative and statistically significant.

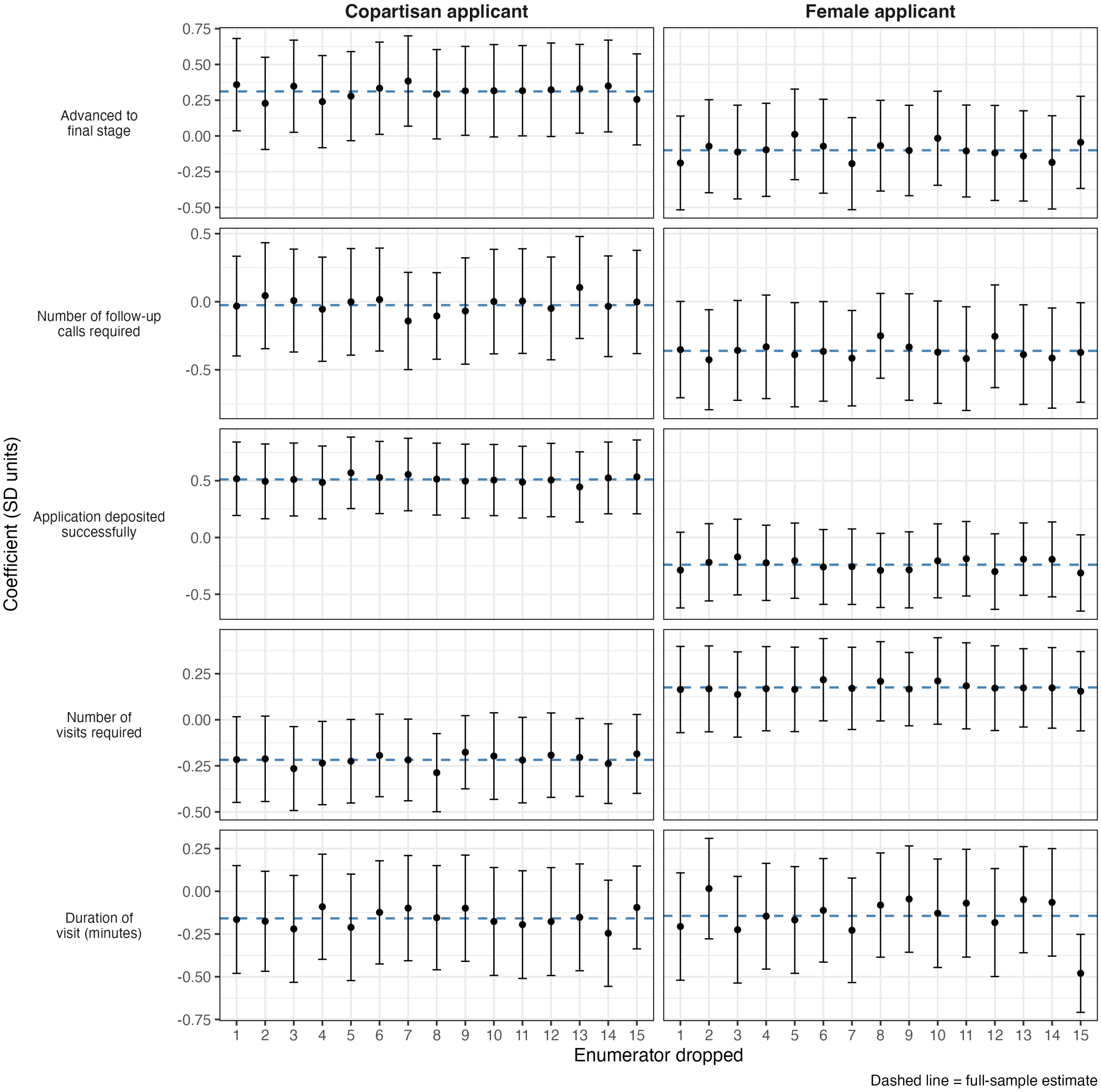

Figure B1: Excluding enumerators one by one

Notes: This figure plots treatment estimates from models in which one enumerator's observations are dropped at a time. Each point represents the estimated treatment effect on a standardized outcome when that enumerator's applications are excluded from the sample. Models are estimated using OLS and include fixed effects for council and permit type. Error bars display 95% confidence intervals. The dashed line indicates the full-sample estimate.

C Interaction between copartisanship and gender

In contrast to the clear predictions that emerge from the existing literatures on political partisanship and gender bias, theory offers less clarity on how these forces interact in gaining access to the private sector. On one hand, partisanship may be a powerful enough signal to offset some of the barriers women face in bureaucratic settings. In contexts where both party politics and the formal economy are male-dominated, a politically active woman may stand out as especially adept. Partisan identity among women in this context might signal deep political embeddedness, business acumen, or both. On the other hand, deeply rooted gender norms may blunt or distort the value of these affiliations. Bureaucrats may perceive women's political ties as less credible or consequential than men's, treating their partisan affiliation as symbolic rather than instrumental.

Table C9 presents the theoretical expectations. Because it is ambiguous whether partisanship ultimately amplifies or attenuates the disadvantages women face, I treat this relationship (i.e., whether group A is higher or lower than group D) as an empirical question to be tested by the research design.

Table C9: Theoretical predictions for interaction of partisanship and gender

		Applicant gender	
		Man	**Woman**
Applicant is bureaucrat's copartisan	**No**	A. Intermediate probability of permit	B. Low probability of permit
	Yes	C. High probability of permit	D. Intermediate probability of permit

To examine these effects, I interact the copartisan treatment with applicant gender and present the results in Figure C2. As these results demonstrate, in general, the political partisan treatment did not differentially affect men and women. An exception is the number of visits required, which is driven primarily by women applicants. It thus seems that copartisanship can offset some of the biases facing women, though, as the other results demonstrate, this did not drive other important outcomes such as successful deposit of application or conditional permit approval.

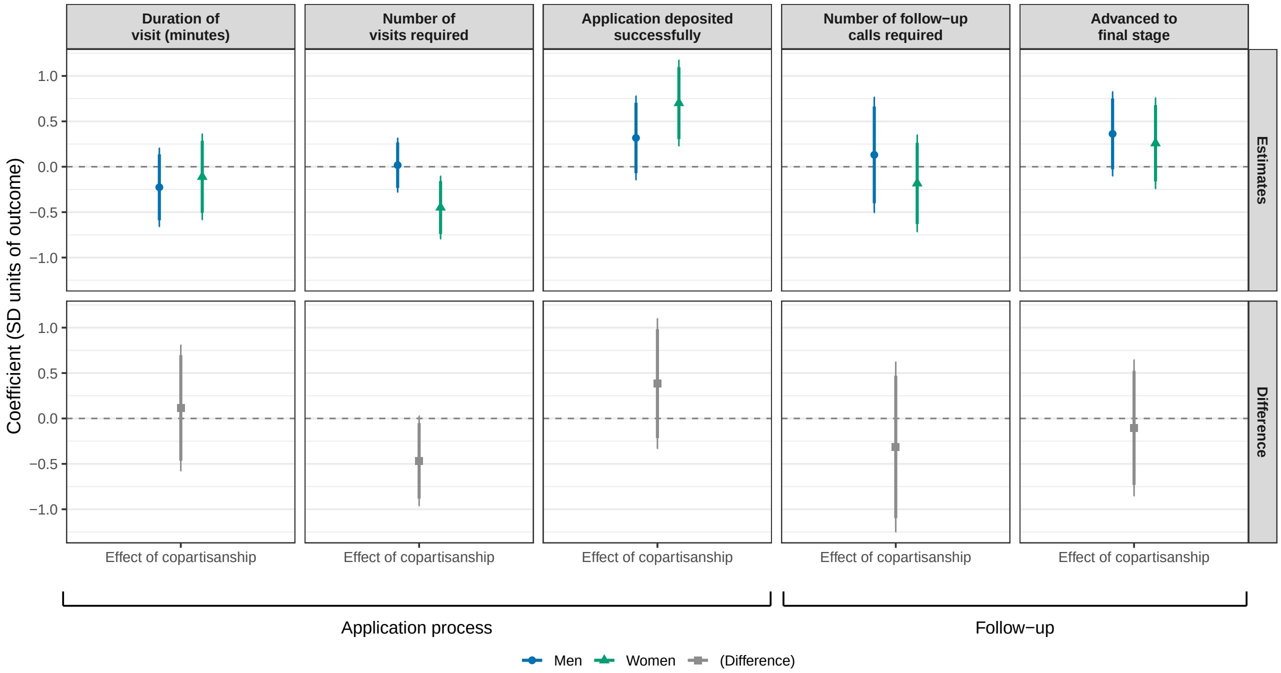

Figure C2: Impact of copartisan treatment by gender

Notes: This figure displays estimates for outcomes collected during the permit application and follow-up processes. Outcomes are standardized. Models are estimated using OLS and include fixed effects for randomization blocks. Whiskers indicate 95% and 90% confidence intervals. The corresponding table output for standardized and nonstandardized outcomes can be found in Appendix Table C10 and C11, respectively.

The following two tables contain these results in table form. The first presents results with standardized outcomes (corresponding to Figure C2), and the second presents the results with non-standardized outcomes.

Table C10: Interaction between treatments (standardized outcomes)

	Application process			**Follow-up**	
	Duration of visit (minutes)	Number of visits required	Application deposited successfully	Number of follow-up calls required	Advanced to final stage
Copartisan applicant	-0.226	0.017	0.317	0.131	0.362
	(0.241)	(0.170)	(0.247)	(0.273)	(0.249)
Female applicant	-0.211	0.408^{**}	-0.428^{*}	-0.205	-0.045
	(0.238)	(0.170)	(0.247)	(0.281)	(0.249)
Copartisan applicant × female applicant	0.115	-0.467^{*}	0.384	-0.314	-0.104
	(0.374)	(0.265)	(0.384)	(0.438)	(0.387)
Baseline mean (non-copartisan male)	0.182	-0.377	-0.181	0.102	-0.169
Baseline std. dev.	1.066	0.381	1.123	1.001	0.952
Observations	183	159	159	159	159

Notes: This table displays estimates for outcomes collected during the permit application and follow-up processes. Outcomes are standardized. Models are estimated using OLS and include fixed effects for randomization blocks. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

Table C11: Interaction between treatments (non-standardized outcomes)

	Application process			Follow-up	
	Duration of visit (minutes)	Number of visits required	Application deposited successfully	Number of follow-up calls required	Advanced to final stage
Copartisan applicant	−12.846	0.012	0.128	0.105	0.177
	(13.656)	(0.119)	(0.099)	(0.219)	(0.121)
Female applicant	−12.001	0.285^{**}	-0.172^{*}	−0.165	−0.022
	(13.509)	(0.119)	(0.100)	(0.225)	(0.121)
Copartisan applicant × female applicant	6.553	-0.326^{*}	0.155	−0.252	−0.050
	(21.225)	(0.185)	(0.155)	(0.351)	(0.189)
Baseline mean (non-copartisan male)	98.959	1.075	0.725	1.541	0.300
Baseline std. dev.	60.473	0.267	0.452	0.803	0.464
Observations	183	159	159	159	159

Notes: This table displays estimates for outcomes collected during the permit application and follow-up processes. Models are estimated using OLS and include fixed effects for randomization blocks. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

D Shared in-group networks

D.1 Results by coethnicity and coreligion

I first present results for coethnicity. Table D12 presents a naive regression of outcomes on coethnicity, Table D13 presents the interactions with partisanship, and Table D14 presents the interactions with gender.

Table D12: Outcomes regressed on coethnicity

	Application process			Follow-up	
	Duration of visit (minutes)	Number of visits required	Application deposited successfully	Number of follow-up calls required	Advanced to final stage
Coethnic bureaucrat	0.392^{*}	0.083	−0.155	0.444	−0.148
	(0.215)	(0.166)	(0.245)	(0.272)	(0.238)
Baseline mean	-0.056	-0.283	-0.013	-0.074	0.051
Baseline std. dev.	0.959	0.657	1.010	0.961	1.012
Observations	183	159	159	159	159

Notes: This table displays estimates for outcomes collected during the permit application and follow-up processes. Outcomes are standardized. Models are estimated using OLS. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

Table D13: Interaction between partisanship and coethnicity

	Application process			Follow-up	
	Duration of visit (minutes)	Number of visits required	Application deposited successfully	Number of follow-up calls required	Advanced to final stage
Copartisan applicant	−0.159	-0.205^{*}	0.543^{***}	0.115	0.234
	(0.165)	(0.123)	(0.176)	(0.200)	(0.175)
Coethnic bureaucrat	0.323	0.060	0.049	0.711^{**}	−0.259
	(0.268)	(0.215)	(0.308)	(0.348)	(0.306)
Copartisan applicant ×	0.135	−0.066	−0.260	−0.770	0.499
coethnic bureaucrat	(0.510)	(0.380)	(0.545)	(0.621)	(0.541)
Baseline mean	0.019	-0.194	-0.313	-0.157	-0.080
Baseline std. dev.	1.131	0.813	1.175	0.686	0.982
Observations	183	159	159	159	159

Notes: This table displays estimates for outcomes collected during the permit application and follow-up processes. Outcomes are standardized. Models are estimated using OLS and include fixed effects for randomization blocks. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

Table D14: Interaction between gender and coethnicity

	Application process			Follow-up	
	Duration of visit (minutes)	Number of visits required	Application deposited successfully	Number of follow-up calls required	Advanced to final stage
Female applicant	-0.393^{**}	0.137	-0.355^{*}	-0.391^{*}	−0.134
	(0.160)	(0.124)	(0.182)	(0.200)	(0.179)
Coethnic bureaucrat	−0.410	−0.026	−0.492	0.234	−0.242
	(0.313)	(0.236)	(0.347)	(0.356)	(0.341)
Female applicant ×	1.558^{***}	0.263	0.728	0.421	0.199
coethnic bureaucrat	(0.457)	(0.377)	(0.553)	(0.634)	(0.544)
Baseline mean	0.171	-0.356	0.131	0.072	0.103
Baseline std. dev.	0.957	0.481	0.891	1.113	1.024
Observations	183	159	159	159	159

Notes: This table displays estimates for outcomes collected during the permit application and follow-up processes. Outcomes are standardized. Models are estimated using OLS and include fixed effects for randomization blocks. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

Next, I present results by coreligion. Table D15 presents a naive regression of outcomes on coreligion, Table D16 presents the interactions with partisanship, and Table D17 presents the interactions with gender.

Table D15: Outcomes regressed on coreligion

	Application process			**Follow-up**	
	Duration of visit (minutes)	Number of visits required	Application deposited successfully	Number of follow-up calls required	Advanced to final stage
Co-religious bureaucrat	−0.386	−0.097	−0.993**	0.652	−0.047
	(0.364)	(0.331)	(0.480)	(0.622)	(0.473)
Baseline mean	0.043	-0.276	0.025	-0.011	0.030
Baseline std. dev.	1.016	0.669	0.981	1.041	1.007
Observations	183	159	159	159	159

Notes: This table displays estimates for outcomes collected during the permit application and follow-up processes. Outcomes are standardized. Models are estimated using OLS. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

Table D16: Interaction between partisanship and coreligion

	Application process			**Follow-up**	
	Duration of visit (minutes)	Number of visits required	Application deposited successfully	Number of follow-up calls required	Advanced to final stage
Copartisan applicant	−0.210	−0.254**	0.464***	−0.038	0.313*
	(0.153)	(0.112)	(0.159)	(0.188)	(0.162)
Co-religious bureaucrat	−0.683	−0.423	−1.096*	0.274	0.035
	(0.430)	(0.398)	(0.565)	(0.914)	(0.573)
Copartisan applicant ×	0.855	0.687	0.529	0.729	−0.032
co-religious bureaucrat	(0.710)	(0.563)	(0.800)	(1.289)	(0.810)
Baseline mean	0.132	-0.160	-0.226	0.022	-0.128
Baseline std. dev.	1.146	0.834	1.138	0.964	0.964
Observations	183	159	159	159	159

Notes: This table displays estimates for outcomes collected during the permit application and follow-up processes. Outcomes are standardized. Models are estimated using OLS and include fixed effects for randomization blocks. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

Table D17: Interaction between gender and coreligion

	Application process			**Follow-up**	
	Duration of visit (minutes)	Number of visits required	Application deposited successfully	Number of follow-up calls required	Advanced to final stage
Female applicant	−0.121	0.209*	−0.186	−0.384**	−0.070
	(0.154)	(0.113)	(0.165)	(0.185)	(0.164)
Co-religious bureaucrat	−0.226	0.326	−0.495	0.454	0.436
	(0.714)	(0.538)	(0.785)	(0.863)	(0.780)
Female applicant ×	−0.160	−0.656	−0.627	0.476	−0.646
co-religious bureaucrat	(0.787)	(0.599)	(0.874)	(1.273)	(0.867)
Baseline mean	0.112	-0.373	0.115	0.140	0.068
Baseline std. dev.	0.947	0.450	0.907	1.175	1.018
Observations	183	159	159	159	159

Notes: This table displays estimates for outcomes collected during the permit application and follow-up processes. Outcomes are standardized. Models are estimated using OLS and include fixed effects for randomization blocks. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

D.2 Results by bureaucrat gender

Table D18: Cogender effects

	Application process			Follow-up	
	Duration of visit (minutes)	Number of visits required	Application deposited successfully	Number of follow-up calls required	Advanced to final stage
Female applicant	0.115	0.223^{*}	-0.269^{*}	-0.343^{*}	-0.026
	(0.177)	(0.135)	(0.200)	(0.220)	(0.195)
Female bureaucrat	0.271	0.044	0.005	0.325	0.194
	(0.264)	(0.201)	(0.297)	(0.316)	(0.290)
Female applicant × female bureaucrat	-0.995^{***}	-0.195	0.138	0.088	-0.231
	(0.355)	(0.279)	(0.413)	(0.465)	(0.403)
Observations	183	159	159	159	159

Notes: This table displays estimates for outcomes collected during the permit application and follow-up processes. Outcomes are standardized. Models are estimated using OLS and include fixed effects for randomization blocks. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

E Two-stage application process: estimating the TRACE

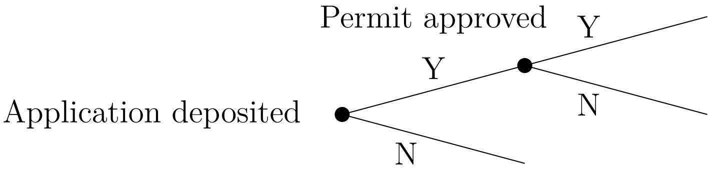

Figure E3: Permit decision tree

Reaching the follow-up stage of the application process is contingent on depositing a successful application (see Figure E3). While the total effect of treatment on eventual permit approval is indeed causally identified, because application deposit is itself partially a result of treatment, inferences about bias in the second stage of the process in particular using the total effect may be susceptible to bias (Slough 2023). This is because the total effect estimated at this stage includes always-takers (those who would successfully be able to deposit their applications regardless of their treatment status), compliers (those who would successfully be able to deposit their applications only if they were treated), and defiers (those who would successfully be able to deposit their applications only if they were untreated).

To address this concern, I estimate the Treatment Reactive Average Causal Effect (TRACE), defined as "the total effect of treatment in the group that, if treated, would realize a particular value of the relevant post-treatment variable" (Hazlett, McMurry and Shinkre 2025), with the post-treatment variable in this case being a deposited application. The TRACE provides the total effect of treatment on permit approval, but averaged over only permit applications in which applicants, if treated, *would have successfully deposited their applications.* Identification of the TRACE relies on positing values for TRACE(0), the total effect among the applications that, if treated, would have not resulted in a deposited application. In this experimental setting, it is plausible to argue that TRACE(0) = 0, which allows for point identification.[31] This assumption is supported because it is impossible to be approved for the permit in the second-stage without successfully depositing an application in the first stage, meaning there can be no effect for those that would not have successfully deposited if treated; in the TRACE framework, M is a *necessary* condition for $Y = 1$. This also rests on assumption of monotonicity, which is reasonable in this experimental setting.[32]

I re-estimate the primary findings from the follow-up stage in Table E19 using the TRACE. Note that, to align with the monotonicity assumption, I recode the gender treatment to equal 1 for male applicants; defiers are defined as women who are permitted to submit their applications only because they are women (which, as Section 5.2 demonstrates, is unlikely to be the case in this sample).

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[31] Point identification is possible for all single values of TRACE(0).

[32] Bounds on the TRACE(0) are still possible to calculate if the assumption of no defiers needed to be relaxed, however. $TRACE(0) \equiv \mathbb{E}[Y_i(1) - Y_i(0)|M_i(1) = 0] = \mathbb{E}[Y_i(1)|M_i(1) = 0] - \mathbb{E}[Y_i(0)|M_i(1) = 0]$. The former term is identified under the experimental randomization, while the latter term is equivalent to $\mathbb{E}[Y_i(0)|nevertakers + defiers]$. Because $\mathbb{E}[Y_i(0)|nevertakers] = 0$ in this setting, this leaves $\mathbb{E}[Y_i(0)|defiers] * (\frac{Pr(defiers)}{Pr(nevertakers+defiers)})$. Because of the binary outcome, the maximum value $\mathbb{E}[Y_i(0)|defiers]$ could take is 1 (i.e., all untreated defiers get a permit) and the minimum it could be is 0 (i.e., no untreated defiers get a permit). Thus, under a given assumption about the proportion of defiers in the sample, it is possible to estimate sharp bounds on TRACE(0), and therefore on the TRACE. The likelihood of defiers is quite low, however: the most plausible chance for defiers would be if female bureaucrats only accepted permit applications from female applicants, but, as Table D18 shows, this is unlikely to be the case.

Table E19: TRACE estimates

	Copartisan applicant		**Male applicant**	
	Number of follow-up calls required	Advanced to final stage	Number of follow-up calls required	Advanced to final stage
	-0.028 (0.205)	0.351^{**} (0.175)	0.347^{*} (0.178)	0.121 (0.191)
Observations	159	159	159	159

Notes: This table displays TRACE estimates for outcomes collected during the follow-up process. Outcomes are standardized. Models include fixed effects for randomization blocks and bootstrapped standard errors. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

The TRACE estimates suggest that for the most significant follow-up outcome—advancement to the final stage—there did not seem to be gender bias, but copartisan bias was significantly positive. This would suggest that all stages of the application process are tilted in the direction of copartisans while only the in-person application stage is biased against women. This reinterpretation does not ultimately alter the primary conclusions and recommendations of this paper: that face-to-face encounters are where biases emerge and policies should aim to reduce bias at this stage. However, these updated results do suggest that additional protections need to be instituted even after the deposit stage. Still, by blinding the initial submission process (or incorporating other measures of standardization, as mentioned in the manuscript's conclusion), the effect of partisanship on the follow-up stage will mechanically be eliminated.

F Pre-analysis plan

The text from the pre-registration plan pertaining to hypotheses and tests are below.

What are the hypotheses to be tested/quantities of interest to be estimated?

- Relative to non-affiliated/non-connected entrepreneurs, politically affiliated/connected entrepreneurs will have better access to permits in terms of immediate and long-term outcomes.
- Relative to men, women will face disproportionate barriers to applying for and receiving business permits in terms of immediate and long-term outcomes.

- The interactive impact of gender and political affiliation may positively or negatively impact access to permits (two-tailed).

- Relative to out-group applicants in terms of the applicant-bureaucrat dyad (measured by ethnic and religious networks), in-group applicants will face fewer barriers to applying for and receiving business permits in terms of immediate and long-term outcomes.

How will these hypotheses be tested?

Using a business that I created for research purposes in Senegal, I hire employees who apply on behalf of the company for permits controlled by municipal governments (town halls) in Dakar and the surrounding areas. Employees will apply for permits that allow them to occupy public spaces as well as permits that authorize publicizing their businesses, valuable permits which are managed at town halls.

This will be done in the context of a field experiment. There are two treatments. First, employees will signal political affiliation/connection via polos from either the ruling coalition in Senegal or the opposition coalition (the polo with the relevant town hall's political affiliation will be worn). For permit application processes assigned to the control condition, employees will wear their regular clothes. There are 40 town halls currently identified, and four permit applications are to be submitted to each. The political treatment is block randomized at the town hall and permit level such that each permit type at the town hall receives both the treatment and the control conditions. These applications will be spaced over time to minimize spillover concerns. If additional institutions are added (e.g., if the project extends beyond Dakar), the same block randomization approach will be followed. The second treatment is the gender of the applicant, blocked at the town hall level.

Immediate outcomes include the waiting time and amount of time required to spend at the town hall, the number of steps required to submit applications, and incidence of bribes requested, among others. Longer-term outcomes include the number of follow-ups required after submitting the permit, as well as successfully receiving the permit.

The hypotheses above will be tested with one-tailed tests unless indicated otherwise. If results are in the opposite direction and significant with two-tailed tests, different symbols will be used in tables to distinguish them. Qualitative evidence from employees' self-administered surveys will also be integrated into the analysis to add texture to the results and to trace specific mechanisms.

F.1 Deviations from pre-analysis plan

Upon consistent feedback, I have reworded the hypotheses in the main text to be more concise; however, their meaning remains the same, and I include the precise pre-registered hypotheses above. Furthermore, I have conducted exploratory analyses to probe the theoretical implications of intermediate outcomes and the timing of bias (during the application process vs. the receipt of permits). The theoretical explorations are thus not presented as pre-registered hypotheses but rather as exploratory, and such analyses are indicated as exploratory in the main body. All such analyses are two-tailed. Last, due to space constraints, the results for the final two hypotheses are presented in the Appendix rather than in the main body of the manuscript.

Note that one pre-registered outcome is missing in the manuscript: the incidence of requested bribes. This is because a very small portion of employees reported bribes (only 4 out of the 159 permit applications—and in two cases, employees stated "I don't know"). Upon closer inspection, it was clear that other bureaucrats were hinting at bribes but never outright requested them, which makes this a difficult variable to code. Instead of bribes, I use the fees that bureaucrats quoted to employees as a proxy measure of bribe request, under the assumption that they would pocket the extra. Data for both variables will be made publicly available upon publication.

G Survey instrument

The following is the survey that employees filled out on tablets during the application stage of the project, immediately before and after entering the town hall where they attempted to apply for a business permit. Bureaucrats did not see the tablets nor know that they were part of a study until they were debriefed (after the full project's implementation was complete).

Enumerator Prefill Section

- Record today's date.
- Enter the ID code for the permit.
- Select enumerator number.
- Verify that the ID corresponds to the correct permit and *mairie* name. Confirm that you are in the correct treatment group and wearing the appropriate apparel before proceeding.

Entering the Institution

- Note the exact time immediately before entering the institution.
- Enter the institution and start the stopwatch on your phone.
- Put away the tablet and do not advance in the survey until you have exited the institution.

Post-Visit Enumerator Questionnaire

The following are to be filled immediately after exiting the institution.

- Record the current time.
- How much time elapsed before you were first assisted?
- How many people did you have to talk to before being pointed in the correct direction?
- Provide the names and titles of the people you spoke with.

Characteristics of Initial Helper

- Who helped you initially? Indicate the department and the agent's name if possible.
- Do you know their ethnicity? Take your best guess.
    - Balante, Bambara, Diakhanke, Diola, Laobe, Lebou, Malinke, Mancagne, Mandinka, Manjack/Manjago, Maures, Pulaar, Peul, Sarakole, Serer, Soce, Soninke, Soussou, Toucouleur, Wolof, Etranger, Sénégalais seulement, Other.
- Do you know their religion? Take your best guess.

  - Aucun, Chrétien seulement, Catholique romain, Chrétien évangélique, Musulman seulement, Sunni seulement, Layenne, Mouride, Tidjane, Khadrya, Animiste, Agnostique, Athée, Other.

Characteristics of Bureaucrat

- Who helped you complete and submit the application? Provide name and title.
- Do you know this person's ethnicity? Religion? Take your best guesses.
- Were you asked to pay any costs? If yes, how much were you asked to pay?
- Did anyone ask you to pay a bribe? If yes, who asked and how?
- What is the current status of your submission?
- Did anyone at the institution mention or ask about politics? If yes, who asked and what did they say?
- Did anyone mention or ask about your political connection? If yes, who asked and what did they say?

Phone and Follow-up Information

- Record a phone number for follow-up.
- Indicate who the number belongs to and the department. If it is a personal number, give the agent's name and title.
- Indicate how you obtained the number.

Enumerator Perceptions

- On a scale of 1–10, how polite were the people you interacted with?
- On a scale of 1–10, how easy did you find the process?
- On a scale of 1–10, how helpful or available was the staff?

Enumerator Narratives

- Describe in detail everything that happened between entering and exiting the institution.
- Add any other comments about the process.

End of Survey.