Personnel is Policy (Implementation): Bureaucrats and the Korean Export Miracle*

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Abstract

Does the effect of industrial policy depend on the capacity of the bureaucrats implementing it? We exploit the three-yearly rotation of managers of South Korea's export promotion offices in 87 countries between 1965 and 2000 to show that a one standard deviation increase in bureaucrat ability boosts exports by 37%. The differences between bureaucrats are so large that the policy has no effect when implemented by a bureaucrat one standard deviation below average – despite a large average effect of the policy. Under higher-ability bureaucrats, South Korean exports respond more strongly to a country's import demand, suggesting a more effective transmission of market information. Higher-ability bureaucrats are also more likely to serve multiple appointments, consistent with first appointments functioning as screening devices for bureaucrats. Lastly, bureaucrat experience matters – the products a bureaucrat is exposed to during their first appointment also see export increases in subsequent appointments. This signifies that while organizational capacity can grow endogenously, it might also exhibit path dependence.

JEL classification: D73, L52, M50, O1, F13

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1 Introduction

State and bureaucratic capacity are strongly associated with economic development (Besley, Burgess, Khan, and Xu, 2022; Rasul and Rogger, 2017; Xu, 2018). There has been substantial interest in the importance of individual bureaucrats and managers in determining the capacity of the bureaucracy (Bergeron, Tourek, and Weigel, 2024; Best, Hjort, and Szakonyi, 2023; Dahis, Schiavon, and Scot, 2023; Fenizia, 2022; Janke, Propper, and Sadun, 2019; Muñoz and Prem, 2024; Otero and Muñoz, 2022). At the same time, a resurgent literature investigates the effects of industrial policies, with a focus on their economic rationale and design (Juhász, Lane, and Rodrik, 2023; Lane, 2025), but less is known about whether their impact depends on execution. We bridge these two strands of research by asking: How much does the effect of an industrial policy depend on the capacity of the bureaucrats implementing it?

In this paper, we make three contributions. First, we provide evidence that the effect of an industrial policy on export promotion crucially depends on the ability of the directors of overseas export promotion offices. Second, we find empirical support that the policy's effect can be increased by screening bureaucrats based on performance in their first appointment. Third, we show that learning-by-doing can build a bureaucrat's capacity, implying a novel channel for path dependence in organizational capacity.

Investigating whether bureaucratic capacity impacts the effect of a policy has been difficult because doing so requires a setting that satisfies the following conditions: First, we need variation in bureaucratic capacity while holding constant the policy. This condition may be satisfied if a national policy is implemented decentrally across many locations. Second, this capacity needs to vary while holding constant the location, whose economic conditions may directly impact the policy outcome. Such variation may occur when the bureaucrats implementing a policy move between locations. Third, enough bureaucrats need to move so that locations and bureaucrats form large connected sets, ideally one connected set containing all locations and bureaucrats. To apply frontier methodologies, even a leave-one-out connected set is needed (Kline, Saggio, and Sølvsten, 2020). Fourth, the mapping from bureaucrats to the policy's effect needs to be one-to-one, i.e. the bureaucrats do not engage in multi-tasking. This condition is satisfied if each bureaucrat only works on this policy, and the policy's outcome is measurable in each location – ideally, an outcome closely linked to economic growth.

To satisfy these conditions, we pick an appropriate context: South Korea's overseas export promotion, a pivotal industrial policy during the country's growth miracle. First, the policy was implemented decentrally in 87 destination countries. Second, the bureaucrats who manage each country office rotate between countries every three years, providing potentially exogenous variation in the implementing capacity within location. Third, the largest connected set includes 86 of 87 countries. The leave-one-out connected set is also large, including 75 countries and 93% of all appointments. 2/3 of these country offices are run by at least eight distinct bureaucrats over the study period – providing statistical power for our

main estimation strategy. Fourth, in each country the policy has a sole target: exports to that country, an important development outcome.[1]

Our setting is also of substantial intrinsic interest: South Korea may be the most prominent example of a low-income country to reach high income. Exports were a key target of its policies (Jones and Il, 1980) and South Korea's export growth is a particularly remarkable phenomenon to be explained, growing from 1/64 of U.S. per capita exports in 1962 to parity by 2000 (IMF: Direction of Trade Statistics, 2023). Qualitative political economy accounts link this growth to intervention by a capable state (Johnson, 1982; Amsden, 1989; Wade, 1990; Evans, 1995; Woo-Cumings, 1999). By quantifying how much a key policy's effect is due to implementing capacity, our findings shed light on the lessons today's low- and middle-income countries can draw from South Korea's development success, particularly as export promotion remains central to many industrial policies (Juhász, Lane, and Rodrik, 2023; Lederman, Olarreaga, and Payton, 2010).

We find that the policy had a substantial effect on average, which motivates the study of how much this effect depends on individual bureaucrats. We use the offices' staggered roll-out to estimate the effect of opening an office.[2] Exports increase by 38% in the ten years after an office opening.

Our main result demonstrates that the effect of the policy on exports strongly depends on the bureaucrat assigned to manage a country office. We use a movers design in a two-way fixed effects framework (Abowd, Kramarz, and Margolis, 1999) that exploits the regular rotation of bureaucrats between offices. We find that increasing bureaucrat ability by one standard deviation increases exports by 37%. In combination with the estimated effect of an office opening, this suggests the policy of overseas export promotion through these offices would have no effect if implemented by bureaucrats one standard deviation below average. The estimate of a standard deviation in bureaucrat ability is obtained via a variance decomposition that uses a leave-out estimator to correct for limited mobility bias (Kline, Saggio, and Sølvsten, 2020). An alternative bias correction method inspired by covariance shrinkage techniques (Best, Hjort, and Szakonyi, 2023) produces near-identical values.

Unbiased estimation of bureaucrat fixed effects in the two-way fixed effects framework requires a key assumption: that bureaucrat ability is uncorrelated with underlying trends

[1] Exporting is important for economic growth and development more broadly. For evidence highlighting the effect of exports on development outcomes at the firm-level, see Atkin, Khandelwal, and Osman (2017). For evidence at the macro-level, see Hausmann, Hwang, and Rodrik (2007); Atkin, Costinot, and Fukui (2025) For support that demand-side factors may be decisive in economic development, see Goldberg and Reed (2023). Exporting remains central to many sectoral industrial policies Juhász, Lane, Oehlsen, and Pérez (2022). Further, Lederman, Olarreaga, and Payton (2010) report that more than 100 countries have an export promotion agency comparable to the Korean one.

[2] The main specification estimates the effect of opening an overseas office relative to a never-treated control group. This choice of control group avoids the potential biases arising in a two-way fixed effects regression caused by the combination of dynamic treatment effects with the treatment's staggered roll-out. Recent advances in the difference-in-differences literature allow us to test robustness using a not-yet-treated control group (Callaway and Sant'Anna, 2021) and to test how sensitive the results are to violations of the parallel trends assumption (Rambachan and Roth, 2023).

in the outcome variable.[3] We provide evidence in support of the assumption through numerous diagnostic checks (Card, Cardoso, and Kline, 2016). Our main diagnostic check is an event-study of changes in exports around the event of bureaucrat switches, based on the following predictions. First, the three-yearly rotation means bureaucrat appointments cannot be timed perfectly. Hence, there would be differential pre-trends by bureaucrat ability if good bureaucrats were appointed because of underlying trends, as some bureaucrats would be appointed only after the change in trends materialize. Second, the three-yearly rotation means losing a bureaucrat is determined three years prior – at the time of the bureaucrat's appointment. Thus, there is less discretion regarding bureaucrats that leave a country office than those that join it. If our estimated bureaucrat effects were biased due to strategic appointments of good bureaucrats to countries with growing exports, an event-study should find larger absolute effects to gaining a bureaucrat than losing one. Empirically, however, we find parallel pre-trends and symmetric effects of gaining and losing a bureaucrat. Further diagnostic checks investigate other potential concerns.

The large differences between the abilities of office managers are explained by one key mechanism: Better bureaucrats transmit timely market information more effectively. We show that, upon the appointment of a high-ability bureaucrat, exports of a product rise much more responsively to simultaneous growth in the product's import demand (or export supply)[4].

Descriptive appointment patterns are consistent with the organization recognizing and managing uncertainty over bureaucrat ability. First, bureaucrats are much less likely to be reappointed if exports underperform during their first appointment. Second, first appointments as country office managers are to less important trade partners, while later appointments tend to be to more important ones.[5] Hence, the most important offices are rarely run by underperforming bureaucrats.[6,7]

In the final part of our empirical analysis, we show that bureaucrat experience in certain products raises their exports relative to other products. This finding has mixed implications. On the one hand, bureaucratic capacity along a targeted dimension can be built, not solely from selecting inherently competent individuals but also from an existing stock of bureaucrats. On the other hand, bureaucratic capacity may exhibit path dependence and may even be backwards-looking in an environment with fast-changing policy objectives. To arrive at this finding, we isolate quasi-random variation in bureaucrat experience: a

[3]Importantly, this assumption is not violated if bureaucrat and country effects are correlated – e.g., if better bureaucrats are assigned to larger countries.

[4]Import demand is measured by exports of the same product from countries other than South Korea to the same destination, while export supply is measured by the exports of the same product from South Korea to other destination countries.

[5]We classify countries as less important if they have lower fixed effects or later opening years.

[6]Note that as long as the ranking of country importance is time-invariant, such an appointment process satisfies the identifying assumptions for bureaucrat fixed effects.

[7]These patterns are consistent with the organization using offices in less important countries to experiment with inexperienced bureaucrats – a strategy more broadly applicable to teachers and managers in other settings, where observable characteristics insufficiently predict the substantial variation in performance.

product's import demand growth during the bureaucrat's first appointment. Event-study estimates around a switch in bureaucrats indicate that exports increase by 3.0% when the quasi-random component of product-specific experience increases, relative to other products in the same country-year. Similar to differences in bureaucrat ability, experience in a product causes exports to increase more strongly when import demand (export supply) goes up.

This paper contributes to the literature on bureaucratic capacity and economic development (Besley et al., 2022) by combining aspects of the following two approaches. The first approach studies bureaucrats with a clearly defined output for which they are immediately responsible. These are often front-line bureaucrats in lower tiers of the state's hierarchy.[8] The second approach studies CEO-like bureaucrats with geographic responsibility who can then plausibly be linked to broad measures of economic activity in their region of responsibility.[9] The bureaucrats we study embody advantages of both approaches, as they manage offices targeting a common outcome variable, defined at a geographic level: exports to the country where the office is located. This setting allows for a unique quantification of the role of bureaucrats in industrial policy implementation during a growth miracle.

Second, we contribute to the literature on the effects of industrial policies (Juhász, Lane, and Rodrik, 2023; Juhász, 2018; Liu, 2019; Lane, 2025; Choi and Levchenko, 2021; Choi and Shim, 2024; Kim, Lee, and Shin, 2022). Unlike previous studies, this paper focuses on understanding the variation in industrial policy outcomes driven by the bureaucrats implementing the policy. In doing so, we provide evidence for the often-hypothesized but under-researched link between state capacity and the effectiveness of industrial policy.

Third, this paper builds on research into the role of managers in determining economic outcomes. It investigates managers by applying methods from the labor literature on worker and firm heterogeneity first proposed by Abowd, Creecy, and Kramarz (2002).[10] Fenizia

[8]Front-line bureaucrats with specific tasks are often studied, as the empirical tools from the "credibility revolution" (Angrist and Pischke, 2010) are typically more appropriate for them. As summarized by Besley, Burgess, Khan, and Xu (2022), these include: "agricultural extension workers (Dal Bó, Finan, Li, and Schechter, 2021), revenue collectors (Khan, Khwaja, and Olken, 2019, 2016; Aman-Rana, 2020), health care providers (Ashraf and Bandiera (2018), Khan 2020), teachers (Akhtari, Moreira, and Trucco 2020, Leaver, Ozier, Serneels, and Zeitlin 2021, Brown and Andrabi 2021), procurement officers (Bandiera, Best, Khan, and Prat, 2020, Best, Hjort, and Szakonyi 2019), and judges (Dahis, Schiavon, and Scot 2020, Mehmood 2021)". For the papers that have since been updated, see Bandiera, Best, Khan, and Prat (2021); Best, Hjort, and Szakonyi (2023); Dahis, Schiavon, and Scot (2023); Mehmood (2022); Khan (2018); Akhtari, Moreira, and Trucco (2022); Leaver, Ozier, Serneels, and Zeitlin (2021); Brown and Andrabi (2021). Beyond Besley et al. (2022), further relevant paper are Kondylis and Stein (2023); Otero and Muñoz (2022); Chetty, Friedman, and Rockoff, 2014a,b.

[9]As summarized by Besley, Burgess, Khan, and Xu (2022), these include: "provincial governors and GDP growth (Jia 2017), governors and colony-level revenue generation (Xu 2018)" - up to date citation: Jia (2024) Besley, Burgess, Khan, and Xu (2022) further include among these bureaucrats with CEO characteristics ones with tasks more distant to economic growth, e.g. office managers in charge of processing social insurance claims (Fenizia 2022). An interesting case forms (Gulzar and Pasquale 2017) who study Indian bureaucrats responsible for district-level development outcomes writ-large. The authors focus on one of their tasks with clearly measurable and interpretable outcomes: the implementation of the NREGA workfare program.

[10]Card, Heining, and Kline (2013); Card, Cardoso, and Kline (2016); Kline, Saggio, and Sølvsten (2020); Bonhomme, Holzheu, Lamadon, Manresa, Mogstad, and Setzler (2023).

(2022) and Otero and Muñoz (2022) follow a similar approach to show that managers matter in the processing of social insurance claims and in public hospitals. A closely related literature similarly finds large effects of managers on firms (Bertrand and Schoar, 2003; Metcalfe, Sollaci, and Syverson, 2023).[11] This paper differs by studying managers implementing an economic policy targetting firms. Moreover, by showing that managers develop domain-specific capacity through learning-by-doing, we shed light on a less explored aspect of organizational capacity: how it might be shaped endogenously.

Fourth, we contribute to the extensive literature on trade and economic development. One particular area we build on examines the impact of exporting on firm growth (Atkin, Khandelwal, and Osman, 2017; Alfaro-Ureña, Manelici, and Vasquez, 2022) and market-size constraints in development (Goldberg and Reed, 2023). We provide evidence suggesting that individual bureaucrats affect firms' access to foreign markets. A closely related line of research investigates export promotion. Similar to industrial policy, most studies on export promotion estimate average treatment effects (Munch and Schaur, 2018; Hayakawa, Lee, and Park, 2014; Bagir, 2020; Volpe Martincus and Carballo, 2008), while some delve into specific mechanisms by examining which sectors and firm types benefit the most (Volpe Martincus and Carballo, 2010, 2012). This includes Kim and Kim (2024), who show that public information on potential foreign buyers, provided by the same trade promotion agency that we study, significantly boosted exports of small and medium-sized firms by alleviating search frictions. This paper contributes to this literature by showing that the effect of export promotion varies critically by the capacity of the bureaucrats implementing it.

The rest of the paper proceeds as follows. Sections 2 describes the institutional background. Section 3 introduces the data. Section 4 studies how much individual bureaucrats affect exports, and the success of the policy. Section 5 discusses the policy's average effect estimated using office openings. Section 6 focuses on experience as a factor determining a bureaucrat's effect. Section 7 concludes.

2 Institutional Background

During our period of study, South Korea's real GDP per capita increased from $1,275 (1962) – below most of Sub-Saharan Africa[12] – to $24,834 (2001).[13] Figure 1 highlights that exports

[11] as well as more heterogeneous methodologies: Mollick (2012); Lazear, Shaw, and Stanton (2015); Bandiera, Prat, Hansen, and Sadun (2020); Frederiksen, Kahn, and Lange (2020); Hoffman and Tadelis (2021); Adhvaryu, Nyshadham, and Tamayo (2022); Adhvaryu, Kala, and Nyshadham (2022); Friebel, Heinz, and Zubanov (2022); Patault and Lenoir (2024).

[12] The countries with higher GDP per capita in 1962 in Sub-Saharan Africa in order of 2023 population: Nigeria, the Democratic Republic of the Congo, South Africa, Kenya, Ghana, Madagascar, Côte d'Ivoire, Cameroon, Niger, Zambia, Chad, Senegal, Zimbabwe, Guinea, Benin, Togo, Republic of the Congo, the Central African Republic, Liberia, Mauritania, Gambia, Namibia, Gabon, Mauritius, the Comoros, the Seychelles.

[13] Both numbers are in 2017 USD. Relative to the U.S., this corresponds to an increase from 1/16 of real GDP per capita in 1961 to 1/2 in 2001. Data from Penn World Tables.

experienced particularly stark growth. Korean exports per capita in 1952 were below 2% of the U.S. level with little convergence between 1952 and 1960. From 1960 on, exports increased rapidly, reaching parity with the U.S. before the end of the century. Over this period, other developing countries saw little growth relative to the U.S.[14]

It is often argued that this growth is partly due to industrial policies conducted by a well-functioning state.[15] On the other hand, the Korean state was described as aid-dependent and corrupt until the 1960s (Kim and Vogel, 2011).[16] These contradicting narratives, summarized in section 2.1, motivate us to study how much a particular industrial policy's effect was due to the capacity with which it was implemented. We focus on overseas export promotion, a policy area highlighted in a representative survey of Korean manufacturers from 1976 as the one where government intervention improved the most under President Park Chung-hee (1961-1979) compared to Syngman Rhee (1948-1960) (Jones and Il, 1980).

2.1 Bureaucratic Capacity and Korean Economic Development

Qualitative political economy attributes East Asia's rapid economic growth to successful industrial policy (Johnson, 1982; Amsden, 1989; Wade, 1990; Evans, 1995; Woo-Cumings, 1999). These accounts either explicitly or implicitly argue that South Korea's high state and bureaucratic capacity was essential to the economic and export growth it experienced from the mid-1960s. Amsden (1989), perhaps the most influential account of South Korean industrial policy, emphasizes that the "the power of the state to discipline big business was greater in Korea – and Japan and Taiwan as well – than in other late-industrializing countries" (p. vi). Jones and Il (1980) further highlight the importance of implementation and adaptation in South Korea's industrial policies, "only possible to governments possessing a well-trained bureaucracy" (p. xxxi, foreword by Edward S. Mason).[17]

Conversely, other historical research describes South Korea's *lack* of bureaucratic capacity in the 1950s and 1960s: "Under Syngman Rhee the bureaucracy was generally both ineffective and disorganised, characterised by widespread corruption and patronage. Not only were policy instruments used for political purposes, but the staffing of the bureaucracy itself was an important form of patronage [Suh (sic), 1967]." (Cheng, Haggard, and Kang, 1998; Bark, 1967).[18] The limited capacity of the state is further underlined by Kim and Baik (2011):

[14] Even the growth in Chinese exports under Deng Xiaoping (from 1978) is moderate relative to South Korea's export growth between 1960 and the late 1980s.

[15] Wade (1990) and Cheng et al. (1998) as cited by Besley et al. (2022); Amsden (1989); Juhász, Lane, and Rodrik (2023). See also the well-known popular book by Studwell (2013). Economists remain broadly skeptical that industrial policies are desirable, including in the case of South Korea's recent history.

[16] Korea's level of state capacity may be highlighted by the lack of continuity in its ministries. Between 1948 and 1960, under President Rhee, the average agriculture minister lasted just 9 months. The average commerce minister lasted 13 months (Haggard, Kim, and Moon, 1991).

[17] Mason argues that this was particularly important for a government that intents to apply "discretionary command procedures" in addition to non-discretionary policies. Overseas export promotion may be considered as very discretionary, as the countries targeted, and the specific services supplied to which sector in a given country, are largely up to the decision of the bureaucrat assigned to the country.

[18] Cheng, Haggard, and Kang (1998) describe the Rhee bureaucracy in unfavourable terms except regarding

"South Korea lacked the expertise necessary for modern government and frequently relied on American advisors to strengthen state capabilities". As over 90 percent of the government budget in 1961 was funded by U.S. aid, U.S. advisors were "overseeing and shaping South Korea's major social and economic policies for all practical purposes."

These vastly different perspectives may, in some part, be due to hindsight bias; authors around 1990 may have attributed high bureaucratic capacity to 1960s South Korea partly because, at the time of writing, they were observing a newly rich and democratic country with a well-functioning bureaucracy. Overall, assessing the importance of South Korean bureaucratic capacity on subsequent economic growth is complicated by the difficulty of establishing the link causally. We provide a step forward by causally identifying the effectiveness of individual bureaucrats in implementing the effect of one important policy pursued by the South Korean government.

2.2 Export Promotion by KOTRA: Tasks and Outputs Produced

We study the overseas offices of South Korea's Trade Promotion Agency (KOTRA) founded in 1962. At its inception, KOTRA was tasked with "promot[ing] the increases of exports. In order to accomplish this goal, its functions include[d] sales promotion and research, a campaign of public relations and advertising, [and] information service to exporters and importers" (Udell, 1965). Figure 2 displays the countries where an overseas KOTRA office opened between 1962 and 2001. Most of these offices remained open from the indicated date until 2000, the end of the sample period.[19]

While the South Korean government had many policies aimed at increasing exports, KOTRA's overseas offices each targeted their efforts at exports *to a particular destination country* – making variation in the work of the overseas offices plausibly orthogonal to other industrial policies by the South Korean government, even if these targeted particular sectors or regions within South Korea.

The overseas offices contributed to three main functions of KOTRA that were maintained consistently from the early years of the organization's establishment throughout the period of study. First, KOTRA's "Investigation/Research" division investigated factors related to export supply and demand with a focus on (1) South Korea's capability to supply a product for exports and (2) the import demand in a particular foreign market. The overseas offices produced reports by product and country that were compiled and published by the head office.

Second, the overseas offices served a key role in the "Market Development" division by helping domestic producers and retailers find new trade partners in new and existing markets.

their ability to extract long-term aid commitments from the United States. Jones and Il (1980) also note that corruption was widespread during the Rhee presidency.

[19] Appendix figure A.1 displays the number of countries with an overseas KOTRA office over time. By 1970, offices had opened in 32 countries. This number rose to 75 by 1981. Then the pace slowed. In 2000, the 88th office opened in Algeria.

This was achieved through direct matchmaking and by collecting and publicizing inquiries for imports from Korea by foreign firms, as well as export inquiries from domestic companies. These inquiries were published in KOTRA's Daily Market Newspaper. Business transactions were then mediated between the inquirers and respondents.

Third, the overseas offices helped the "Trade Fair" division with the organization of a South Korean pavilion at international trade fairs, which were viewed as an efficient way to generate export results in a short time by allowing exporters to engage in direct conversations with local buyers. The overseas offices coordinated logistics and handled strategic tasks such as recruiting, selecting, and briefing exporters. Domestic companies were selected based on newly trending styles and designs that aligned with the market demands at the fair venues. Prior to the fairs, they disseminated information about the exporters and their products to attract potential buyers. The bureaucrats ran ads, sent letters and made phone calls to promising exporters and foreign buyers, and reached out to trade associations.

Compared to other bureaucracies, KOTRA's overseas offices had a large degree of discretion regarding how to carry out the task of promoting exports. For this reason, we focus on KOTRA's ultimate outcome of interest: exports. It appears difficult to centrally plan whether exports to a particular destination will benefit more from market reports or networking with potential importers, and whether networking should happen through fairs, phone calls, or some other channel. Instead, such a goal relies on the bureaucrats' knowledge, which may be both tacit and local, and requires substantial improvisation. Hence, rather than having a centrally mandated list of tasks to fulfill,[20] KOTRA office managers shared similarities with the proverbial "man on the spot" charged with the running of an entire geographic region in the Indian Administrative Service (Bertrand, Burgess, Chawla, and Xu, 2020) or the British colonial administration (Lugard, 1926; Xu, 2018). However, unlike these bureaucrats responsible for a range of policies and outcomes, KOTRA bureaucrats implemented exactly one policy with one clearly defined target, which can largely be summarized as exports during their appointments. The primary performance measure, as assessed by the head office, was whether export targets were met – supported by figure 8, which shows that bureaucrats saw fewer future appointments if exports underperformed during their first appointments. Thus, KOTRA bureaucrats were much less susceptible to the multi-tasking problem faced when evaluating the effectiveness of most bureaucrats with regional responsibilities. Finally, the outcome targeted by overseas offices, exports to the country, is an outcome of direct importance for economic growth and development.

2.3 Assignment of Managers to KOTRA's Overseas Offices

Over the entire time period from 1965 to 2000, KOTRA operated 138 offices in 87 countries. The analysis will focus on the main country offices as data on the outcome – exports – is

[20] Or managing people who have a list of tasks to fulfill, as in Bandiera, Best, Khan, and Prat (2021); Fenizia (2022); Best, Hjort, and Szakonyi (2023).

available at the country level.[21]

Official rules do not dictate which bureaucrat gets assigned to which office. The assignment system falls under the discretion of the HR team at the head office. According to interviews we conducted with current and former KOTRA employees, however, there is a general understanding that several factors come into play. The most important factor is language skills; a Spanish speaker is more likely to get sent to a Hispanophone country. Second, a bureaucrat who was previously posted to an undesirable location, such as a small, low income country far from Korea, might be compensated by getting posted to a desirable location next. Lastly, connections with KOTRA executives may matter for assignments to desirable locations.

Organizational rules do, however, provide substantial rigor regarding the timing of appointments. The regular nature of these directors' appointments is highlighted by the fact that both the modal and median appointment duration is 36 months – three years. Appendix figure A.2 plots the distribution of appointment durations. Between appointments, managers return to South Korea, typically working at KOTRA's headquarters in Seoul and sometimes at regional offices. The timing of their re-appointment is also largely pre-determined: The median duration for the gap between appointments is 29 months, the modal gap is 30 months. Appendix figure A.3 plots the distribution of gaps between appointments. This rotation limits its discretion in appointments as not all bureaucrats are available when a particular country is due to receive a new bureaucrat.

More importantly, the rotation schedule provides exogenous variation in the bureaucrat appointed to manage a country office. In particular, while there is discretion to the decision of appointing bureaucrat b to country c in year t, this decision then largely pre-determines losing bureaucrat b in year $t + 3$.

2.4 KOTRA and Korea's Largest-Scale Industrial Policy

One reason for studying export promotion is the narrative of South Korea's development as being export-driven, as well as exports' prominent role in South Korean industrial policy. Korea's largest scale industrial policy, the Heavy and Chemical Industries drive (HCI), commenced in early 1973 and ended in October 1979.

Appendix figure A.4 displays how the targeting of KOTRA's activity changed over time. Before the HCI drive, only 15-25% of product-specific reports discussed HCI products.[22] During the HCI drive, this share increased rapidly, reaching nearly half of all reports in the late 1970s. After the HCI drive, the share of reports targeting these sectors remained roughly

[21] The most important or geographically largest countries have multiple offices in different cities. For example, by 1977, KOTRA had 79 overseas offices, of which 64 were the respective country's head office.

[22] To show the connection between KOTRA and HCI, we linked about 45,000 of the reports written by KOTRA's overseas offices between 1965 and 2001 to the products or sectors discussed by each report. When discussing whether a product was treated by HCI, we follow Lane (2025), who included those "listed in the enforcement decrees and national sectoral acts underlying HCI". HCI's six broadly defined target sectors included steel, nonferrous metals, shipbuilding, machinery, electronics, and petrochemicals.

constant. These patterns show that export promotion formed a part of other well-studied industrial policies.

At the same time, it is worth noting that national sectoral industrial policies, such as the HCI drive, targeted particular sectors. In our study, we remove the effect of such policies by netting out product-year fixed effects.

3 Data

Our main analyses uses data on bureaucrat appointments to explain Korean exports. This is complemented with additional data regarding the three main functions of KOTRA's overseas activities.

3.1 Bureaucrat Appointments

The most complete source of bureaucrat appointments is contemporaneous reports on appointments to KOTRA's overseas offices published in major South Korean newspapers. In most years, there were two main dates at which appointments were announced, usually in January and July. The actual appointments most frequently started in April and October. Further, this information is usually reported in three major newspapers (*Dong Ah Ilbo*, *Choson Ilbo*, and *Kyonghyang Sinmun*). Because of these overlapping information sources there are almost no rounds of announcements that were not reported by any newspaper. For almost all rounds of announcements, we were able to corroborate the information using at least two of these sources.

The newspaper announcements are further complemented and verified using a variety of KOTRA publications on the director in charge of an office at a given point in time. We obtained and digitized the names of bureaucrats in (i) monthly publications aimed at non-Korean importers (1966-1971), (ii) a directory of KOTRA's network including all of its overseas bureaucrats (1977, 1991-1994, 1998-2000), (iii) KOTRA's reports on trade fairs (1969, 1971-1997), and (iv) a full directory of all overseas office directors using the Korean Business Directory, published by the Korean Chamber of Commerce and Industry.

Overall, we are able to identify 138 offices that existed between 1962 and 2001, located in 87 distinct countries. We identify 475 unique directors and 974 unique appointments of directors to offices. Table 1 provides additional descriptive statistics on directors and appointments.

Managers are identified using their names, which requires us to avoid two types of errors. First, we may erroneously code two bureaucrats as the same one, e.g, it may be that bureaucrats share names. A priori, this could have been a problem as 45% of bureaucrats in our sample share the last names *Kim*, *Lee*, and *Park*.[23] However, this is remedied by a

[23]Moreover, the top 15 last names account for 76% of bureaucrats.

great diversity in first names.[24] After a plethora of checks, it appears very unlikely that any bureaucrats in our data share the exact same full name. More challenging in practice, we have to determine whether slightly different names truly refer to distinct individuals. This task is complicated as over time our sources move from Chinese to Korean characters to render the bureaucrats' names. In addition, in the few cases where names are given using romanizations, inconsistent romanization is used, e.g. both *rhee* and *lee* to render the same Korean syllable. We resolve this challenge in four steps: Identify incorrectly spelled or digitized names by (1) matching very unusual names to more common ones, (2) harmonizing the rendering of certain syllables, e.g. *rhee* and *lee*, (3) identifying offices with likely mistakes, e.g. the director's name flips back and forth, and (4) re-creating the career of each bureaucrat and assessing patterns of overlap or missing years. Following these steps meticulously allowed us to create a consistent panel of unique bureaucrats covering all offices and all years.

3.2 Exports

Our main measure of exports comes from Feenstra and Romalis (2014) who create consistent measures of bilateral trade flows, based on UN Comtrade data, at the year and 4-digit product level starting in 1962 and covering the entire period, up to 2001. Examples of these 4-digit products are given by "Rails of iron or steel", "Aircraft, heavier than air", and "Fur clothing".

4 Do Bureaucrats Drive Korean Exports?

This section finds that the effect of a South Korean overseas export promotion office differs substantially depending on the bureaucrat managing it: Increasing the ability of the bureaucrat by one standard deviation increases exports to the respective destination country by 37%. This effect is comparable to that of opening an office for the first time, estimated in section 5. This implies that the policy under study would have no effect if every bureaucrat's ability is reduced by one standard deviation.

4.1 Identifying Bureaucrat Fixed Effects

We adapt the AKM framework to study how much bureaucrats matter in explaining South Korean exports (Abowd, Kramarz, and Margolis, 1999). We proceed in two steps. In the current section, we obtain unbiased estimates of bureaucrat fixed effects and check that identifying assumptions are met. In section 4.2, we use the estimated fixed effects to obtain measures of the variation in exports explained by bureaucrat abilities, while correcting for variation in sampling error to isolate the true, signal variance.

[24] Only twenty first names occur more than once. Only two first names occur three times in our data (*Dae-gyun* and *Won-kyung*).

$$y_{cpt} = \lambda_{pt} + \gamma_c + \theta_{b(c,t)} + \epsilon_{cpt} \tag{1}$$

We model the relationship between the inverse hyperbolic sine (IHS)[25] of South Korean exports – henceforth "exports" – to country c of product p in year t, and the bureaucrat assigned to that country-year, denoted by $b(c,t)$. Exports are modeled as the sum of a product-year component (λ_{pt}), a bureaucrat component ($\theta_{b(c,t)}$),[26] a country component (γ_c), and an error term (ϵ_{cpt}). As in other parts of the paper, exports are at the product-level, in line with KOTRA's export targets being typically product-specific. It also avoids a few dominant export products driving the results for a country-year.

The AKM estimation based on equation (1) identifies the bureaucrat and country fixed effects only within the largest connected set.[27] Additionally, it requires that manager mobility is as-good-as-random, conditional on product-year and country fixed effects. In other words, bureaucrat assignments need to be orthogonal to ϵ_{cpt}, which we refer to as *underlying trends in exports*. On the other hand, this orthogonality condition allows for manager assignment to offices on the basis of the permanent component of country effects γ_c or the permanent component of manager ability $\theta_{b(c,t)}$. That is, sorting of better bureaucrats to destinations with greater time-invariant South Korean exports, e.g. to larger or richer countries, would not violate this identifying assumption.

4.1.1 Descriptives: Bureaucrats and Countries Form One Large Connected Set

Table 1 describes the structure of the sample. The full sample contains 736 appointments of 399 bureaucrats to 87 country offices. The largest connected set among these contains all appointments to 86 out of 87 countries.[28] This connected set contains 735 appointments of 398 managers, of whom 184 were appointments to multiple offices.[29]

The bureaucrat-country graph is interconnected enough such that 75 countries and 93% of appointments form a leave-one-out connected set. The reason is that most country offices remain open for decades, headed by many different office managers. Column 4 indicates that

[25] We explore robustness to the IHS transformation. We show in Appendix section C.2 that bureaucrat fixed effects are predictive of export changes along both the extensive and intensive margins. Furthermore, we show in Appendix section C.3 that the fixed effect estimates themselves are robust to alternative transformations: (1) just using the extensive margin, and (2) explicitly weighting the value of the extensive margin against the intensive margin, a solution to the unit-dependent nature of IHS proposed by Chen and Roth (2023).

[26] To account for the fact that it takes time for a new manager to influence exports, we code each country-year as being managed by the bureaucrat in office until March that year. This means, we attribute effects to a bureaucrat for up to nine months after their successor has been appointed.

[27] Appendix D illustrates the concept of a connected set and leave-one-out connected set in our setting.

[28] Only Cambodia is outside the largest connected set because it was only ever appointed one bureaucrat who was never appointed to any other country. The data only contains one appointment to Cambodia because its office opening occurred shortly before the end of our sample period.

[29] 184 happens to be the same number as in the balanced analysis sample of Fenizia (2022). In Fenizia (2022), these moves result in 143 distinct connected sets – while in our case the moves connect all offices in one single connected set (except for Cambodia).

in the leave-one-out connected set, 72 offices have four or more distinct office managers, 61 offices have six or more managers, 49 offices even have eight or more distinct managers.

Our preferred estimation uses only the appointments in the largest leave-one-out connected set. That way, we can apply Kline, Saggio, and Sølvsten (2020)'s correction method for limited mobility bias in our variance decomposition exercise. Moreover, Kline, Saggio, and Sølvsten (2020) show that restricting attention to the leave-one-out connected set substantially reduces limited mobility bias.

4.1.2 Identifying Assumption: Appointments are Orthogonal to Export Trends

This section discusses how factors influencing bureaucrat appointments relate to the identifying assumption that appointments are orthogonal to export trends.

The central factor driving the movement of bureaucrats is their three-yearly rotation schedule. As highlighted in appendix figure A.2, a new appointment to country c in year t usually occurs if the previous bureaucrat's appointment to country c occurred in year $t-3$. This has two important implications that allow us to investigate the assumption that bureaucrat appointments are as good as random with respect to export trends.

First, suppose KOTRA appoints good bureaucrats to countries with increasing export trends, a violation of the identifying assumption. In that case, we would observe growing exports prior to the appointments of good bureaucrats. Even if KOTRA appoints good bureaucrats to countries where export trends are *due* to increase, the rigid rotation schedule would impede them from timing all such appointments to precisely the years when exports start to rise. Then, the increasing trend would have materialized before some appointments, once again leading to differential pre-trends by bureaucrat ability. Section 4.4 tests this hypothesis in an event-study setup for export changes around the event of bureaucrat switches. We find parallel pre-trends.

Second, while KOTRA has some discretion in new appointments, the rigid rotation schedule dictates when a country *loses* a bureaucrat. Suppose good bureaucrats are strategically appointed to countries with increasing export trends. The greater discretion at the start of an appointment, compared to the end, would imply that exports rise more when gaining a bureaucrat than they fall when losing a bureaucrat of the same ability. Section 4.4 tests this hypothesis in the event-study setup and finds that the effects of gaining and losing a bureaucrat are almost perfectly symmetric. This result also rules out other alternative hypotheses that violate the identifying assumption. For example, the identifying assumption would be violated if the South Korean government synchronized new investments into exporting to particular countries with new bureaucrat appointments. However, the symmetric effects we find contradict this hypothesis, unless the investments are reversed at the end of the bureaucrat's term.

In addition to these testable implications, the rotation schedule also constrains the pool of bureaucrats available when a country office needs a new manager, restricting the potential for strategic appointments. In qualitative interviews, KOTRA employees mention two additional

constraints on the appointment decisions. (1) Bureaucrats are more likely to be appointed to a country when they speak the local language. (2) Bureaucrats prefer being appointed to high-income, English-speaking countries. Because these preferences are commonly shared, KOTRA's Human Resources manages discontent by rotating bureaucrats between low- and high-desirability locations. In most cases, both a country's language and its income relative to other countries change little over time. Therefore, while appointments may be correlated with country fixed effects,[30] the scope for appointing bureaucrats based on anticipated export trends is limited.

Despite these constraints, one may still wonder why KOTRA does not strategically appoint good bureaucrats to countries with growing import demand. There would be no reason to, if there are no match effects between bureaucrats and countries, as specified in equation (1) and verified through various misspecification checks in Appendix section C.1. Even if KOTRA expected there to be positive match effects, it could be that KOTRA prioritized time-invariant country characteristics over trends in gauging export potential, much like it did for office openings.[31]

4.2 The Share of Variation in Exports Explained by Bureaucrats

This section explains how we decompose the variance in South Korean exports to estimate how much of it is due to differences between bureaucrats. To focus on the bureaucrat and country components, we first residualize exports for product-year fixed effects:

$$(\text{exports}|pt)_{cpt} \equiv \text{exports}_{cpt} - \hat{\lambda}_{pt} = \theta_{b(c,t)} + \gamma_c + \epsilon_{cpt} \tag{2}$$

where $\hat{\lambda}_{pt}$ is estimated from equation (1).[32] The remaining variation in exports can be decomposed into:

$$\mathsf{Var}[(\text{exports}|pt)_{cpt}] = \mathsf{Var}(\theta_{b(c,t)}) + \mathsf{Var}(\gamma_c) + 2\mathsf{Cov}(\theta_{b(c,t)}, \gamma_c) + \mathsf{Var}(\epsilon_{cpt}) \tag{3}$$

As the variation in residualized exports *within* appointment spells – i.e., across years or products for a bureaucrat in a particular appointment – is uninformative in the estimation of the bureaucrat or country fixed effects, we take the spell-level averages of the residualized exports as the total variation.[33]

Our primary object of interest is the variation explained by the bureaucrats: $\mathsf{Var}(\theta_{b(c,t)})$.

[30] Recall that a correlation between bureaucrat ability and the time-invariant country effects would not violate the identifying assumption.

[31] The roll-out of export promotion offices mainly followed pre-determined gravity variables, as reported in figure 11 and discussed in section 5.3.2.

[32] The calculation of $(\text{exports}|pt)_{cpt}$ follows Chetty, Friedman, and Rockoff (2014b). To remove the effect of pt without biasing the bureaucrat or country effects, $\hat{\lambda}_{pt}$ needs to be estimated using only within-bureaucrat and within-country variation. $\hat{\lambda}_{pt}$ captures macroeconomic shocks, but also long-run changes in South Korea's industrial structure. For instance, $\hat{\lambda}_{\text{cars},1965}$ is very small compared to $\hat{\lambda}_{\text{cars},1995}$. Table B.2 demonstrate the importance of product-year trends, which explain 35.5% of the total variation in exports.

[33] The bureaucrat and country fixed effects estimated from the collapsed data equal those estimated from the uncollapsed, raw data. The variance of the raw (i.e., country×product×year-level) residualized exports is also reported in Table 2 for reference.

The challenge in obtaining an estimate for $\mathsf{Var}(\theta_{b(c,t)})$ is that a naive plug-in estimator, $\mathsf{Var}(\hat{\theta}_{b(c,t)})$, would be biased upwards due to sampling error in $\hat{\theta}_{b(c,t)}$, which is aggravated when bureaucrats move countries too infrequently (limited mobility bias). Kline, Saggio, and Sølvsten (2020) estimate the degree of bias in $\mathsf{Var}(\hat{\theta}_{b(c,t)})$ under unrestricted heteroskedasticity, building on previous approaches which required homoskedastic error terms (Andrews, Gill, Schank, and Upward, 2008).

Our preferred variance decomposition approach follows the bias correction method of Kline, Saggio, and Sølvsten (2020). We use the computational algorithm of Bonhomme, Holzheu, Lamadon, Manresa, Mogstad, and Setzler (2023) for implementation. Although unreported, the Andrews, Gill, Schank, and Upward (2008) correction method that assumes homoskedasticity delivers quantitatively very similar results.[34]

One downside of the analysis based on Kline, Saggio, and Sølvsten (2020) is that it does not allow us to make statements about some other moments of the distribution of bureaucrat abilities, e.g. percentiles. An alternative approach involves shrinking each raw fixed effect to minimize its mean-squared-error.[35] We use covariance shrinkage, developed by Best, Hjort, and Szakonyi (2023), which extends the standard shrinkage method (Chetty, Friedman, and Rockoff, 2014a, Kane and Staiger, 2008) to account for the correlation between the two dimensions of fixed effects – including the negative correlation in the sampling error of bureaucrat fixed effects with that of country fixed effects caused by limited mobility bias.[36] We bootstrap equation (2) at the appointment level to estimate the variances and covariances of the sampling errors.[37] Covariance shrinkage has the advantage of yielding shrunk fixed effects for each bureaucrat ($\hat{\theta}^{\text{CovSh}}_{b(c,t)}$), allowing us to compare different parts of the distribution, e.g. the 25th and 50th percentile.

The covariance shrinkage procedure inspires another bias correction method for the variance decomposition. Armed with the estimates of the variances and covariances of sampling errors, we can calculate the bias in the plug-in estimators, $\mathsf{Var}(\hat{\theta}_{b(c,t)})$, $\mathsf{Var}(\hat{\gamma}_c)$, and $\mathsf{Cov}(\hat{\theta}_{b(c,t)}, \hat{\gamma}_c)$. We subtract the estimated biases to isolate $\mathsf{Var}(\theta_{b(c,t)})$, $\mathsf{Var}(\gamma_c)$, and $\mathsf{Cov}(\theta_{b(c,t)}, \gamma_c)$. We call this method "bootstrap bias correction."[38] This method builds on Best et al. (2023) by recognizing that the variances or covariances of the shrunk fixed effects,

[34] The Kline, Saggio, and Sølvsten (2020) correction method can only be performed on the leave-one-out connected set which covers 75 countries and 380 bureaucrats, because the bias is an aggregation of observation i-specific bias estimated using a leave-i-out estimator. In contrast, the Andrews, Gill, Schank, and Upward (2008) correction method can also be performed on the largest connected set covering 86 countries and 397 bureaucrats. The two methods delivers extremely similar results for either measure of connectedness.

[35] Although each fixed effect is unbiased, it is noisy. By shrinking them towards the mean value, we introduce bias for reduced variance.

[36] We discovered a minor error in Best et al. (2023)'s derivation of the covariance shrinkage matrix. We use the corrected matrix for our covariance shrinkage. The resulting covariance-shrunk fixed effects are mostly similar to the Best et al. (2023) counterparts but sometimes diverge considerably. See Appendix section E.3 for our derivation and E.6 for the comparison with Best et al. (2023)'s.

[37] We bootstrap at the appointment level because, as discussed before, it is the variation across appointments that identifies the bureaucrat and country fixed effects.

[38] Derivations are in Appendix section E.1.

whether from standard shrinkage or covariance shrinkage, do not consistently estimate the true values.[39] In fact, the variance of the covariance-shrunk bureaucrat fixed effects theoretically underestimates the true variance,[40] while in practice, it might overestimate the true variance.[41]

4.3 Result: Bureaucrats Drive Korean Exports

Figure 3 plots the cumulative distribution function of raw bureaucrat fixed effects. It shows substantial dispersion, although it overstates the true dispersion. Appendix figure A.7 compares the distributions of the raw fixed effects and the covariance-shrunk fixed effects.

Table 2 reports the variance decomposition results, correcting for bias using the Kline et al. (2020) method (preferred) in columns 1-2 and the bootstrap bias correction method in column 3-4. The two bias correction methods yield similar results overall. The variance of the bureaucrat component is remarkably similar.

Bureaucrats explain a substantial amount of variation in Korean exports. Columns 2 and 4 show that they explain around 14% of the variation. Furthermore, column 1 shows that one standard deviation of bureaucrat ability is estimated to be 0.318,[42] implying an increase in the dollar value of exports of 37%.[43] Moreover, the magnitude is comparable to the policy's average effect – the effect of opening an office – of 0.321 (38%), estimated in section 5. Hence, opening an office would not impact exports if the office is headed by a bureaucrat whose ability was one standard deviation below the mean.[44]

To put the magnitude into perspective, we compare the effect of a one standard deviation increase in bureaucrat ability to that of geographical distance. Assuming an elasticity of trade to distance of -1 (Anderson, 2011; Head and Mayer, 2014), it amounts to roughly the effect of reducing trade distance from London-Seoul (8,900km/5,500 miles) to Mumbai-Seoul (5,600km/3,500 miles).

Both columns 1 and 3 show that bureaucrats explain around 1/7 as much variation as countries do. The 1/7 number is relatively small compared to effect sizes in recent studies of public-sector managers using similar methodolody. For example, managers in Fenizia (2022) explain 1/3 as much variation in the processing of social insurance claims as the

[39] Best et al. (2023) explain that covariance shrinkage is their preferred variance decomposition approach because the correlation between the covariance-shrunk bureaucrat and organization fixed effects is positive, a more plausible outcome, while other approaches they consider yield negative values. Yet, the positive correlation may partly be mechanically driven by positive off-diagonal elements of the covariance shrinkage matrix. In our setting, we find that $\mathsf{Cov}(\hat{\theta}^{\text{CovSh}}_{b(c,t)}, \hat{\gamma}^{\text{CovSh}}_{c}) > 0$ while $\mathsf{Cov}(\theta_{b(c,t)}, \gamma_c) < 0$. See Appendix section E.4 for why the off-diagonal elements of the covariance shrinkage matrix may likely be positive.

[40] Shown in Appendix section E.3.

[41] Appendix E.5 describes the conditions under which a few covariance-shrunk fixed effects might blow up, which would expand the variance.

[42] $0.318 = \sqrt{0.101}$

[43] We approximate the percentage change using $(e^{0.318} - 1) \times 100 \approx 37$.

[44] Under the simplification that the estimated effect of office opening reflects the office's true effect under an average bureaucrat.

offices they manage, and those in Otero and Muñoz (2022) explain 3/4 as much variation in mortality as the public hospitals they manage.[45] The comparatively small contribution of KOTRA bureaucrats is not surprising, given that the country fixed effects represent much more than just the overseas offices. The country fixed effects encapsulate time-invariant gravity variables, such as distance and market size, which have been found to be very strong predictors of bilateral trade volume.

We find a negative covariance between bureaucrat and country fixed effects, suggesting that better bureaucrats work in smaller countries. The negative covariance is almost exclusively driven by single-appointment bureaucrats.[46] For multi-appointment bureaucrats, the covariance is negative but very close to zero. Moreover, we find that the average bureaucrat effect and the average country effect for multi-appointment bureaucrats are both larger than those for single-appointment ones, an observation we discuss at length in section 4.6.1. Overall, bureaucrat and country fixed effects jointly explain around 90% of the spell-level variation in exports (after removing product-year fixed effects).

4.4 Diagnostics

In this section, we perform several diagnostic checks to assess the validity and reliability of our fixed-effect estimates. We begin by presenting evidence supporting the identifying assumption, discussed in detail in Section 4.1.2. When this assumption holds, our bureaucrat fixed effect estimates are unbiased, but they may still be noisy. We conclude this section by showing that they are not driven by noise. Additional diagnostic checks are provided in Appendix section C, corroborating the additive separability assumption of bureaucrat and country effects, as well as the robustness to different transformations of export value.

4.4.1 Are Bureaucrat Appointments Orthogonal to Export Trends?

We estimate event-study regressions to alleviate concerns that the appointment of KOTRA bureaucrats may not be orthogonal to underlying export trends, a violation of the identifying assumption that results in biased estimates.

As explained in section 4.1.2, if KOTRA strategically appointed high-ability bureaucrats to countries with increasing export trends, we would expect to observe differential pre-trends by ability, as well as asymmetric effects of gaining and losing a bureaucrat.

[45] Other papers studying bureaucrats in non-management roles similarly find that individuals matter more than in our setting. Best, Hjort, and Szakonyi (2023) find that individual procurement agents explain similar shares of the variation in procurement prices as the agencies for which they work. Dahis, Schiavon, and Scot (2023) find that judges matter 2/3 as much as courts in determining the number of cases disposed. Outside the public sector, Metcalfe, Sollaci, and Syverson (2023) find that retail store managers explain 58% as much variation as store fixed effects in determining sales.

[46] The total covariance between bureaucrat and country effects can be decomposed into: a) the covariance within the group of single-appointment bureaucrats, b) the covariance within the group of multi-appointment bureaucrats, and c) the covariance between the average bureaucrat effect and the average country effect across the two groups.

$$y_{ept} = \eta_{ep} + \lambda_{pt} + \sum_{k \neq -2} \Big(\alpha_k + \beta_k \cdot \mathbb{1}\{\Delta\hat{\theta}_e \text{ in top tercile}\} + \delta_k \cdot \mathbb{1}\{\Delta\hat{\theta}_e \text{in mid tercile}\} \Big) \mathbb{1}\{t = T + k\} + \epsilon_{ept} \tag{4}$$

First, to test for differential pre-trends, we estimate equation (4), which models how exports change around the event (e) of bureaucrat switches for different terciles of the change in bureaucrat ability induced ($\Delta\hat{\theta}_e$).[47] An event e is uniquely defined by the country (c) and the year of the event (T), defined as the first full year that the new bureaucrat is appointed to country c. The key parameters of interest are β_k and δ_k. β_k (δ_k) is the double-difference of exports in year $T + k$ relative to year $T - 2$, for a change in bureaucrat ability in the top (middle) tercile relative to a change in the bottom tercile. We take $T - 2$ as the base year since it is the last full year when the old bureaucrat was in charge. We control for trends using product-year fixed effects (λ_{pt}) and for pre-event levels of exports using event-product fixed effects (η_{ep}).

Figure 4 plots the estimates $\hat{\beta}_k$ and $\hat{\delta}_k$, which are statistically insignificant up to the year of the bureaucrat switch. The lack of differential pre-trends corroborates the identifying assumption. At the same time, we do see jumps in exports upon the appointment of the new bureaucrat, and more so for the top transition (30%) than the middle transition (11%), adding confidence that our bureaucrat fixed effects meaningfully capture the differences in bureaucrat abilities.

$$y_{ept} = \eta_{ep} + \lambda_{pt} + \sum_{k \neq -2} \left(\alpha_k + \beta_k \cdot \hat{\theta}_e^{\text{new}} + \delta_k \cdot \hat{\theta}_e^{\text{old}} \right) \mathbf{1}\{t = T + k\} + \epsilon_{ept} \tag{5}$$

Second, to test whether gaining a bureaucrat coincides with larger increases in exports than the decreases associated with losing one, we estimate equation (5). It models exports as a time-varying function of the fixed effects of the new bureaucrat ($\hat{\theta}_e^{\text{new}}$) and the old bureaucrat ($\hat{\theta}_e^{\text{old}}$). Other than distinguishing between $\hat{\theta}_e^{\text{new}}$ and $\hat{\theta}_e^{\text{old}}$, the specification is similar to equation (4).

Figure 5 plots the event-study estimates ($\hat{\beta}_k$ and $\hat{\delta}_k$) obtained from equation (5). It shows that exports change sharply in the direction of the ability of the incoming bureaucrat and symmetrically against the direction of the outgoing bureaucrat's ability. In addition, the pre-trends in this specification, like in Figure 4, are not statistically distinct from 0 and economically very small, providing further support that appointments are not strategically timed to coincide with increased export potential.

It may be surprising that there is a strong drop in exports upon the appointment of an ineffective bureaucrat. However, this drop is only relative to South Korean exports to other countries. Given that South Korean exports were growing at more than 35% annually[48]

[47] We consider terciles, following the literature (Fenizia, 2022; Otero and Muñoz, 2022).

[48] Between 1962 and 1981, South Korean exports increased from 57 million to 21 billion U.S. Dollars,

and all the regression equations include product-year fixed effects, losing a good bureaucrat means exports drop *only relative to this trend*.

4.4.2 Are the Fixed Effect Estimates Driven by Noise?

Next, we address concerns about overfitting: that the bureaucrat fixed effect estimates capture not only the true underlying bureaucrat ability but also the noise or idiosyncratic variations in the sample data, an issue aggravated when the number of observations per bureaucrat is small. The variance decomposition results in section 4.2 already account for this noise, but we perform a series of checks to reinforce the reliability of the estimates.

First, we conduct an out-of-sample predictiveness test. An implication of overfitting is that the fixed effect estimates have low predictive power outside the sample. We directly test and confirm that our estimated bureaucrat effects are, indeed, predictive out of sample.[49] The most natural and conservative way in our setting to obtain fixed effects that are testable out of sample is to only use *other countries* to estimate the fixed effects. For example, to estimate the fixed effects of bureaucrats appointed to the UK, we obtain their fixed effects when excluding the UK from the sample. The estimation of such out-of-sample fixed effects is only possible due to the interconnectedness of our data. When leaving out one country, we always retain one very large connected set, as 75 countries in our data are part of the same leave-one-out connected set.

Figure 6 displays a binned scatterplot of residual exports on in-sample (i.e., baseline) and out-of-sample bureaucrat fixed effects. By construction, the slope for the in-sample fixed effects equals 1. More interestingly, the out-of-sample fixed effects do strongly predict residualized exports, with a slope coefficient of 0.52.[50] Furthermore, we replicate the event-study regression depicted in Figure 5 using the out-of-sample fixed effects. Appendix Figure A.8 shows that upon a switch of bureaucrats, new and old bureaucrat ability still statistically significantly predict exports in the expected way even when ability is estimated only using other countries. In addition to alleviating overfitting concerns, these findings also strengthen the support for the identifying assumption. The fact that estimated bureaucrat effects are predictive out-of-sample makes it implausible that they are primarily capturing correlations between bureaucrat appointments and underlying export trends.

Second, we conduct a robustness check using the bootstrap bias correction. The main limitation of the out-of-sample predictiveness test is that it cannot be applied to single-appointment bureaucrats, who constitute about half of all bureaucrats and whose fixed

implying an annual growth rate of 36.5%.

[49] Additionally, table B.2 reports limited additional explanatory power from allowing bureaucrat effects to differ between appointments; the gains in the adjusted R^2 is minimal when we replace bureaucrat fixed effects with bureaucrat×country fixed effects in estimating equation (1) (columns 3 and 4). The stability of the bureaucrat fixed effects across appointments indicates that we are capturing true signals rather than mere noise.

[50] This value is very close to the relationship between a retail store manager's pre-Covid and Covid performance found by Metcalfe, Sollaci, and Syverson (2023), who study managers of retail stores.

effects are the most susceptible to overfitting. The leave-i-out estimation of the bias by Kline et al. (2020) faces the same challenge, so the i-specific variance of the sampling error for a single-appoint bureaucrat is assumed to equal the average among multi-appointment bureaucrats ever appointed to the same country – possibly an underestimate. In contrast, the bootstrap bias correction does not require this homogeneity assumption, as bootstrapping equation (2) yields variance estimates for all bureaucrats.[51] The fact that both correction methods produce extremely similar estimates of the true dispersion in bureaucrat ability (columns 1 and 3, Table 2) offers confidence that we are appropriately accounting for noise also for single-appointment bureaucrats.

Third, we perform a placebo check. We apply the variance decomposition on synthetic data where bureaucrats are randomly shuffled to countries. Importantly, we maintain the number of appointments of each bureaucrat in the random shuffling to preserve the connectivity of the graph.[52] The fixed effects of these *placebo* bureaucrats should capture only noise and should not have any explanatory power. Columns 5-6 of Table 2 report the results. We confirm that the total number of bureaucrats and countries, as well as the number of bureaucrats by the number of appointment spells is maintained similarly to the baseline. Remarkably, both the variation in bureaucrat fixed effects, as well as the covariance between bureaucrat and country fixed effects, go to zero, indicating that the Kline et al. (2020) bias-correction method successfully removes the contribution of noise to the variance and covariance values. This finding shows that the results in columns 1-2 are not spurious, because if they were, we would expect results in columns 5-6 to resemble them.

Last, we conduct a comparative relevance check against ambassadors' effects on exports, reported in columns 7-8 of Table 2. Instead of KOTRA bureaucrats, we consider the Korean ambassadors – who also follow rotational assignments – to each country with a KOTRA overseas office.[53] The connectivity of the ambassador-country graph closely mirrors that of the KOTRA bureaucrat-country graph. While ambassadors may influence Korean exports through various channels, such as negotiating trade agreements, resolving bilateral disputes, and networking with local business leaders, export promotion is not their prime focus. Instead, they have broad diplomatic responsibilities, making export promotion just one, if at all, of many duties. In contrast, KOTRA overseas office directors are dedicated solely to increasing exports. Therefore, if our decomposition exercise captures true effects rather than noise, we should expect to find that KOTRA bureaucrats have a larger impact on exports than ambassadors. Column 8 shows that while ambassadors do explain some variation in exports, they account for a much smaller share of the total variation (4%) than KOTRA directors (14%, column 2).

[51] As expected, the variance of the sampling error of bureaucrat fixed effects obtained this way steadily decreases in the number of appointments per bureaucrat.

[52] We identify the set of unique bureaucrat-country pairs in the original data, randomly shuffle the matches, and merge it into the original data. For example, if bureaucrat 1 was appointed to country A from 1965 to 1968, a random bureaucrat X replaces bureaucrat 1 in country A for these years.

[53] The only country that drops out is Slovenia, which has a KOTRA office but not a Korean embassy.

Overall, these results demonstrate that bureaucrat fixed effects reliably identify the causal impact of an individual bureaucrat on exports.

4.5 Mechanism: Good Bureaucrats Tap into Import Demand

In this section, we investigate how a high-ability bureaucrat increases exports by more than another bureaucrat of lower ability. We find that they raise the reactivity of Korean exports to market conditions. Upon the switch to a more effective bureaucrat, Korean exports increase more strongly for products that see increasing import demand in a given country-year. They also increase more strongly for products that see increasing export supply to other countries from Korea. Our findings suggest that much of the effect of high-ability bureaucrats stems from their ability to better exploit market conditions, for instance by relaying information about destination market demand.

$$
\begin{aligned}
y_{ecpt} =& \eta_{ep} + \lambda_{pt} + \psi^D \text{demand}_{cpt} + \psi^S \text{supply}_{cpt} \\
&+ \left(\beta^D \text{demand}_{cpt} + \beta^S \text{supply}_{cpt}\right) \times \mathbb{1}\{\Delta\hat{\theta}_e \text{ in top tercile}\} \\
&+ \left(\delta^D \text{demand}_{cpt} + \delta^S \text{supply}_{cpt}\right) \times \mathbb{1}\{\Delta\hat{\theta}_e \text{ in mid tercile}\} \\
&+ \sum_{k \neq -2} \Big[\alpha_k + \psi_k^D \text{demand}_{cpt} + \psi_k^S \text{supply}_{cpt} \\
&+ \left(\beta_k + \beta_k^D \text{demand}_{cpt} + \beta_k^S \text{supply}_{cpt}\right) \times \mathbb{1}\{\Delta\hat{\theta}_e \text{ in top tercile}\} \\
&+ \left(\delta_k + \delta_k^D \text{demand}_{cpt} + \delta_k^S \text{supply}_{cpt}\right) \times \mathbb{1}\{\Delta\hat{\theta}_e \text{in mid tercile}\}\Big] \mathbb{1}\{t = T + k\} + \epsilon_{ecpt}
\end{aligned} \tag{6}
$$

We estimate equation (6), an event-study regression around the event of bureaucrat switches. It augments equation (4) with the main and interacted effects of import "demand" and export "supply." demand_{cpt} equals the IHS of other countries' exports of the same product p to the same destination country c in year t, while supply_{cpt} equals the IHS of Korean exports of the same product p to destination countries other than c in year t. ψ^D and ψ^S estimate the elasticity of Korean exports with respect to market conditions in the base year, $T-2$. β^D, β^S (δ^D, δ^S) estimate the additional elasticity when the change in bureaucrat ability falls in the top (middle) tercile, relative to the bottom tercile, in year $T-2$. ψ_k^D and ψ_k^S allow the elasticity to market conditions to differ by event year, and $\alpha_k, \beta_k, \delta_k$ allow exports to differ by event year, for each tercile. The new parameters of interest are $\beta_k^D, \beta_k^S, \delta_k^D$, and δ_k^S. They capture the double-difference of the elasticity of Korean exports to market conditions in year $T+k$ relative to year $T-2$, for a change in bureaucrat ability in the top (or middle) tercile relative to a change in the bottom tercile.

Figure 7 plots the estimates $\hat{\beta}_k^D$ and $\hat{\delta}_k^D$ in the left panel and $\hat{\beta}_k^S$ and $\hat{\delta}_k^S$ in the right panel. We find that upon the arrival of a new bureaucrat, the elasticity of Korean exports to market conditions rises more when the increase in bureaucrat ability is greater. The patterns are very similar whether we consider the elasticity to import demand or to export supply. Thus, high-ability bureaucrats appear to conduct a more effective transmission of timely market

information.

Lastly, the estimated coefficients in the pre-period are statistically insignificant and much smaller in magnitude than those in the post-period. The parallel pre-trends in this figure, similarly to Figure 4, support the orthogonality of bureaucrat appointments and export trends.

4.6 Policy Implications: Screening Bureaucrats to Increase Policy Effect

4.6.1 1st Appointments Provide Information on Bureaucrat Ability

Our findings thus far show that individual bureaucrats' ability significantly impacts the effectiveness of export promotion policy. Then, selecting good talent becomes crucial for strengthening bureaucratic capacity. What could be a viable way for the state to select high-ability bureaucrats, when there is incomplete information on their ability, perhaps even to the bureaucrats themselves?

We show that the descriptive appointment patterns of bureaucrats are consistent with KOTRA recognizing and managing the uncertainty over bureaucrat ability.

First, residualized exports during a bureaucrat's first appointment are predictive of whether they are reappointed in the future. Figure 8 presents a visual illustration. It plots the probability density function of (a) bureaucrat fixed effects (covariance-shrunk), and (b) residualized exports by appointment, splitting the sample by whether a bureaucrat has more than one appointment as director of a country office over their career. The distributions exhibit a substantial mass of lower-ability bureaucrats whose careers only include one appointment.[54]

This visual observation is buttressed by a regression of bureaucrat's total number of appointments on residualized exports during their first appointment. We include dummies for the year of a bureaucrat's first appointment to rule out various omitted variable biases, such as the number of appointments mechanically being smaller for bureaucrats whose first appointments near the end of our sample, and systematic differences in the characteristics of bureaucrats by cohort. We find a positive and statistically significant correlation, with a regression coefficient of 0.240 (standard error: 0.112). The positive correlation survives alternative specifications. For instance, the coefficient increases to 0.430 (standard error: 0.109) when we replace first-appointment residualized exports with a dummy indicating whether they exceed the 25th percentile.

Second, first appointments tend to be to less important countries, while third appointments tend to be the most significant markets (Appendix Figures A.14 and A.15). A coun-

[54]We use covariance-shrunk bureaucrat fixed effects to correct for the fact that the raw fixed effects of single-appointment bureaucrats are noisier. Furthermore, panel (b) shows that among multi-appointment bureaucrats, there is a very close resemblance between the distributions of residualized exports in the first appointment (solid blue) and in subsequent appointments (dashed blue). This strengthens the case that the difference in single- and multi-appointment bureaucrats reflect differences in true ability rather than noise.

try's importance is proxied by how early its KOTRA office opened and how high its fixed effect is.

Together, these two patterns align with a model where the organization learns about bureaucrat ability by initially appointing them to less important countries, and then uses the information to select out low performers from getting reappointed, particularly to important countries.[55] An appointment as a manager of a small country office is plausibly much more informative about a bureaucrat's ability than a subordinate role in a more important country. Assigning bureaucrats to manage small countries may therefore be justified, even if placing them in subordinate roles in key markets could have a greater impact on overall Korean exports. Pastorino (2024) finds a similar dynamic in a private firm, where the firm shapes workers' career paths by considering how different job assignments influence how much the firm can learn about a worker's ability.

4.6.2 Crude Counterfactuals - Gains from Screening Bureaucrats

Table 3 provides four simple counterfactuals to understand how much exports could change from screening out ineffective bureaucrats based on their first-appointment performance.

First, Korean exports would rise by 6.8% if single-appointment bureaucrats were as good as multi-appointment ones. This is a relevant counterfactual as KOTRA could operate an additional set of offices in smaller countries to experiment with bureaucrats prior to appointing them to the countries in our data. The estimated difference in ability implies a 25%[56] increase in Korean exports from replacing a single-appointment bureaucrats with the average multi-appointment bureaucrat. 30% of appointments are of single-appointment bureaucrats, increasing exports by 6.8% averaging across all appointments.

Second, Korean exports would decrease by 7.1% if ability did not predict a bureaucrat's number of appointments, i.e., if there was no selection on ability. Replacing a single-appointment bureaucrat with the average across all bureaucrats increases exports by 11%. Doing the same for a multi-appointment bureaucrat decreases exports by 13%. As there are more appointments of the latter type of bureaucrats, the overall effect is negative.

Together, these two counterfactuals suggest that the organization is exploiting just above half of the possible gains from selecting bureaucrats in line with its screening ability.

Third, Korean exports would decrease by 16% if all bureaucrats were as negatively selected as the single-appointment bureaucrats.

Fourth, how does screening out single-appointment bureaucrats before the 1st appointment compare to replacing the bottom quartile with the median bureaucrat, similar to Best,

[55] Note that there could be many other explanations, as the appointments we observe are an equilibrium outcome. For example, we cannot detect voluntary quits or observe various individual-specific or organization-wide Human Resources considerations.

[56] $lim_{x\to\infty} sinh(x+0.223)/sinh(x) = 1.250$, $lim_{x\to\infty} sinh(x+0.066)/sinh(x) = 1.068$, $lim_{x\to\infty} sinh(x+0.106)/sinh(x) = 1.112$, $lim_{x\to\infty} sinh(x-0.139)/sinh(x) = 0.870$, $lim_{x\to\infty} sinh(x-0.074)/sinh(x) = 0.929$, $lim_{x\to\infty} sinh(x-0.172)/sinh(x) = 0.842$

Hjort, and Szakonyi (2023)?[57] Compared to this new counterfactual, the first counterfactual based on KOTRA's observed screening behavior exploits 58% of the gains.

5 What is the Policy's Average Effect?

This section of the paper uses the staggered roll-out of each country's first export promotion (KOTRA) office to identify the causal effect of opening an such an office on Korean exports to this country. This allows us to discuss the average effect of export promotion offices, a policy-relevant variable. More importantly for this paper's main question, the effect of an office provides a natural benchmark against which to compare the variation in exports due to individual bureaucrats. Section 4 finds that one standard deviation increase in bureaucrat ability is of a similar magnitude as the effect of opening an office. Assuming that an office's average effect corresponds to the average bureaucrat, this suggests an office with a bureaucrat one standard deviation below average has no effect on exports.

Our setting is exceptional in enabling this natural comparison between a policy's average effect and how this effect changes due to implementation by individual bureaucrats.[58] This is because (1) We observe a sufficient number of office openings and (2) South Korean exports to a country constitute a well-defined variable even in absence of an export promotion office.

Figure A.1 displays the staggered roll-out of offices: There were no offices prior to KOTRA's founding in 1962. Over the next two decades, KOTRA opened offices in 75 countries – close to four new countries per year. After this breathless initial roll-out, KOTRA's expansion ground to a sudden halt: only three new countries experienced their first office opening over the next seven years.[59] The empirical analysis in this section will focus on the initial office openings (1962-1981). Figure 2 displays the economies which had an office opening between 1962 and 2001.

Using this roll-out, we estimate a 38% increase in exports 9-11 years after the first office opening. Assuming an elasticity of trade to distance of -1 , this is equivalent to reducing the distance between London and Seoul to the distance between Mumbai and Seoul.

5.1 Identification: Effect of Office Opening on Exports

To estimate the effect of an export promotion office, the ideal experiment would randomly allocate a fully-developed office to some countries and not to others. As this is not feasible, the analysis here will use the staggered roll-out of offices to countries. It further allows for dynamic effects to account for office effects fully materializing only over time.

[57] Consistent estimates of differences between quartiles of the distribution requires using the shrunk estimates.

[58] Settings from related papers do not lend themselves to obtaining such a benchmark (Fenizia, 2022; Best, Hjort, and Szakonyi, 2023; Otero and Muñoz, 2022; Metcalfe, Sollaci, and Syverson, 2023).

[59] Only 13 openings in total over the next twenty-year period (1982–2001).

$$y_{cpt} = \lambda_{pt} + \gamma_{cp} + X_{cpt}^{T} + \sum_{k \neq -1} \theta_k D_{ct}^{k} + \epsilon_{cpt} \tag{7}$$

As a first step, we estimate the specification given by equation (7). λ_{pt} indicates product-year fixed effects, γ_{cp} indicates country fixed effects that may differ at the product-level. D_{ct}^{k} are dummies equal to 1 if year t is k years after the first office opened in country c. θ_k corresponds to the effect of an office that has been open for k periods. X_{cpt} includes time-varying controls. The main specification uses the inverse hyperbolic sine of South Korean exports as the outcome variable and does not include additional control variables (X_{cpt}).

Estimating equation (7) relies on two central assumptions for $\hat{\theta}_k$ to give unbiased estimates of the causal effect of the office opening after k years. It requires a parallel trends assumption: We assume that counterfactual trends - in absence of an office opening - do not differ in periods $g + k$ with $k > 0$ between those treated in year g and the control.[60] Alleviating concerns about parallel trends violations, there is little indication of differential pre-trends (discussed in section 5.2) and no "effect" on non-South Korean exports to a destination (discussed in section 5.3). We further show that the European offices' rollout can almost fully be explained using pre-determined (1962) import market size, alleviating concerns regarding the parallel trends assumption: there was little room to time office openings based on time-varying counterfactual trends in exports to a country – either strategically or coincidentally.

The second central assumption to estimating θ_k requires no spillovers, i.e. an office affects exports only to the country in which it is located. If this assumption is violated, the estimated office opening effect should be interpreted as re-allocations of export flows rather than absolute effects on exports to a given country. More technically we need to assume that unit treatment values are stable (SUTVA) – one unit's treatment value must not depend on other units' treatment. Concerns regarding this assumption are somewhat alleviated as we find similar effects whether we use a never-treated or not-yet-treated control group, as these two groups as SUTVA violations would likely affect those groups differentially. Further, SUTVA violations would be most concerning if they caused an upwards bias in the estimated effects. Alfaro-Ureña, Castro-Vincenzi, Fanelli, and Morales (2023) provide some justfication to believe SUTVA violations would bias our results downwards not upwards not upwardly bias our estimates. They find that exports to different countries are complements. Following this, violations of SUTVA could lead to underestimates.

A third assumption – no anticipation – is required for identification. This would be violated if office openings have a causal effect, at $k < 0$. Negative anticipation – an Ashenfelter (1978) dip – would lead to overestimates. We find no indication for this.[61]

[60] Persistent level differences between the treatment and control group do not constitute a violation of this assumption. In line with the recent difference-in-differences literature we carefully select the sample such that either the never-treated or the not-yet-treated form the control group (Callaway and Sant'Anna, 2021).

[61] Positive anticipation may occur if firms decide to export to a market today because KOTRA will open an office there next year. Under the parallel trends assumption, positive anticipation is part of KOTRA's causal effect and would give rise to an underestimate. In all specifications with the never-treated control

5.1.1 Addressing Concerns about Staggered Difference-in-Differences

While staggered two-way fixed effects regressions are popular among economists, a recent literature clarifies a number of circumstances under which such a specification fails to identify causal effects (de Chaisemartin and D'Haultfœuille, 2020; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021; Borusyak, Jaravel, and Spiess, 2023; Rambachan and Roth, 2023). For example such a regression may obtain biased estimates under dynamically increasing treatment effects if already-treated units are included in the sample. For this reason, in estimating equation (7), we construct a panel for the ever-treated countries that is balanced in an event-horizon around the first office opening and a second panel for the never-treated countries that is balanced in all years included in any of the above-mentioned event horizons.

Such a balanced panel requires excluding some of the earliest treated countries. As the export data starts in 1962, we can only include x pre-periods for countries that are treated in year $1962 + x$. Hence, including more pre-periods in the regression comes at the cost of excluding more events from the regression. For the baseline specification, we include five pre-periods, i.e. excluding economies with events that took place between 1962 and 1966.[62] Sections 5.2 and 5.3 show that pre-trends remain small and effects comparable when including four (two) pre-periods, additionally including events from 1966 (1964 and 1965).

For countries that experience an office opening between 1967 and 1981, we include all observations that are no more than five years prior to the office opening and no more than eleven years after the office opening. Hence, the earliest start year for a treated country's event horizon is 1962 while the latest end year is 1992. Hence, for countries that do not receive an office, we include in the sample all observations between 1962 and 1992. In estimating equation (7), this sample serves to estimate λ_{pt}. [63].

An obvious disadvantage of estimating the treatment effect relative to the never-treated is that these are the countries which may be least comparable to the treated countries. To address this, appendix 5.3 provides estimates using a not-yet-treated control group, using the estimator proposed by Callaway and Sant'Anna (2021). To make the never-treated economies more comparable, we exclude all never-treated economies with a population below one million.[64]

group, there is no indication for such positive anticipation. Different from the concern about anticipation, increasing exports prior to an office opening could also cast doubt on the validity of the parallel trends assumption. This possibility is explored in appendix G.2.

[62]Countries and territories with first office opening in 1962: U.S., Thailand, Taiwan. 1963: none. 1964: Japan, Singapore, Indonesia, South Vietnam. 1965: Philippines, Peru, Kenya, Iran, Hong Kong, UK. 1966: Italy, Netherlands, Panama, Nigeria.

[63]To ensure a balanced panel, we exclude all never-treated economies that are not well-defined for the entirety of the period between 1962 and 1992. For this reason, we exclude Br.Antr.Terr, CACM NES, Carib. NES, Eur.Other NE, Eur. EFTA NS, EEC NES, E Europe NES, China SC, St.Helena, Occ.Pal.Terr, LAIA NES, Int Org, Germany, Slovenia, Fm Yemen AR, Fm Yemen Ar, Fm Yemen Dm, Fm USSR, Russian Fed, TFYR Macedna, Tajikistan, Yugoslavia, Fm Yugoslav, Ukraine, Slovakia, Rep Moldova, Lithuania, Latvia, Kyrgyzstan, Czech Rep, Belarus, Bosnia Herzg, Kazakhstan, Croatia, Azerbaijan, Estonia, Viet Nam

[64]For this reason, we exclude Falkland Is, Gibraltar, Greenland, Bahamas, Barbados, Belize, Bermuda,

5.2 Results: Effect of Office Opening on Exports

Figure 9 reports the estimated effects of the first overseas export promortion office in a destination country around the office opening. The coefficients in the pre-periods are economically small and not statistically distinct from 0. This assuages concerns that the parallel trends assumption is violated. Figure A.18 again finds pre-trends very close to zero and much smaller than any of the post-coefficients, although when including four pre-periods (but not when including two) some pre-coefficients are marginally statistically significant at the 5%-level.

Figure 9 further shows that the opening of an export promotion office is associated with an increase in Korean exports to that destination. While the estimates in nearly all post-periods allow us to reject the null-hypothesis of no effect, the point estimates themselves are somewhat imprecisely estimated. The estimates increase over time, suggesting that the entire effect of an office opening only materialize over time. The point estimates stabilize a few years after the office opening. The average point estimate in years 9-11 is 0.321, suggesting exports are 38%[65] higher relative to the control group.

To put these effects into perspective, it is natural to compare them to the effect on trade of distance – a central predictor for trade flows between two locations. Assuming an elasticity of trade to distance of -1 (Anderson, 2011; Head and Mayer, 2014), this is equivalent to reducing the distance between London and Seoul (8,900km or 5,500 miles) to the distance between Mumbai and Seoul (5,600km or 3,500 miles).

An alternative way of quantifying the effect size is to consider how much more attractive a KOTRA office makes a country as a destination for South Korean exports. This suggests an office opening makes Ecuador – a country with a fixed effect at the 25th percentile – as attractive as Greece – a country at the 50th percentile. At the same time Greece with an office is as attractive as Spain – a country at the 75th percentile.

5.3 Robustness

This section considers potential violations of the parallel trends assumption underlying our estimates of the effect of opening an office.

Appendix G further contains a detailed discussion of the results robustness to (1) including the countries with office openings between 1964 and 1966, (2) using a not-yet-treated control group following Callaway and Sant'Anna (2021), and (3) considering only the extensive margin of exports – alleviating concerns that the results are an artifact of transforming the raw export values using the inverse hyperbolic sine.

Botswana, China MC SAR, Cyprus, Djibouti, Eq.Guinea, Fiji, Fr Ind O, Fr.Guiana, Gabon, Gambia, GuineaBissau, Guadeloupe, Guyana, Iceland, Kiribati, Malta, Mauritania, Mauritius, Neth.Ant.Aru, New Calednia, Maldives, Oman, Qatar, Samoa, Seychelles, St.Kt-Nev-An, St.Pierre Mq, Suriname.

[65] $lim_{x\to\infty} sinh(x + 0.321)/sinh(x) = 1.379$

5.3.1 No Increase in Import Demand upon Office Opening

This section considers that a country's first export promotion offices may systematically – perhaps strategically – be opened at a time when that country experiences increases in import demand. We address this concern in two ways. First, instead of South Korean exports to a country, we use imports from the rest of the world as the dependent variable in estimating equation (7). The coefficients from this regression are reported in figure 10.[66]. The point estimates are very close to zero before and after an office opening. Overall, these indicate that opening an export promotion office does not coincide with statistically significant effects regarding this placebo outcome.

Second, we re-estimate equation (7) while controlling for non-South Korean exports to a country (also transformed using the inverse hyperbolic sine). Appendix figure A.18 (a) shows that the estimates from this specification are largely unchanged compared to the baseline. Given South Korea's rapid economic growth, it may be that the relationship between South Korean and other exports differs over time.[67] Panel (b) shows that estimates are largely unchanged when allowing the effect of non-South Korean exports to differ by year.

5.3.2 Rollout Follows Pre-Determined Gravity Variables

This section shows that the year in which a country's first office opened was largely pre-determined by time-invariant factors. As long as the effect of these factors on exports is also time-invariant, they are absorbed in the country fixed effects – γ_c. Even if the effect of these time-invariant variables is not stable over time, the pre-determined order of the roll-out makes it unlikely that office openings are timed to coincide with counterfactual increases of exports, whether strategically or coincidentally, rendering violations of the parallel trends assumption less plausible as drivers of the main results. To predict office openings, we use insights from a gravity equation. Apart from the U.S., the first office openings took place in Taiwan, Thailand, Japan, Singapore, Indonesia, and South Vietnam – among the geographically closest non-communist countries and territories.[68]

Within Europe, distance from South Korea does not vary much between countries[69], so the main predictor for office openings from a gravity equation would be the size of each destination's market.[70] We use 1962 non-South Korean exports to a country – a measure of a destination's market size – to predict the year when a country's office opening occurs. As there was no KOTRA office in Europe until 1965, 1962 non-South Korean exports are pre-

[66] As well as appendix figure A.18, panels (e) and (f).

[67] Non-South Korean exports do increase South Korean exports at the country-product-year level. However, these non-South Korean exports do not change systematically upon an office opening.

[68] North Korea, China, the U.S.S.R., North Vietnam were ideological opponents of the South Korean governments.

[69] Athens' distance from Seoul is 96% of the distance between London and Seoul.

[70] A further advantage of restricting attention to European countries with an office opening between 1962 and 1981 is that other gravity variables also vary less between them with respect to South Korea – e.g. language distance.

determined from the perspective of the roll-out of KOTRA offices to Europe. If KOTRA's offices perfectly follow this ranking, this rigidity in the roll-out schedule would alleviate concerns about violations of the parallel trends assumption due to the timing of office openings.

For the 17 European countries where an initial office opened during the main roll-out of overseas offices, figure 11 plots each country's rank regarding its office opening year against its rank in terms of 1962 market size, i.e. non-South Korean exports to the country. E.g., the UK was the biggest market (rank 1) and was the first to receive an office (rank 1). On the other hand, Portugal was the smallest market (rank 17) and was the last to receive an office (rank 17). Across the 17 countries, the rank correlation between 1962 imports and office opening year is 0.87, leaving very little room for timing offices in violation of the parallel trends assumption – either for strategic reasons or coincidentally.

Appendix table B.1 further predicts opening years for the 17 European countries using 1962 non-South Korean exports. It shows that true and predicted opening years often coincide exactly, again highlighting the limited degrees of freedom for strategically or coincidentally violating the parallel trends assumption.

6 Experience & Effect on Exports by Product

Chapter 4 showed the managers of overseas export promotion offices mattered greatly in determining the offices' effects on South Korean exports. This raises the question whether the capacity of these bureaucrats can be built.

This chapter isolates quasi-random variation in a bureaucrat's exposure to different products to estimate the causal effect of product-specific experience on South Korean exports. We find that exports of a product increase by 3.0% if the appointment of a new bureaucrat implies an increase in product-specific experience. Quantifying this effect in terms of distance, it is similar as moving London as close to Seoul as Frankfurt is. While this effect does not come close to the differences between individuals, it is sizable when considering that it is reflects only the effect of the quasi-random component of experience.

This is the first evidence regarding learning-by-doing as a channel for increasing bureaucratic capacity. It complements the existing literature on bureaucracy which has focused on selection and incentives. Learning-by-doing in an organization also points to a novel source of path dependence in organizational capacity. A bureaucracy will be most effective at carrying out familiar tasks. Expanding into policy areas in which the bureaucracy has no recent experience builds capacity but is less likely to bring immediate policy success.

6.1 Identification: Quasi-Random Variation in Bureaucrat Experience

This subchapter discusses our strategy to identify the causal effect of product-specific experience on South Korean exports.

$$\text{experience}_{bp} = \sum_{k=0}^{2} \text{exports}_{C_1(b),p,T_1(b)+k} \tag{8}$$

We conceptualize the measurable component of a bureaucrat's experience as the South Korean exports to which a bureaucrat was exposed during their first appointment, given by equation (8).[71] $T_1(b)$ and $C_1(b)$ indicate the year and country of bureaucrat b's first appointment.[72] As in the remainder of the paper, *exports* always refers to the inverse hyperbolic sine of exports.

experience_{bp} captures how much of product p was exported by South Korea during bureaucrat b's first appointment. This is a natural measure of bureaucrat b's experience because their job consists in facilitating exports by South Korean firms. Hence, the bureaucrats are unlikely to learn much about product p if $\text{experience}_{bp} = 0$ – i.e., South Korean firms do not export product p at all during bureaucrat b's first appointment. On the intensive margin, it also appears natural that bureaucrats learn more about products where South Korean exports are greater.

However, while experience_{bp} is a natural measure of a bureaucrat's experience, it is also quite obviously endogenous. First, in light of chapter 4, experience_{bp} is endogenous to bureaucrat actions during their first appointment. Second, experience_{bp} is endogenous if bureaucrat b's first appointment was strategically chosen based on existing exports to that destination. Third, bureaucrats' later appointments may be endogenous to the experience gained during their first appointment.

We address each source of endogeneity below. It is also worth noting that during qualitative interviews, KOTRA bureaucrats dismissed as absurd the notion that bureaucrats are appointed to a particular country because of their experience regarding a particular product. They appeared to think that the products exported to a country were a very minor concern in the decision to appoint bureaucrats. This could suggest that the second and third endogeneity concern may not be first-order.

$$\text{instrument}_{bp} = \sum_{k=0}^{2} \widehat{\text{exports}}_{C_1(b),p,T_1(b)+k} - \sum_{k=-3}^{-1} \widehat{\text{exports}}_{C_1(b),p,T_1(b)+k} \tag{9}$$

$$\widehat{\text{exports}}_{cpt} = \text{exports}^{\text{non-Korean}}_{cpt} \frac{\text{exports}_{-c,pt}}{\text{exports}^{\text{non-Korean}}_{-c,pt}} \tag{10}$$

To address the sources of endogeneity, we proceed in two steps. First, we construct a measure of quasi-random variation in experience that addresses the sources of endogeneity discussed above. This is given by instrument_{bp} as described by equations (9) and (10). Second, we obtain event-study estimates of the effect of experience. In combination with the relatively rigid three-yearly rotation of bureaucrats, the event-study estimates further

[71] The measure of experience sums over the three years starting with the year of the bureaucrat's appointment.

[72] This measure of experience is only defined from a bureaucrat's second appointment on.

assuage concerns regarding the sources of endogeneity, especially of the third type.

First, to avoid that our measure of experience is endogenous to bureaucrat actions during their first appointment, we replace South Korean exports by predicted South Korean exports calculated according to equation (10). To capture a country's overall import demand, we calculate predicted South Korean exports by using contemporaneous non-South Korean exports to the same product-country. To increase this measure's relevance to KOTRA's goal of promoting South Korean exports, non-South Korean exports are normalized by the ratio of South Korean to non-South Korean exports of the same product to *other* countries in the same year.

Second, a bureaucrat's first appointment may be endogenous to existing exports to that destination – e.g., a bureaucrat may be appointed to the Netherlands to learn about semiconductors because the Netherlands are an important destination market for South Korean semi-conductor exports. We rule out that experience is due to such strategic appointments by subtracting lagged predicted exports from our measure of experience – according to equation (10). Hence, our measure of experience is net of differences in exports (of product p) that existed in the three years prior to a bureaucrat's first appointment.

Third, bureaucrats' later appointments may be endogenous to their experience gained during their first appointment. This is a more classic identification concern. First, it should be noted that this is problematic only if bureaucrat appointments are endogenous to the variation in experience that is present in $\text{instrument}_{b(c,t),p}$ because our measure of the change in experience is based on $\text{instrument}_{b(c,t),p}$ – instead of $\text{experience}_{b(c,t),p}$. Second, the event-study estimation discussed below only attributes to the bureaucrat changes in exports relative to the pre-existing level of exports of a country-product. So, strategic appointments of bureaucrats would result in a biased estimate of the effect of experience if it translated into a violation of the parallel trends assumption needed to for β_k in equation (11) to causally identify the effect of experience. The parallel trends assumption is discussed further below.

$$y_{ept} = \sum_{k \neq -2} \beta_k \, \text{increase}_{ep} \, \mathbf{1}\{t = T + k\} + \eta_{ep} + \lambda_{T(e),pt} + \tau_{et} + \epsilon_{ept} \tag{11}$$

We estimate equation (11), a reduced form event-study which aims to identify the causal effect on exports from a switch between two bureaucrats. As before, t indicates the observation year, p the 4-digit product, and $b(c,t)$ the bureaucrat assigned to country c in year t. $T(e)$ indicates the year of the event e, defined as the first full year in which the new bureaucrat is in charge.

The coefficients of interest are β_k. increase_{ep} is a dummy that indicates whether there is an increase in experience regarding product p due to event e: the switch from bureaucrat $b(c(e), T(e) - 1)$ to bureaucrat $b(c(e), T(e))$.

Equation (11) includes event × year fixed effects – τ_{et}. As each event × year corresponds to a unique bureaucrat, τ_{et} absorbs bureaucrat fixed effects and any effect of experience that affects all products equally. The specification thus isolates the differences in exports of

products that see an increase in experience compared to those products that do not see such an increase.

Further, equation (11) includes event × product fixed effects – η_{ep} – to avoid attributing any effects to demand for a product that is time-invariant during the event horizon. This rules out that our effects are spuriously attributed to bureaucrats experienced in product p being appointed to countries where South Korea already exports product p before their appointment.

Finally, equation (11) includes product × year fixed effects that are allowed to differ by year of event – $\lambda_{T(e),pt}$. The first concern this addresses is given by a mechanical relationship between our measures of experience and exports due to secular changes in South Korea's exports of products over time. If a bureaucrat is first appointed in 1968, they gain more experience regarding the type of products that South Korea was exporting in 1968 (e.g. textiles, not cars). This bureaucrat is more likely to be re-appointed in 1973 – when South Korea still exported more textiles than cars – rather than 1993 – when cars had become much more important than textiles. This type of correlation is avoided by including year-product fixed effects. Year-product fixed effects further avoid spurious correlations due to the fact that South Korean exports in later years are larger for any product or the fact that textiles always make up a larger share of South Korean exports than do maize or crude oil.

For β_k in equation (11) to causally identify the effect of product-specific experience on exports, we again rely on a parallel trends assumption. The parallel trends assumption requires that if $\text{increase}_{ep} = \text{increase}_{ep'} = 0$ – i.e., both p and p' are untreated –, exports of products p and p' in expectation follow parallel trends. This would be violated under the third endogeneity concern discussed above – bureaucrats may be appointed to a country × year because of their experience and an anticipated increase in exports to that country in line with their exports. Partly this concern is alleviated by our use of instrument_{bp}. If KOTRA's decision to re-appoint bureaucrats was endogenous to bureaucrat experience but not instrument_{bp}, this would not violate our identifying assumptions. The fixed effects included in equation (11) further weakens the required parallel trends assumption.

The event-study specification (11) allows us to investigate pre-trends which are informative about the plausibility of the parallel trends assumption. As discussed before, due to the three-yearly rotation of KOTRA bureaucrats, if KOTRA tried to strategically appoint bureaucrats with experience in product p to country-years with high counterfactual exports of product p, this would induce differential pre-trends. Hence, observing parallel pre-trends would substantially alleviate such concerns.

A no-spillovers, SUTVA, assumption is also necessary to interpret β_k as the causal effect of experience on exports of a particular product. In this case, the SUTVA has two components: (1) Bureaucrats only affect exports to their country of appointment. (2) increase_{ep} only affects exports of product p (not p'). If either of these assumptions is violated, β_k should be interpreted only as the effect relative to the comparison category rather than a causal effect on exports y_{ept}.

6.2 Results: Experience Increases Exports

Figure 12 plots the event-study estimates (β_k) obtained from estimating equation (11), where increase_{ep} is defined using the experience measured in $\text{instrument}_{b(c,T(e)),p}$ and $\text{instrument}_{b(c,T(e)-1),p}$. The results from the main specification are labeled "Increase vs Decrease".

The main specification finds no pre-trends that are statistically distinct from 0 with very small point estimates. After the event, exports increase sharply in those products where the change in experience due to the switch in bureaucrats exceeds 0, i.e., the new bureaucrat is more experienced than the old bureaucrat. When combining all the post estimates – by replacing the time-dummies with a post indicator[73] – the point estimate 0.0300 (0.0147) is statistically significant at the five percent level. The point estimates translates into an increase in exports by 3% in products in which a bureaucrat is experienced relative to those products in which the bureaucrat is not experienced. As our estimates are within event-year, and thus within bureaucrat, the results are most informative about shifts in the composition of exports due to the switch between bureaucrats.

This chapter set out to answer whether learning-by-doing increases bureaucratic capacity and thus changing the effect of an industrial policy. As described above, we find an effect of bureaucrat learning-by-doing on exports. This effect is an order of magnitude smaller than the policy's average effect as well as the standard deviation in bureaucrat ability. To achieve an effect of similar size to that of experience, it would suffice to counterfactually move London as close to Seoul as Frankfurt currently is. Nevertheless, it should be noted that the 3% increase reported above does not correspond to the entire effect of a bureaucrat's experience. Instead, it reflects only the effect of a small proportion of a bureaucrat's experience – the component of experience that (1) is product-specific and results in zero export gains for other products and (2) is contained in instrument_{bp}. It is thus plausible that bureaucrats' overall experience plays a substantial role in determining which products benefit from this industrial policy.

6.3 Robustness

This subchapter investigates the results' robustness to changes in the measure of experience in equation (11). Concretely, it replaces increase_{ep} by alternative measures reflecting a positive change in bureaucrat experience.

Figure 12 reports results when excluding small changes in experience. For example, the black hollow squares indicate the effect when excluding from the sample those event $\times$ product combinations that are in the middle tercile of changes in instrument_{bp}. The comparison thus becomes one between the top tercile and the bottom tercile. The point estimates from this specification are very similar to the benchmark specification.

The triangles indicate the effect when excluding from the sample those event $\times$ product combinations that are in the second and third quartiles of changes in instrument_{bp} – hence,

[73] This specification omits event-year -1 as it is partially treated.

the comparison becomes one between the top and bottom quartile. Again, the point estimates are similar to the benchmark specification. If anything, the point estimates from these more extreme comparisons (top vs bottom tercile/quartile) give slightly larger point estimates. This seems sensible as moving from comparisons of top vs bottom half, to terciles and quartiles corresponds to increasingly large changes in experience. In line with this, we find attenuated effects when comparing a third to second quartile change.

Appendix figure A.23 reports coefficients from a similar regression that distinguishes between changes in experience in the 1st, 2nd, 3rd, and 4th quartile. The regression's omitted category are products with a 1st quartile change in experience due to the switch in bureaucrat – these are products experiencing a decrease in experience due to the event. We find that a 2nd quartile change only barely increases exports relative to 1st quartile changes. A 3rd quartile change increases exports by 2.5%, while a 4th quartile change causes an increase of 5%. The lack of differential pre-trends allays concerns about the parallel trends assumption underlying these estimates. As previously, KOTRA's rigid schedule implies strategically appointing bureaucrats would result in differential pre-trends. The ordering of the effect sizes reported in appendix figure A.23 further raises our confidence that the measure of experience affects exports in the discussed manner.

Overall, this subchapter shows that the positive effect of bureaucrat experience on exports is robust to a number of natural definitions of the change in bureaucrat experience.

6.4 Mechanism: Experience Increases Exports When Import Demand Increases

This subchapter investigates whether bureaucrats with greater experience increase the responsiveness of exports to market conditions. Similar to the effects of a higher ability bureaucrat, we show that upon the switch to a bureaucrat who is experienced in product p, South Korean exports increase more strongly if this product sees increasing import demand in a given country-year. They also increase more strongly for products that see increasing export supply to other countries from South Korea. Allowing for this triple interaction makes the estimated main effect of experience much more noisy – suggesting that most of the effect of bureaucrat experience comes from more effectively exploiting market conditions, e.g., by relaying information about destination market demand.

$$\begin{aligned}\text{exports}_{cpt,b(c,t)} = \eta_{ep} + \lambda_{T(e),pt} + \tau_{et} + \psi^0_d \text{demand}_{cpt} + \psi^0_s \text{supply}_{cpt} + \\ \psi^0_{d,\text{increase}} \text{demand}_{cpt} \times \text{increase}_{ep} + \psi^0_{s,\text{increase}} \text{supply}_{cpt} \times \text{increase}_{ep} + \\ \sum_{k \neq -2} \Big[\boldsymbol{\beta_k} \text{ increase}_{ep} + \psi_{dk} \text{demand}_{cpt} + \boldsymbol{\beta^{demand}_k} \text{ demand}_{cpt} \times \text{increase}_{ep} + \\ \psi_{sk} \text{supply}_{cpt} + \boldsymbol{\beta^{supply}_k} \text{ supply}_{cpt} \times \text{increase}_{ep} \Big] \mathbf{1}\{t = T + k\} + \epsilon_{ecpt}\end{aligned} \tag{12}$$

We estimate equation (12), which explains changes in exports around a new appointment.

This estimating equation includes all the components from equation (11). In addition, it includes main effects of "demand" and "supply" with as well as interactions with the new and old bureaucrat's ability. "Demand" is the short-hand for other countries' exports of the same product to the same destination. "Supply" is the short-hand for South Korean exports of the same product to other destinations. ψ_d^0 and ψ_s^0 estimate the elasticity to market conditions in the pre-period. $\psi_{d,new}^0$, $\psi_{s,new}^0$, $\psi_{d,old}^0$, $\psi_{s,old}^0$ allow for differences in the pre-period based on the change in experience due to the switch between the two bureaucrats. The new parameters of interest are β_k^{demand} and β_k^{supply} which give the change in exports due to the interaction of market conditions and the change in experience regarding product p between the new and old bureaucrat. This is the difference in elasticity to market conditions relative to the last full year the old bureaucrat was in the country.

Figure 13 plots the estimates of β_k^{demand} and β_k^{supply} for each event year. We find a sharp change in the elasticity of South Korean exports to market conditions in line with the change in experience. This evidence points to the same mechanism discussed earlier for the increases in exports caused by bureaucrats with high fixed effects. Bureaucrats with experience regarding a product may increase exports because they are more effective at transmitting information regarding demand shocks about a product to South Korean exporters or helping them effectively react to such shocks.

As with bureaucrat fixed effects, we would like to say how much of experience's overall effect is mediated by this increased reactivity to market conditions. However, as highlighted by appendix figure A.22, our estimate of the main effect becomes very noisy in this specification.

In addition to exploring this mechanism, the results reported in figure 13 provide further support that the change in exports due to the switch between bureaucrats was not anticipated. Given that KOTRA cannot perfectly time the appointment of bureaucrats, this lack of pre-trends further allays concerns that the results are partly due to strategic appointments of bureaucrats.

7 Conclusion

This paper identifies a setting that allows us to show that bureaucratic capacity changes the effect of an economic policy on exports. The setting is of substantial intrinsic interest as it allows us to quantify narratives linking state capacity and the East Asian miracle – an episode with utmost potential to inform development economics as it includes the only transitions from low to high income since the Second World War.

First, by showing that bureaucratic capacity matters for expands, we expand the study of state capacity to an economic outcome of substantial interest that is not directly controlled by the state. This study further broadens the types of bureaucrats represented in the economics literature by studying bureaucrats with substantial autonomy whose responsibility lies at a level between frontline service provision and the high-level drafting of policies.

Second, the findings imply that implementation matters substantially in determining the effect of an industrial policy. This may translate to other policies that require a strong discretionary component, tacit knowledge, or frequent exchange of information with firms. This adds nuance to the resurgent debate on industrial policy.

Third, we find support that the organization screens bureaucrats based on their performance during the first appointment as country managers, suggesting a path towards managing uncertainty over ability or effort. This possibility provides a rationale for policymakers to choose policies where it is possible to establish such links between individuals and the most relevant policy outcomes.

Fourth, we find that bureaucrats increase exports of a product if they were exogenously exposed to export opportunities for this product in a previous appointment. This suggests a potential path for building state capacity endogenously as bureaucrats become more effective at a task as they gain experience in it. This finding relates to ideas by Hirschmann (1958), who first suggested that exposure to opportunities and problems forms an important channel for capacity-building. However, this paper's results also points to potential path dependence in state capacity as a result of such endogenous capacity growth: A bureaucracy will be most effective at carrying out familiar tasks. So the bureaucracy's past work impacts future effectiveness. This especially matters for industrial policy. Consider a bureaucrat who promoted South Korean exports in the 1960s – when textiles where the dominant product exported by South Korea. The experience results suggest that later appointments of this bureaucrat lead to (relative) increases in the exports of textiles. However, in the 1970s, South Korean firms started exporting products such as steel and non-ferrous metals. In the 1980s, South Korean firms started exporting products such as cars and electronics. During these later decades, the above-mentioned bureaucrat's effect on textile exports forms a channel that makes the policy backward-looking instead of inducing new types of economic activity.

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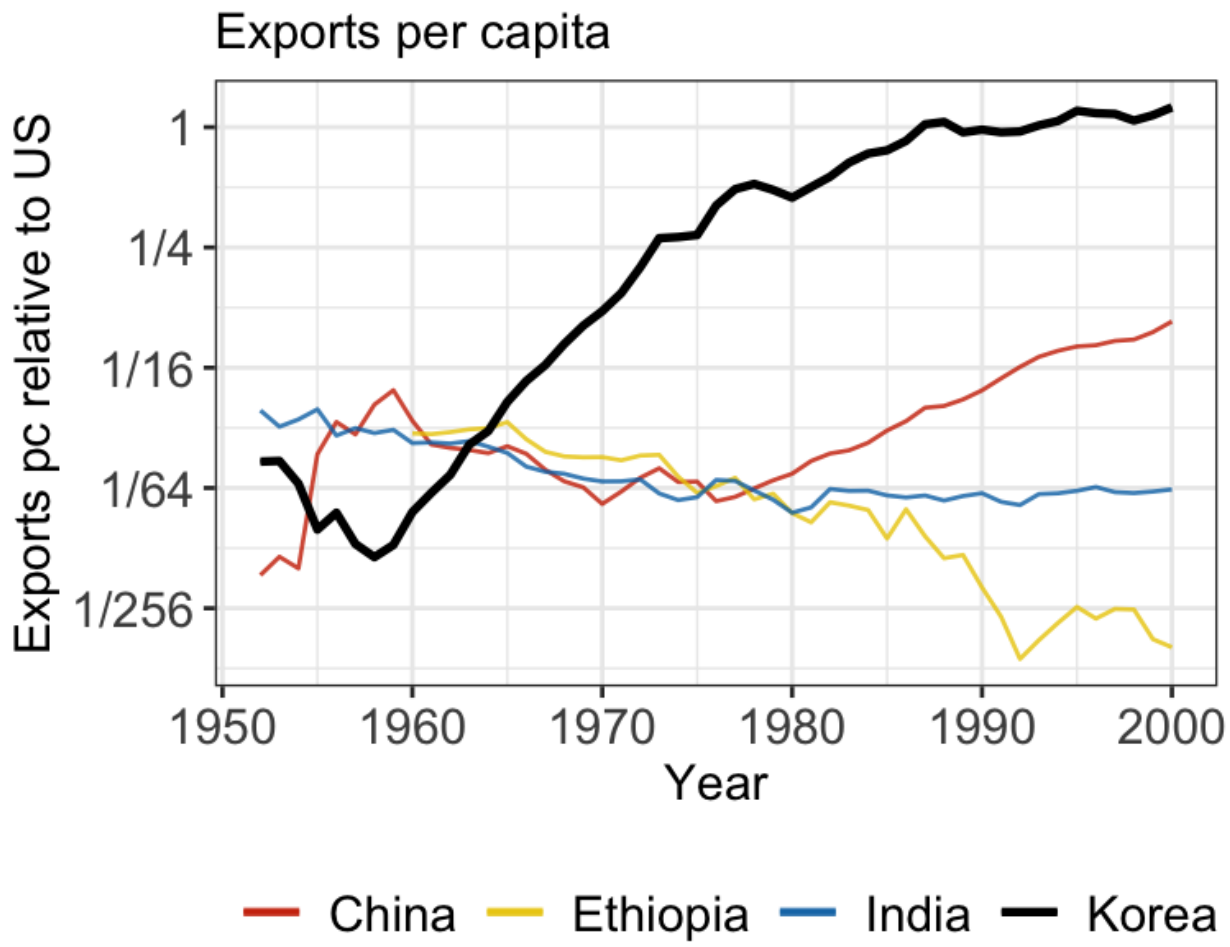
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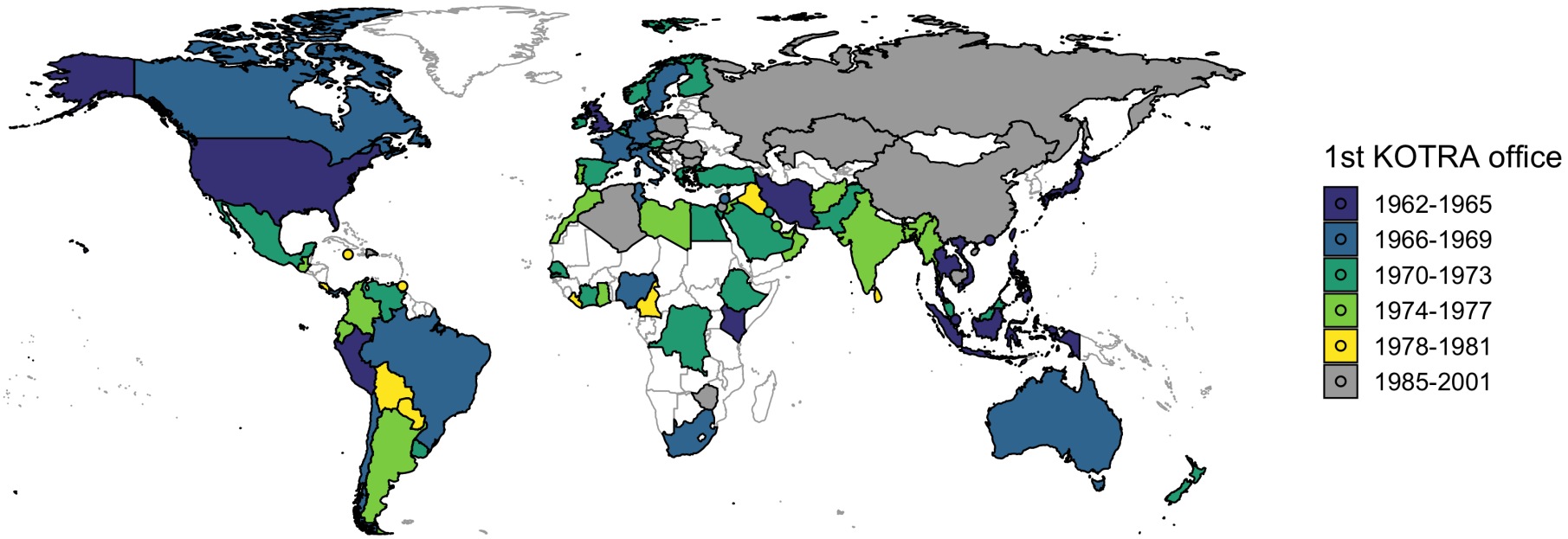
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Figure 1: Growth in Korean Exports



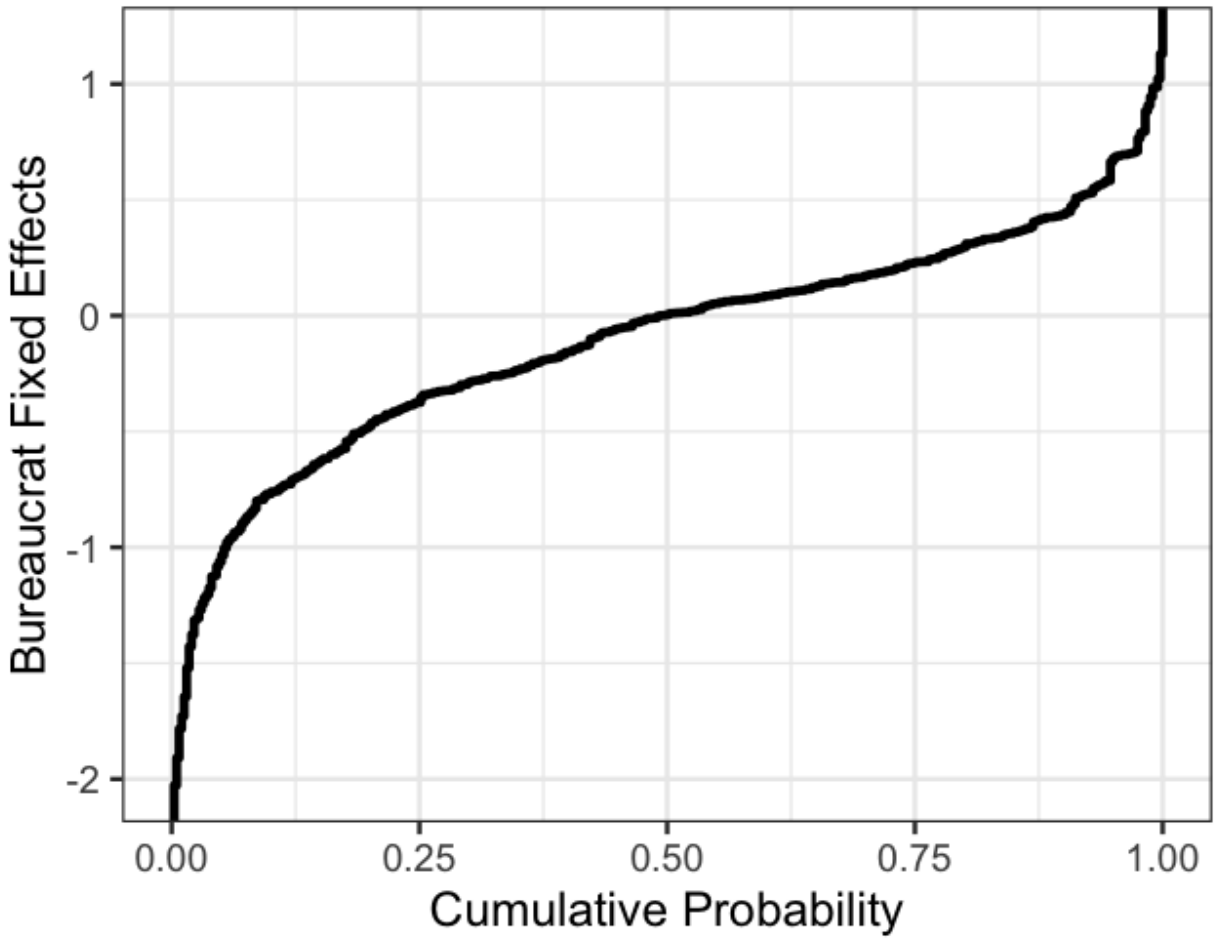
Notes: The figure displays exports per capita relative to the U.S. the years 1952 to 2000 for Korea and a selected group of other countries. Data on exports and population obtained from International Monetary Fund (2023): Direction of Trade Statistics.

Figure 2: Countries with Korean Export Promotion by KOTRA Office



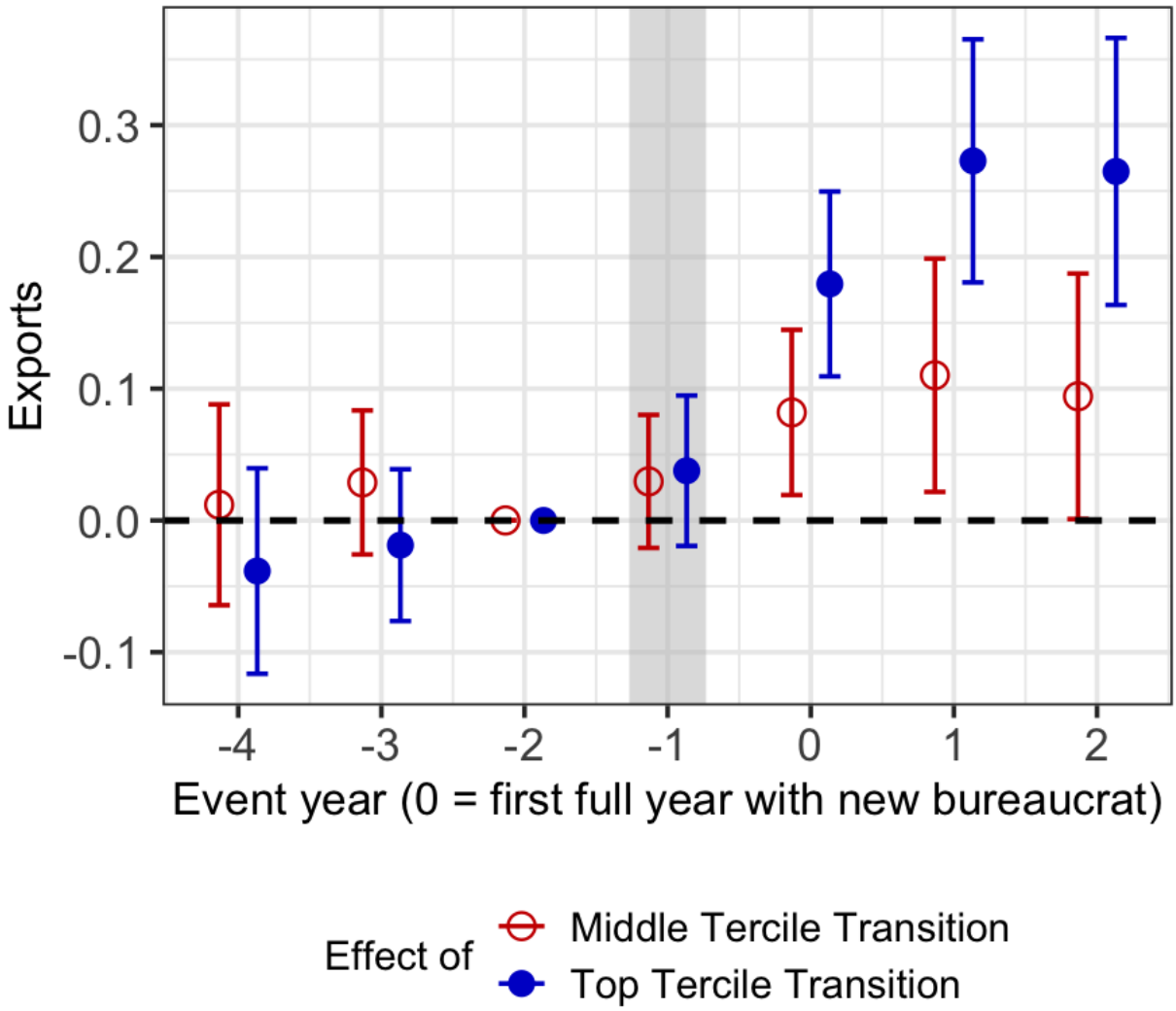
Notes: Colors indicate the opening year of the 1st Korean export promotion office. Most offices remain open from the opening year for the entire period of study. White indicates countries which have not received an office by 2001.

Figure 3: CDF of Raw Fixed Effects of Managers of Country Offices



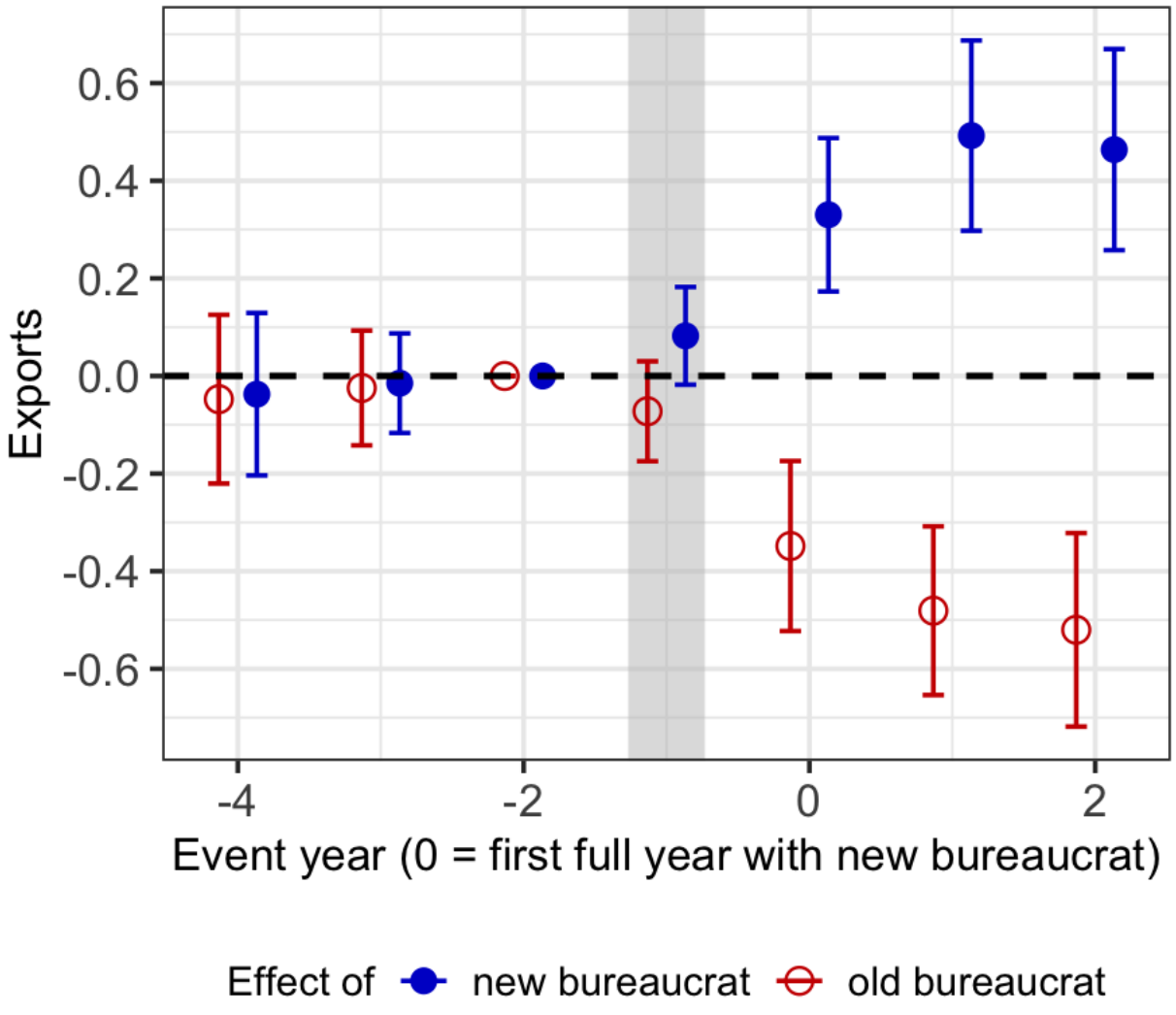
Notes: The figure shows the cumulative density function of bureaucrat fixed effects estimated based on equation (2).

Figure 4: Switches to Better Bureaucrats Not Preceded by Differential Trends.



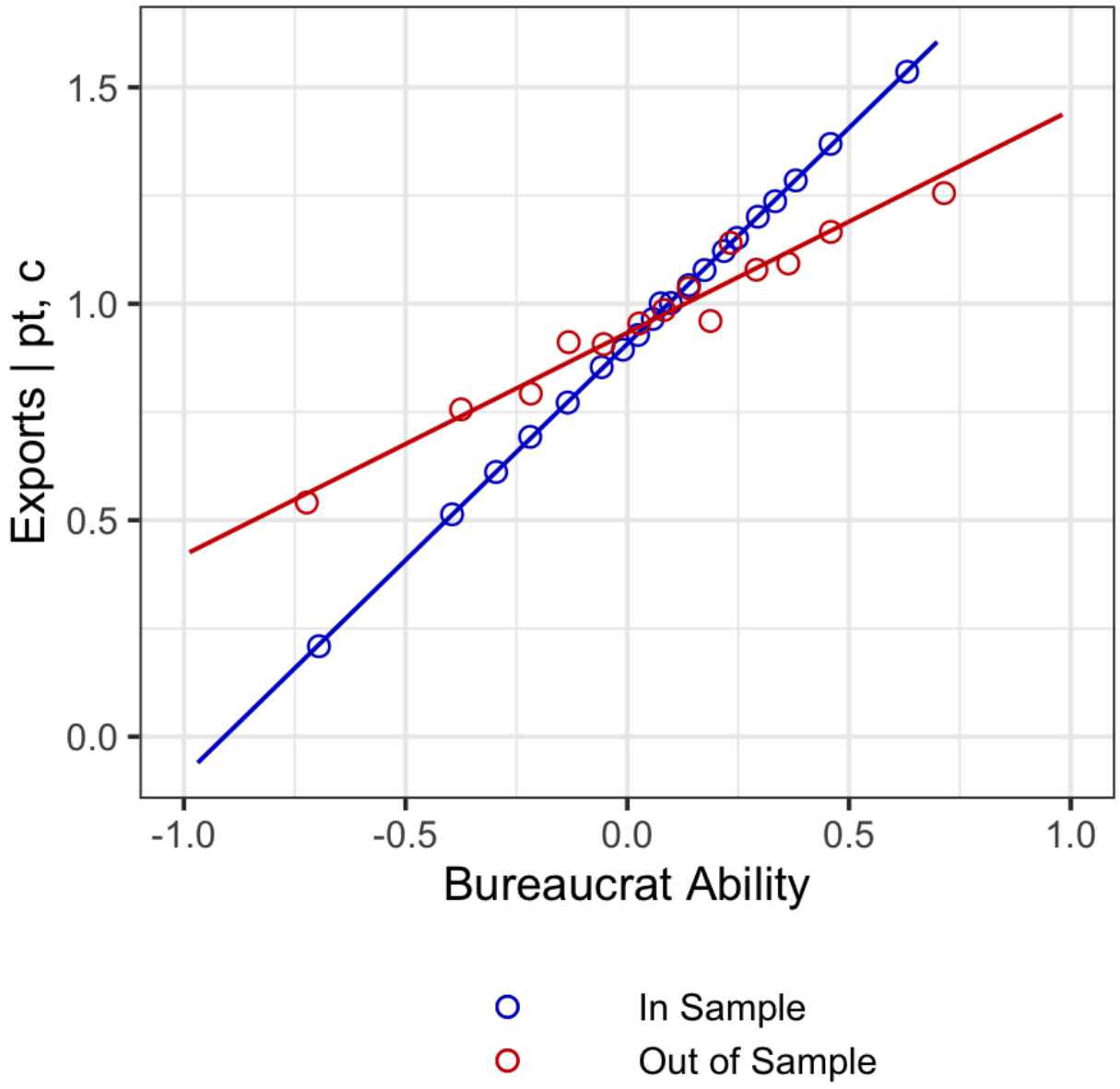
Notes: The figure shows the estimated effect of the change in bureaucrat fixed effects on exports around the time that the bureaucrat managing a country office changes. The estimates are $\hat{\beta}_k$ and $\hat{\delta}_k$ obtained from estimating equation (4). Bootstrapped standard errors are reported. The dependent variable is the inverse hyperbolic sine of exports to the country of the switch between bureaucrats. The switch occurs in year -1. Transitions are categorized into terciles depending on the change in fixed effects implied by the switch in bureaucrats in year -1. The omitted category is a transition in the bottom tercile. The omitted year is -2, the last full year with the old bureaucrat.

Figure 5: Symmetric Effects from Gaining and Losing a Bureaucrat.
No Differential Pre-Trends.



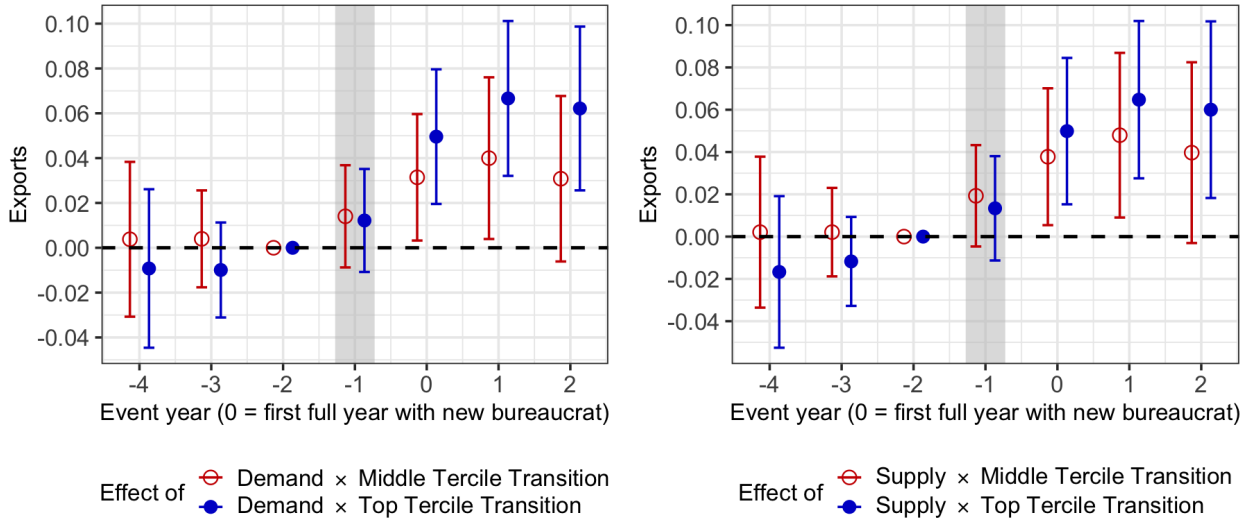
Notes: The figure shows the estimated effect of the change in bureaucrat fixed effects on exports around the time that the bureaucrat managing a country office changes. These estimates are $\hat{\beta}_k$ and $\hat{\delta}_k$ obtained from estimating equation (5). The horizontal axis indexes years in which bureaucrats work in a particular country. Year 0 is the first full year that the new bureaucrat manages the country office. Year -2 is the last full year that the old bureaucrat managed the office. The y axis measures the effect of bureaucrat effectiveness on exports. Bureaucrats effectiveness are fixed effects obtained after residualizing exports by product-country and product-year fixed effects.

Figure 6: Bureaucrat Fixed Effects Predict Exports Out of Sample



Notes: The figure displays a binned scatterplot. The y-axis shows exports residualized for product-year fixed effects (pt) and country (c) fixed effects. These fixed effects, as well as the in-sample (i.e., baseline) bureaucrat fixed effects are estimated using equation (2) and all country-years. Hence, by construction, each in-sample dot lies on the 45-degree line, such that in-sample fixed effects translate one-to-one into higher exports. Out-of-sample fixed effects are estimated only using other countries. For instance, to predict exports to the UK, we obtain the bureaucrat fixed effects from a dataset using all country-years but the UK. The slope coefficient of the regression of residualized exports on these out-of-sample, i.e. *other country*, fixed effects is 0.52.

Figure 7: Good Bureaucrats Increase Exports for Products with Demand (Supply) Growth

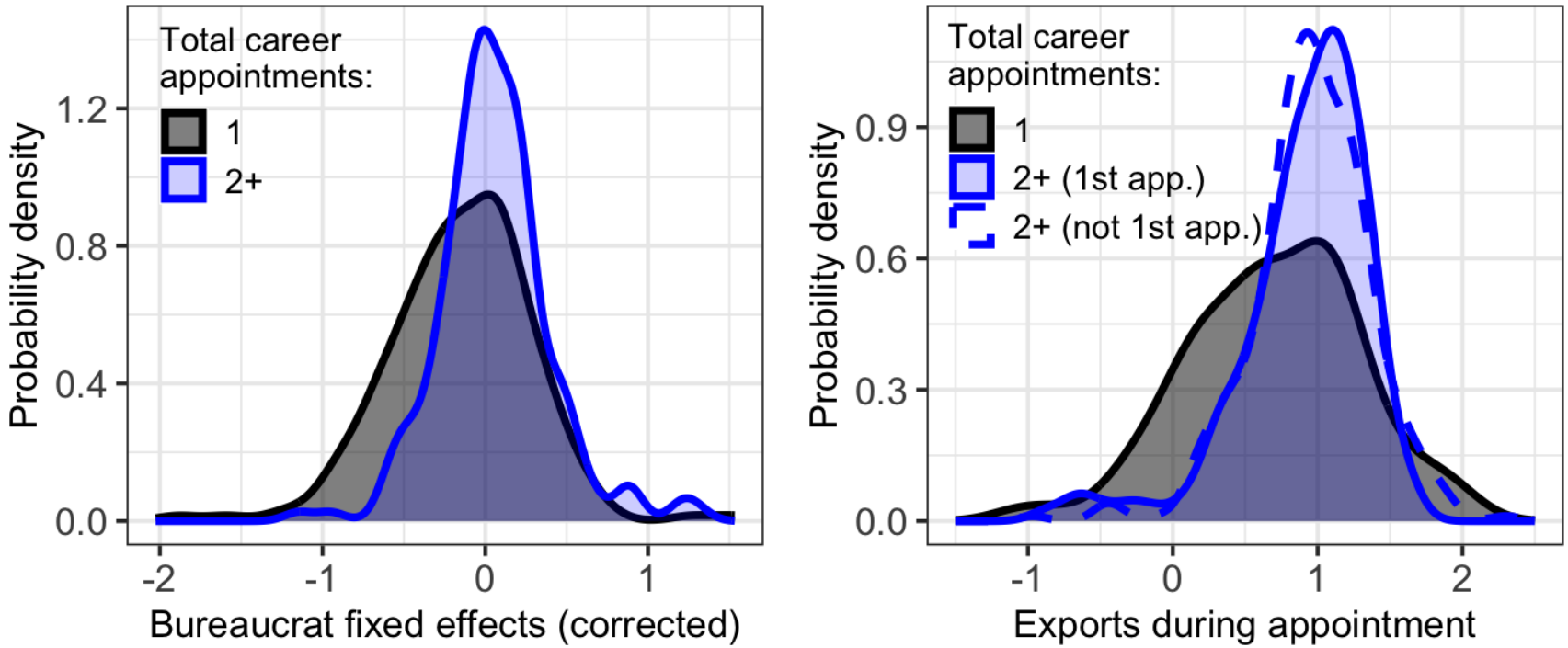


Notes: The figure shows the estimated effect of the change in bureaucrat fixed effects when interacted with two kinds of shocks. The plotted coefficients are estimates of β_k^D, and δ_k^D as well as β_k^S, and δ_k^S obtained from regressions of equation (6). The solid, blue circles give the interaction with exports of the same product to the same destination by other countries (β_k^D, δ_k^D), our proxy for this destination's product-specific demand. The hollow, red circles give the interaction with South Korean exports of the same product to the other destinations (β_k^S, δ_k^S), our proxy for South Korea's product-specific supply. The horizontal axis indicates the years relative to a bureaucrat's appointment. Year 0 is the first full year that the new bureaucrat manages the country office. Year -2 is the last full year that the old bureaucrat managed the office. Bureaucrats effectiveness are fixed effects obtained after residualizing exports by product-country and product-year fixed effects.

Figure 8: Bureaucrat effect. Multi-appointment vs single-appointment managers.

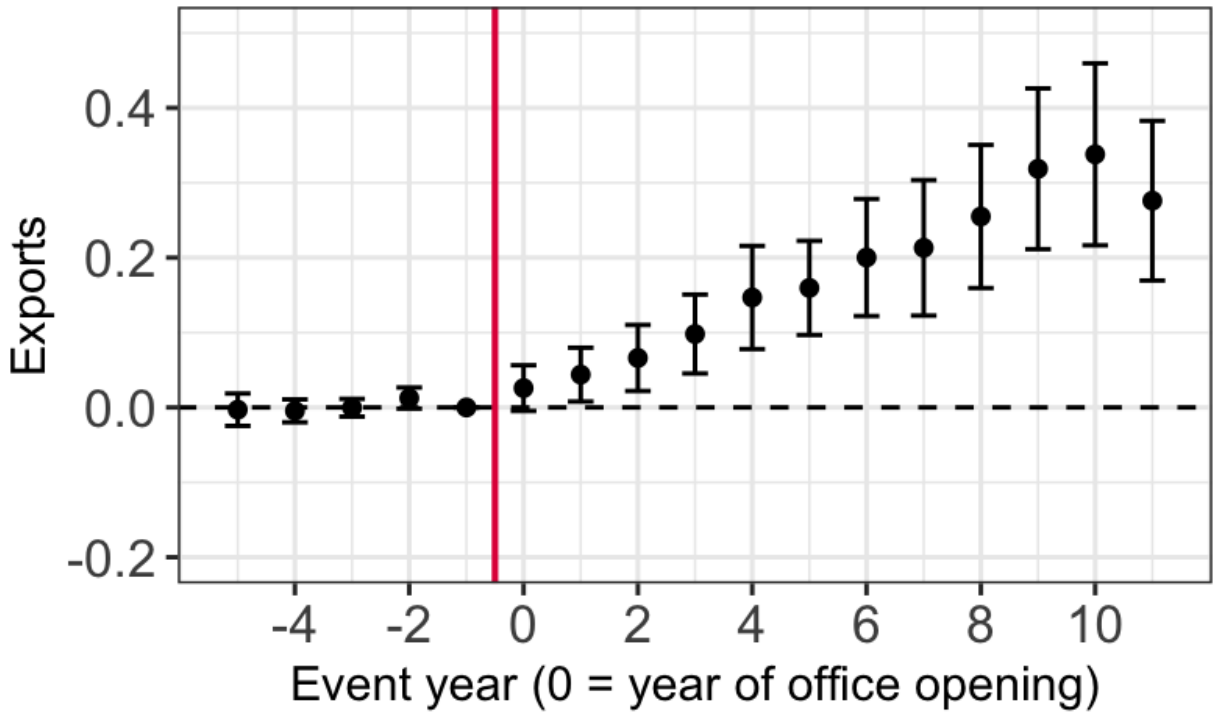
(a) Bureaucrat fixed effects

(b) Residualized exports by appointment



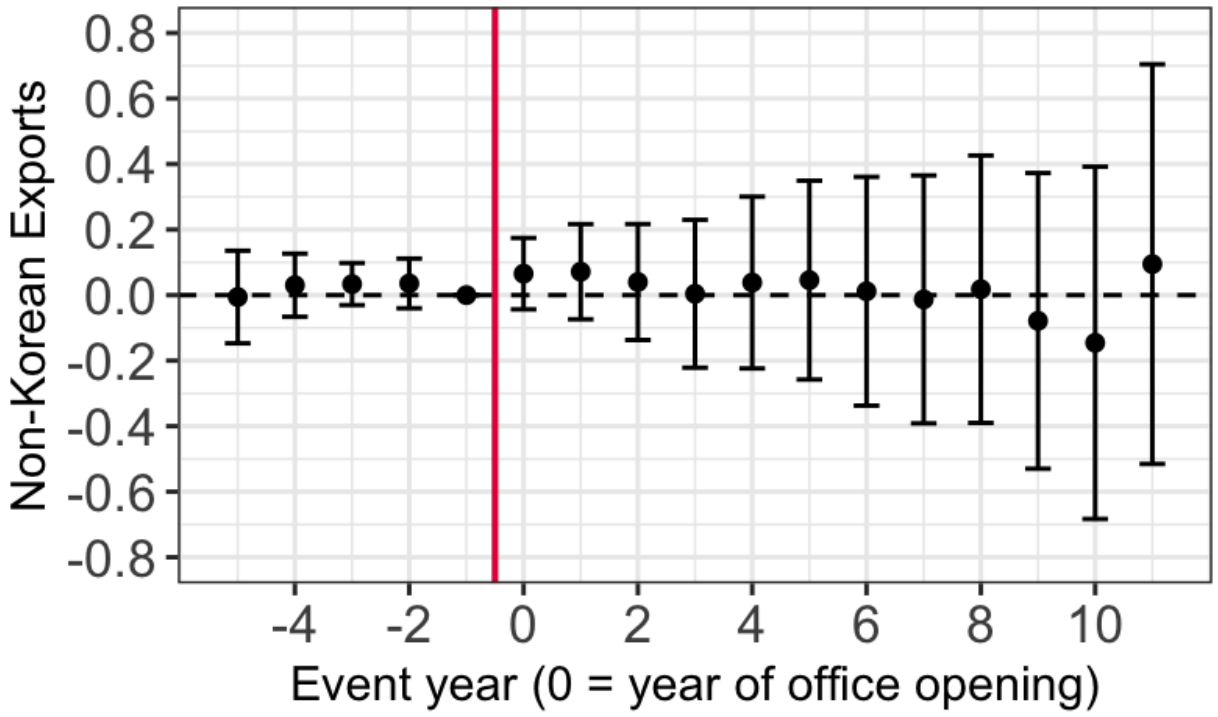
Notes: The figure shows the probability density function of (a) bureaucrat fixed effects (covariance-shrunk) and (b) residualized exports during a bureaucrat's appointment as country manager. It does so separately for bureaucrats who have one career appointment (black) and for bureaucrats who have 2+ appointments over the course of their career (blue). The distributions under the latter group have a much fatter left tail. Panel (b) also shows that the distribution of residualized exports does not change much between the 1st appointment and later appointments for multi-appointment bureaucrats.

Figure 9: Event-study estimates of the effect of office opening on Korean exports.



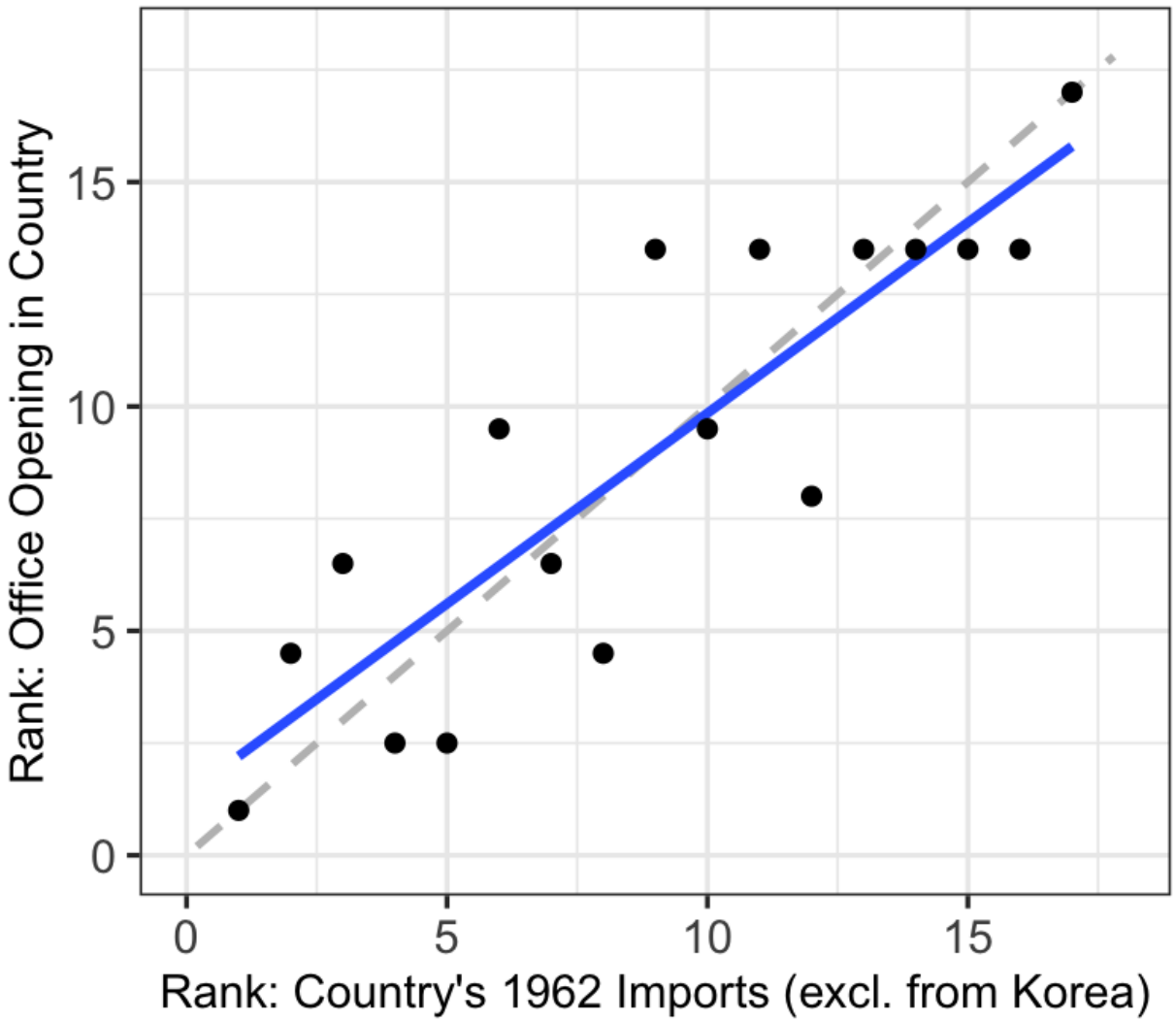
Notes: The outcome variable is the inverse hyperbolic sine of Korean exports to the country-year in question. An observation is at the product-country-year. Point estimates and standard errors are obtained from estimating equation (7). This relies on a never-treated control group. Standard errors clustered at the country-level are reported around each point estimate.

Figure 10: Event-study estimates of the (placebo) effect of office opening on import demand.



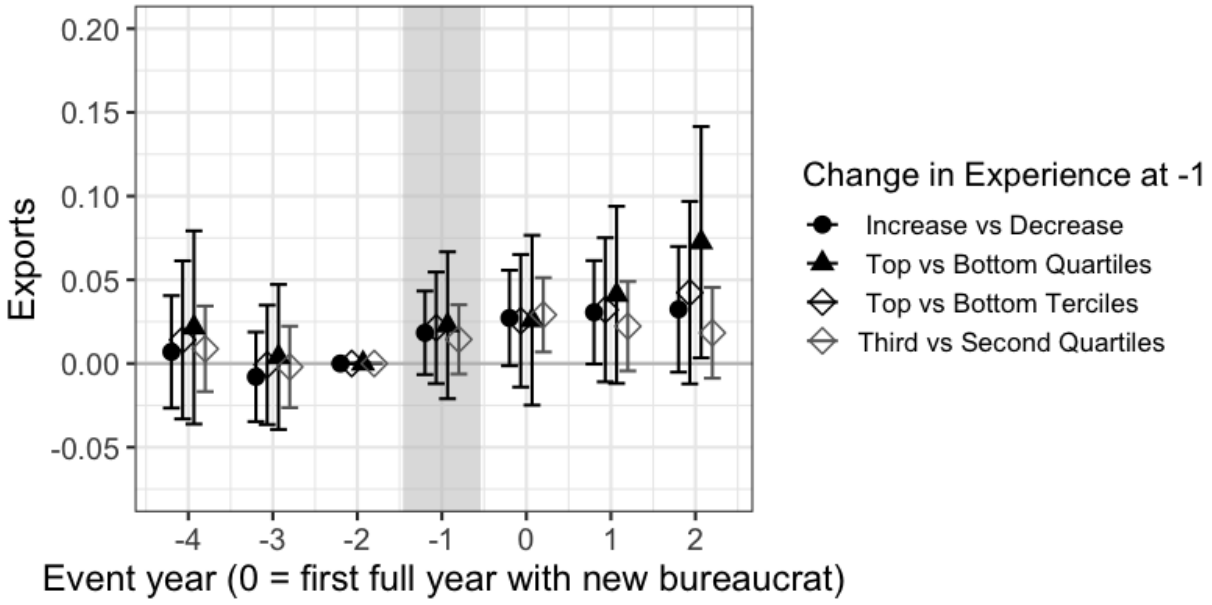
Notes: The outcome variable is the inverse hyperbolic sine of Korean exports to the country-year in question. An observation is at the product-country-year. Point estimates and standard errors are obtained from estimating equation (7). This relies on a never-treated control group. Standard errors clustered at the country-level are reported around each point estimate.

Figure 11: Europe: Openings follow pre-determined market size



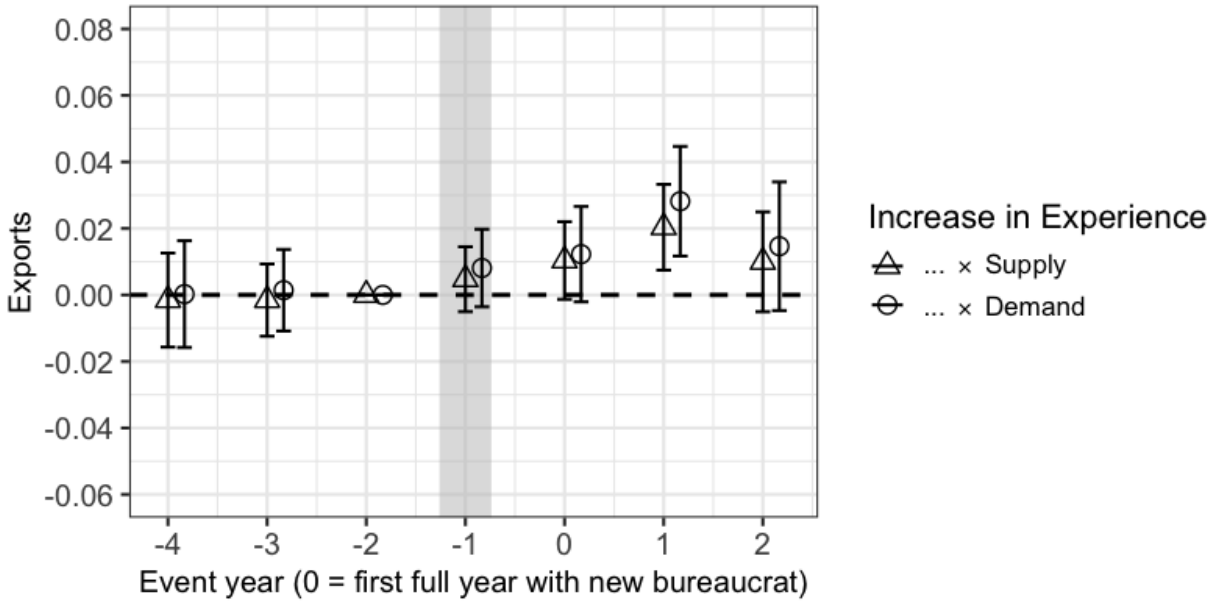
Notes: Each dot corresponds to a European country that received a KOTRA office during the main roll-out of offices (1962-1981). The x-axis gives each country's rank in terms of 1962 imports, excluding imports from Korea. The y-axis gives each country's rank in terms of the order of their office openings. The solid blue line gives the linear fit using 1962 market size to predict the order of office openings. The rank correlation between 1962 imports and office opening is 0.87. The dashed gray line gives the 45-degree line, where the two ranks are exactly equal. This is the case for the UK (rank 1) and Portugal (rank 17). When multiple countries have the same opening year, we assign the average rank to them. For example, Italy and the Netherlands get the second and third offices. As these openings occur in the same year, both have rank 2.5.

Figure 12: Event study – Effect of increase in quasi-random experience$_p$



Notes: The figure shows the estimated effect of the change in the quasi-random component of bureaucrat experience on exports around the time that the bureaucrat managing a country office changes. These estimates are $\hat{\beta}_k$ obtained from estimating equation (11). The solid dots indicate the effect of an increase in experience compared to a decrease. This specification reports results when omitting cases where the change in experience is 0. The other symbols indicate slight variation on the definition of the change in experience. These results are within event-year, so they compare those products where the change in bureaucrat implies an increase in experience vs those where it implies a decrease. The horizontal axis indexes years in which bureaucrats work in a particular country. Year 0 is the first full year that the new bureaucrat manages the country office. Year -2 is the last full year that the old bureaucrat managed the office. The y axis measures the effect of bureaucrat experience on exports.

Figure 13: Quasi-random experience$_p$ increases reaction to demand



Notes: The figure shows the estimated effect of the change in the quasi-random component of bureaucrat experience when interacted with two kinds of shocks. The plotted coefficients are estimates of β_k^{demand}, and β_k^{supply} (12). The hollow circles give the interaction with exports of the same product to the same destination by other countries (β_k^{demand}), our proxy for this destination's product-specific demand. The triangles give the interaction with South Korean exports of the same product to the other destinations (β_k^{supply}), our proxy for South Korea's product-specific supply. The horizontal axis indicates the years relative to a bureaucrat's appointment. Year 0 is the first full year that the new bureaucrat manages the country office. Year -2 is the last full year that the old bureaucrat managed the office.

Table 1: Appointments Descriptives.

	Full Sample	Connected Set	Leave-One-Out Connected Set
	(1)	(2)	(3)
# Managers	399	398	380
# Countries/Offices	87	86	75
# Events/Appointments	736	735	676
# Managers $\geq$ 2 Offices	184	184	180
# Offices $\geq$ 2 Managers	83	83	75
# Offices $\geq$ 4 Managers	75	75	72
# Offices $\geq$ 6 Managers	62	62	61
# Offices $\geq$ 8 Managers	49	49	49

Notes: The table reports summary statistics for KOTRA's overseas offices and their office managers. Column 1 reports these for the full sample of KOTRA's country offices. Column 2 restricts this to those countries and managers which form the largest connected set, while column 3 includes only the countries and managers in the largest leave-one-out connected set – i.e. the set of countries and bureaucrats that would remain connected by ommitting connections due to individual appointments. "# Managers" indicates the number of distinct bureaucrats that held a position as office manager. "# Countries/Offices" indicates the number of distinct offices. This is the same as the number of distinct countries. "# Managers $>$ 1 Office" indicates the number of distinct bureaucrats that held a position as manager of at least two offices. "# Offices $>$ x Manager(s") indicates the number of offices with more than x managers over the course of the sample period.

Table 2: Variance decomposition of exports

		Baseline		Robustness check: Bootstrap bias correction		Placebo check: Bureaucrats randomly shuffled to countries		Relevance check: Ambassadors	
		Component (1)	% Share (2)	Component (3)	% Share (4)	Component (5)	% Share (6)	Component (7)	% Share (8)
Var(exports|*pt*), spell-level		0.732	100	0.732	100	0.737	100	0.781	100
		(0.071)		(0.067)		(0.077)		(0.095)	
Var(bureaucrat)		0.101	13.8	0.100	13.7	0.007	0.9	0.033	4.2
		(0.051)		(0.029)		(0.026)		(0.056)	
Var(country)		0.721	98.6	0.682	93.2	0.590	80.1	0.615	78.8
		(0.102)		(0.097)		(0.073)		(0.139)	
Cov(bureaucrat, country)		-0.088	-12.0	-0.044	-6.0	-0.005	-0.6	-0.015	-1.9
		(0.047)		(0.039)		(0.014)		(0.051)	
Var(bureaucrat+country)		0.646	88.3	0.693	94.7	0.588	79.8	0.617	79.0
		(0.078)		(0.066)		(0.075)		(0.104)	
Var(exports|*pt*), raw		4.404		4.404		4.362		4.552	
Number of observations		1703465		1703465		1753145.1		1616256	
Number of bureaucrats		380		380		387.9		387	
by no. of spells in sample:	1	200		200		207.8		195	
	2	96		96		98.6		105	
	3	56		56		54.4		69	
	4	24		24		21.4		17	
	5	4		4		5.7		1	
Number of countries		75		75		78.3		74	

Notes: The table shows the results of variance decomposition according to equation (3). Columns 1-2 contain our baseline results. We correct for limited mobility bias following Kline, Saggio, and Sølvsten (2020), implemented via the algorithm of Bonhomme, Holzheu, Lamadon, Manresa, Mogstad, and Setzler (2023). Columns 3-4 use the bootstrap bias correction method, described in the main text, instead. Columns 5-6 use synthetic data where bureaucrats are randomly shuffled to countries, preserving the number of appointment spells in the original sample for each bureaucrat. Columns 7-8 replace KOTRA office directors with Korean ambassadors to the same country. The Kline et al. (2020) bias correction method is applied to columns 5-8 as well. Since the algorithm is based on numerical approximations of the traces of large matrix inverses, there is a small degree of randomness in the decomposition results. There is also additional randomness in columns 5-6 arising from the random shuffling of bureaucrats. Thus, the variances and covariances we report in all columns are averages of 100 iterations of the algorithm (holding the original sample fixed). The number of observations, bureaucrats, and countries are also averages of 100 iterations. In columns 5-6, they are decimal numbers because each random shuffling produces a different leave-one-out connected set; their similarity to the ones in columns 1-2 imply that the connectivity of the graph is largely maintained in the random shuffling. The parentheses contain bootstrap standard errors of the variance components, generated by resampling the data at the appointment level 100 times. Again, for each bootstrap sample, we take the average of 100 iterations.

Table 3: Counterfactuals

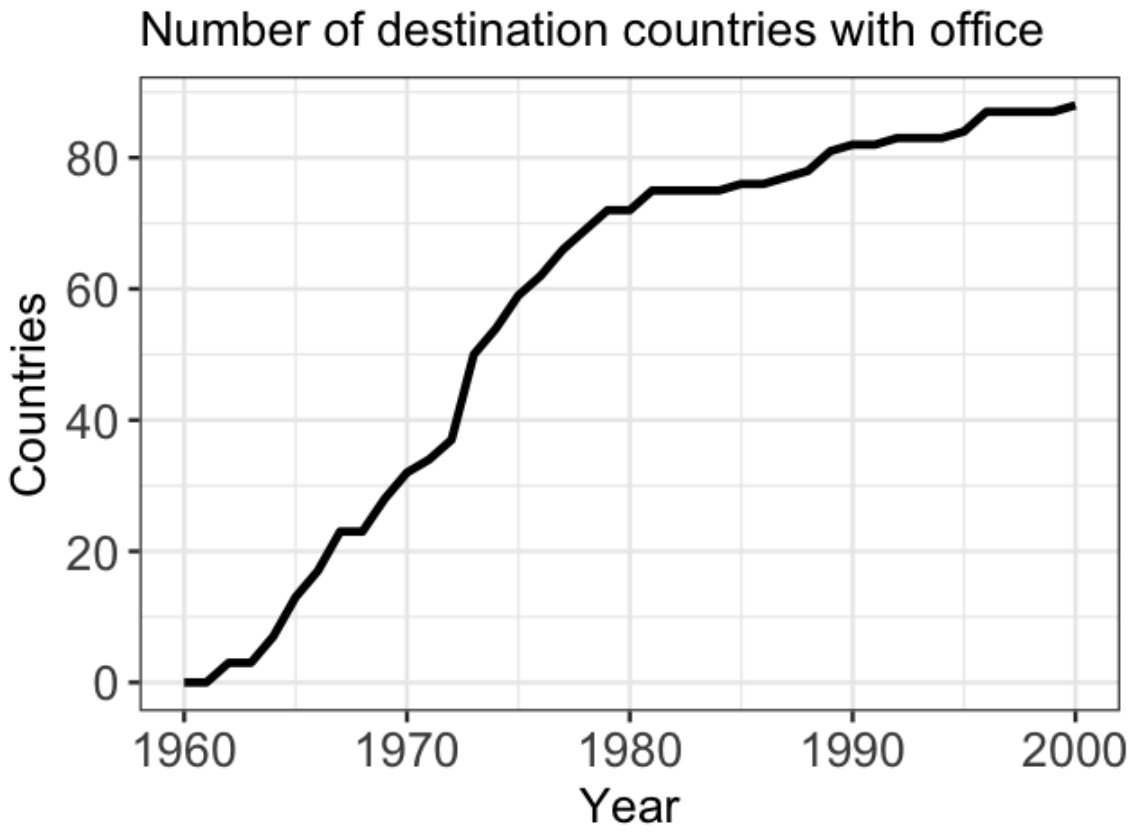
	Change in Exports compared to status quo, i.e., screening occurs after 1st appointment			
	Decomposition		Total Effect	Total Effect
Counterfactual	Appointments with single-appointment bureaucrats	Appointments with multi-appointment bureaucrats	All appointments in leave-one-out connected set	All appointments ... using covariance-shrunk bureaucrat effects
(1) Screening occurs before 1st appointment	0.223	0	0.066	0.055
(2) No screening occurs after 1st appointment	0.106	-0.139	-0.074	-0.057
(3) All as good as screened-out bureaucrats	0	-0.245	-0.172	-0.145
(4) Replace bottom quartile with median				0.095
Number of bureaucrats	200	180	380	380
Number of appointments	200	476	676	676

Notes: The table reports counterfactuals. We focus on the differential selectivity of single- and multi-appointment bureaucrats in the data, which we label as "screening." Counterfactual (1) simulates the change in exports if the average ability of single-appointment bureaucrats were set to be as high as that of multi-appointment bureaucrats. Counterfactual (2) simulates the change in exports if the average ability of both groups of bureaucrat were set to equal the average ability across *all* bureaucrats. Counterfactual (3) simulates the change in exports if the average ability of multi-appointment bureaucrats were set to be as low as that of single-appointment bureaucrats. Counterfactual (4) simulates the change in exports if the the ability of bureaucrats in the bottom quartile were set to the median ability among all bureaucrats. Counterfactuals (1), (2), and (3) are not subject to limited mobility bias as they only compare average ability between two groups, each estimated from a large number of observations. Counterfactual (4), however, is based on individual bureaucrat fixed effects, so we apply covariance shrinkage to adjust for limited mobility bias. The other counterfactuals, too, are repeated using the covariance-shrunk fixed effects, for comparability with counterfactual (4).

Online Appendix

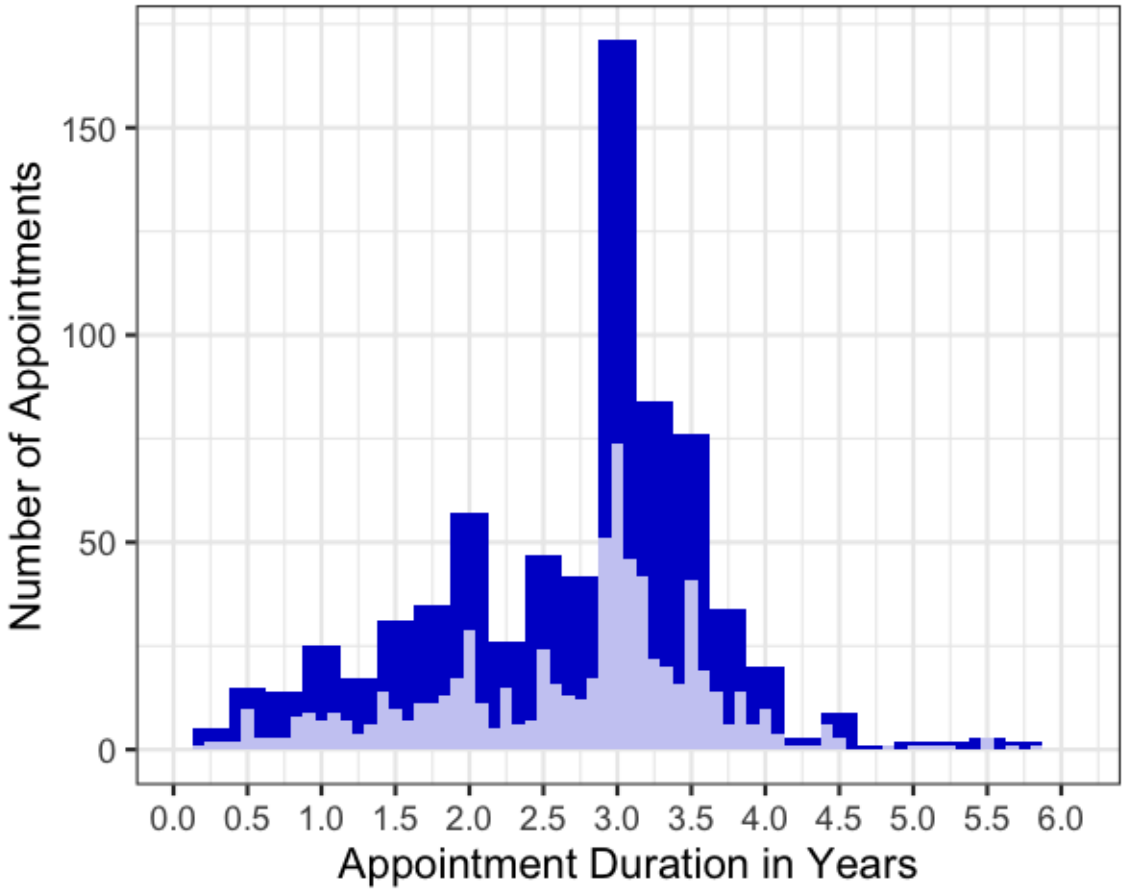
A Appendix Figures

Figure A.1: Rollout of the Policy: Number of Countries with Korean Export Promotion Office



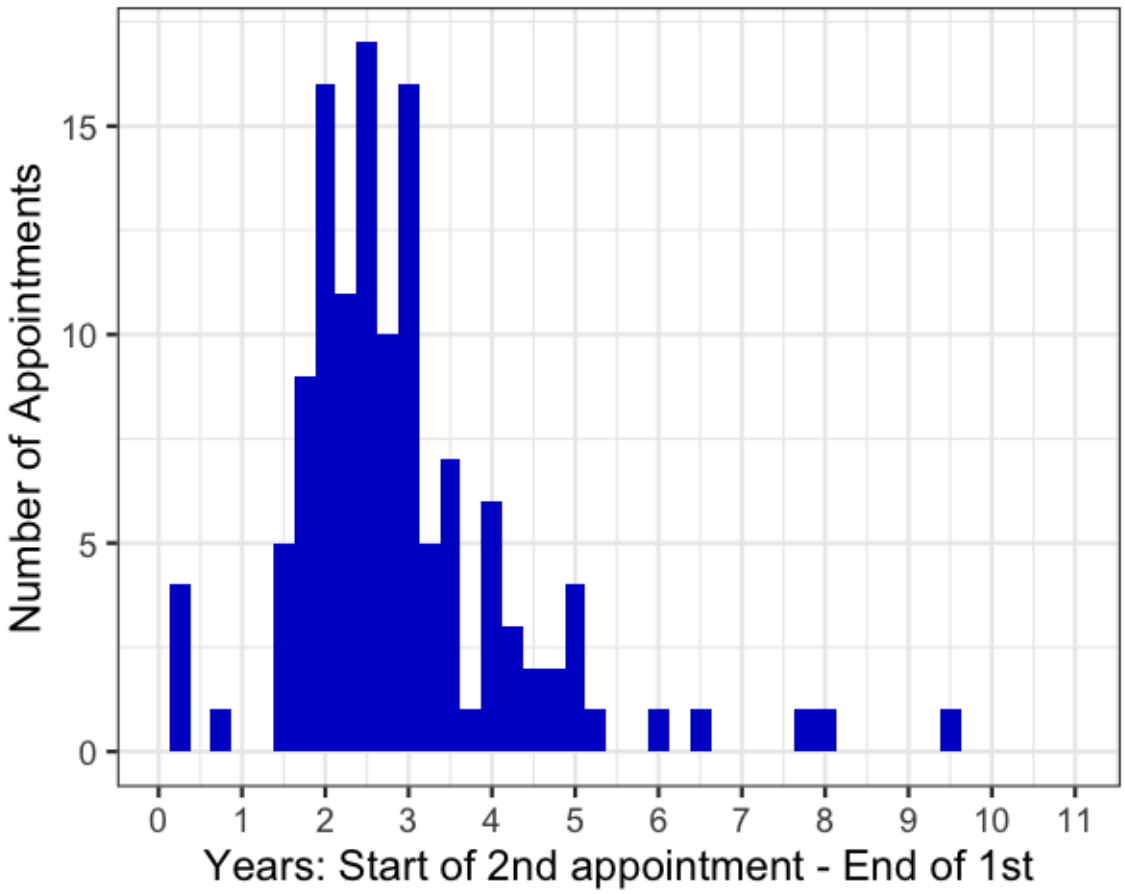
Notes: This figure presents the number of countries in which the Korean export promotion office opened by year t.

Figure A.2: Distribution of appointment durations.
Median and modal duration: 36 months.



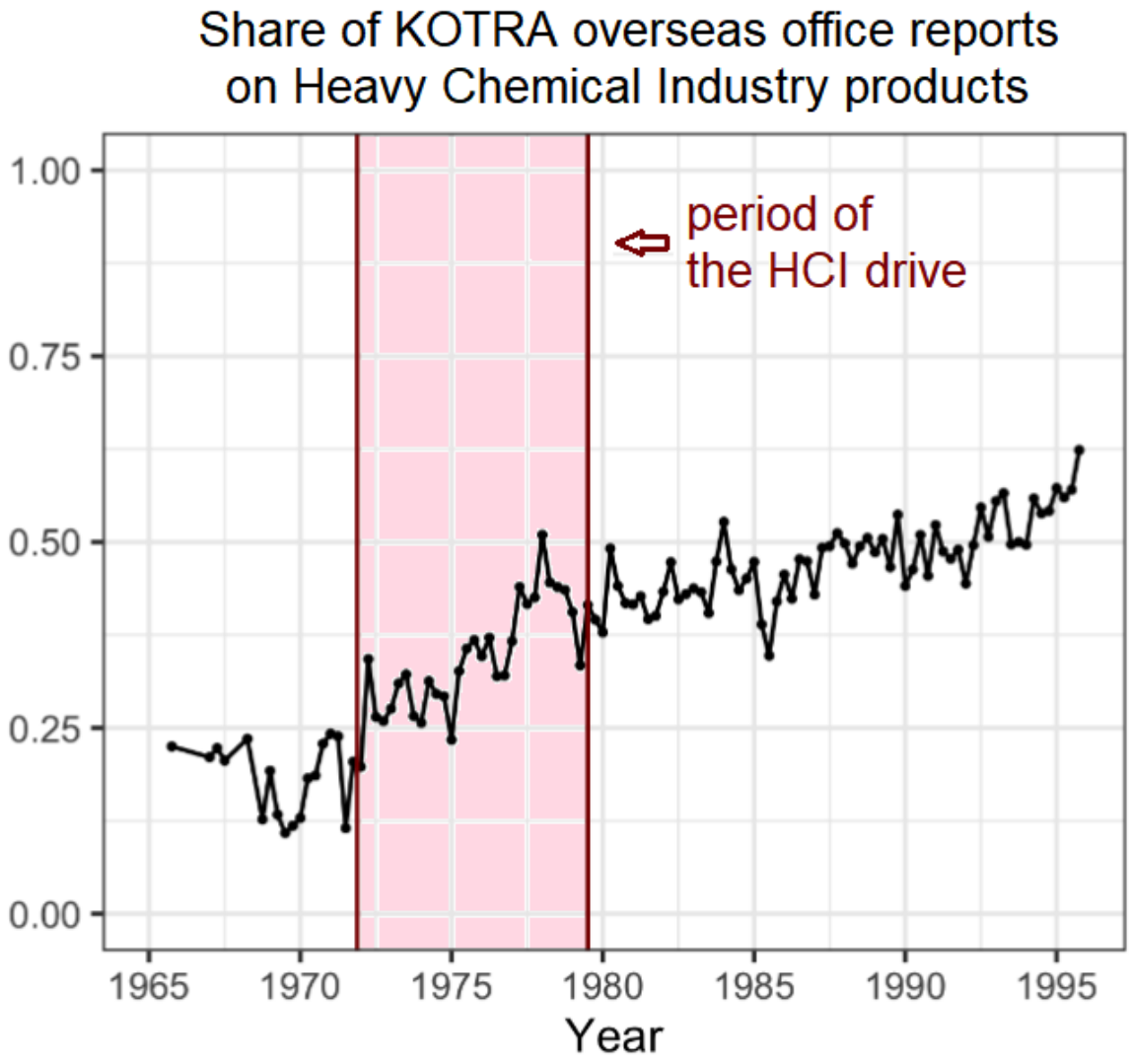
Notes: This figure represents the distribution of appointment durations. The blue bars indicate the number of appointments by quarterly duration whereas the white bars do so for the number of appointments by monthly duration. Hence, as each quarter contains multiple months, the blue bars always (weakly) exceed the white ones. E.g there are 82 appointments that last 3 years and 1 quarter. These are comprised of 42 appointments that last 3 years and 2 months, 21 appointments that last 3 years and 3 months, and 19 appointments that last 3 years and 4 months.

Figure A.3: Distribution of gap lengths.
Median: 29 months. Mode: 30 months.



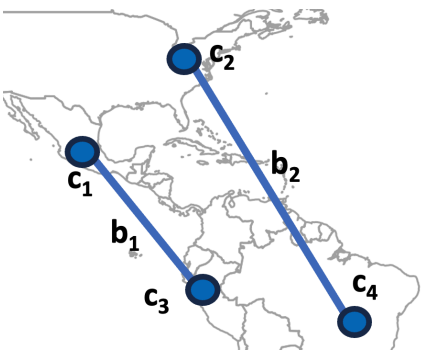
Notes: This figure represents the distribution of the duration of gaps between appointments. The blue bars indicate the number of gaps by quarterly duration.

Figure A.4: Targeting of export promotion activity by product.
Export promotion activity moves in parallel with national industrial policy



Notes: Targeting of export promotion activity by product. For each quarter, the y-axis presents the share of overseas office reports that could be linked to an HCI product relative to the number of reports that could be linked to any product.

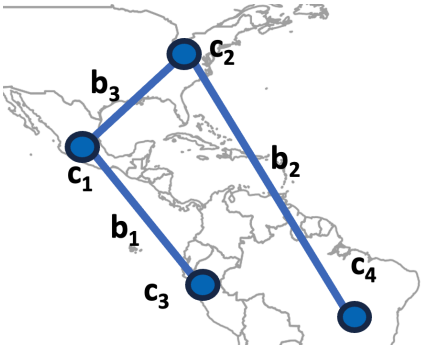
Figure A.5: KOTRA Bureaucrats' Rotation Results in a Single Connected Set



(a) Country-bureaucrat graph composed of two connected sets.



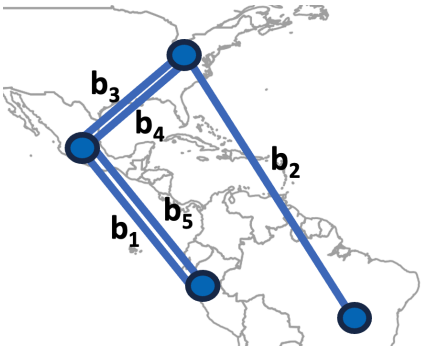
(b) Connections between the UK and other countries due to the bureaucrat appointed to manage the London office in 1981.



(c) Country-bureaucrat graph composed of single connected set.



(d) Connections from UK due to 1981 and 1984 appointments.



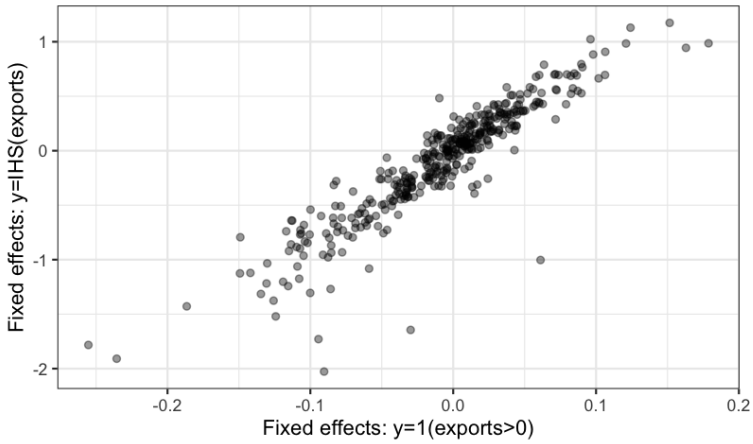
(e) Largest leave-one-out connected set includes c_1, c_2, & c_3, but not c_4.



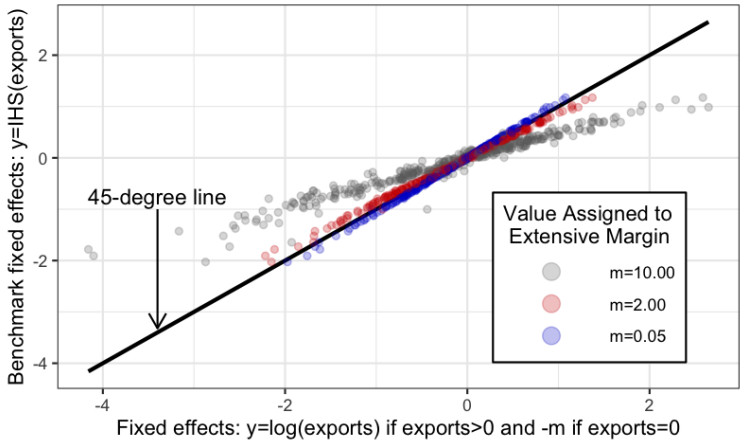
(f) Connections from UK due to 1981, 1984, 1987 appointments.

Notes: This figure highlights how this paper's data fulfills the requirement for the country-bureaucrat graph to form a single connected set. Panels (a), (c), and (e) display a hypothetical country-bureaucrat graphs. The nodes indicate the countries, the edges indicate bureaucrats who are (subsequently) observed as managers of multiple country offices – e.g., b_1 is observed in both Mexico and Peru. b_2 is observed in both Brazil and the United States. This visualization of the bureaucrat-country graph would be unchanged if there were further appointments of bureaucrats who are only ever appointed to one country. Panels (b), (d), and (f) display the connections between the UK and other countries to the appointment of bureaucrats to manage the London office in 1981, 1984, and 1987.

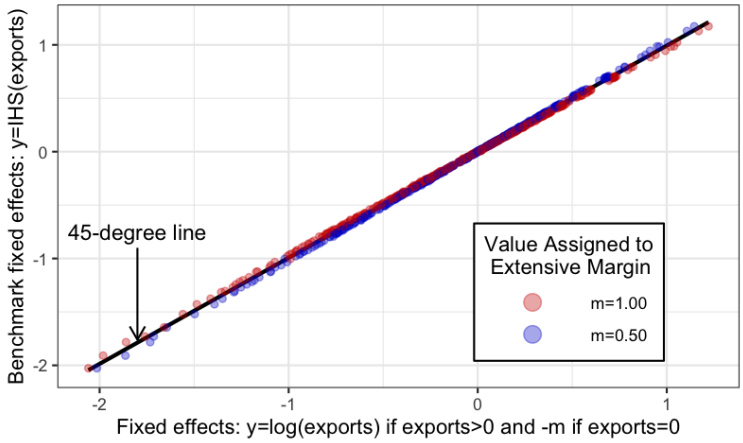
Figure A.6: Bureaucrat fixed effects under alternative transformations of exports.



(a) Compare to fixed effects based on extensive margin.



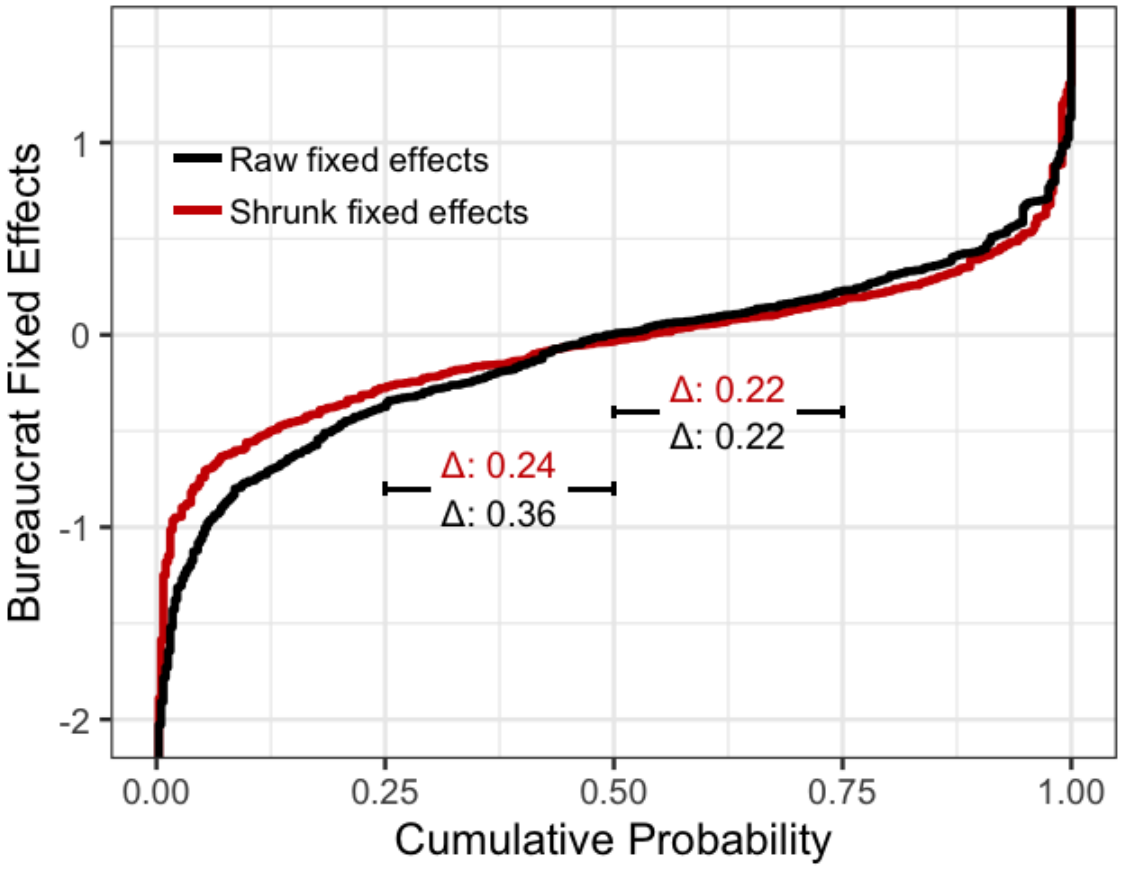
(b) Compare to FE based on explicit (extreme) values assigned to extensive margin.



(c) Compare to FE based on explicit (moderate) values assigned to ext. margin.

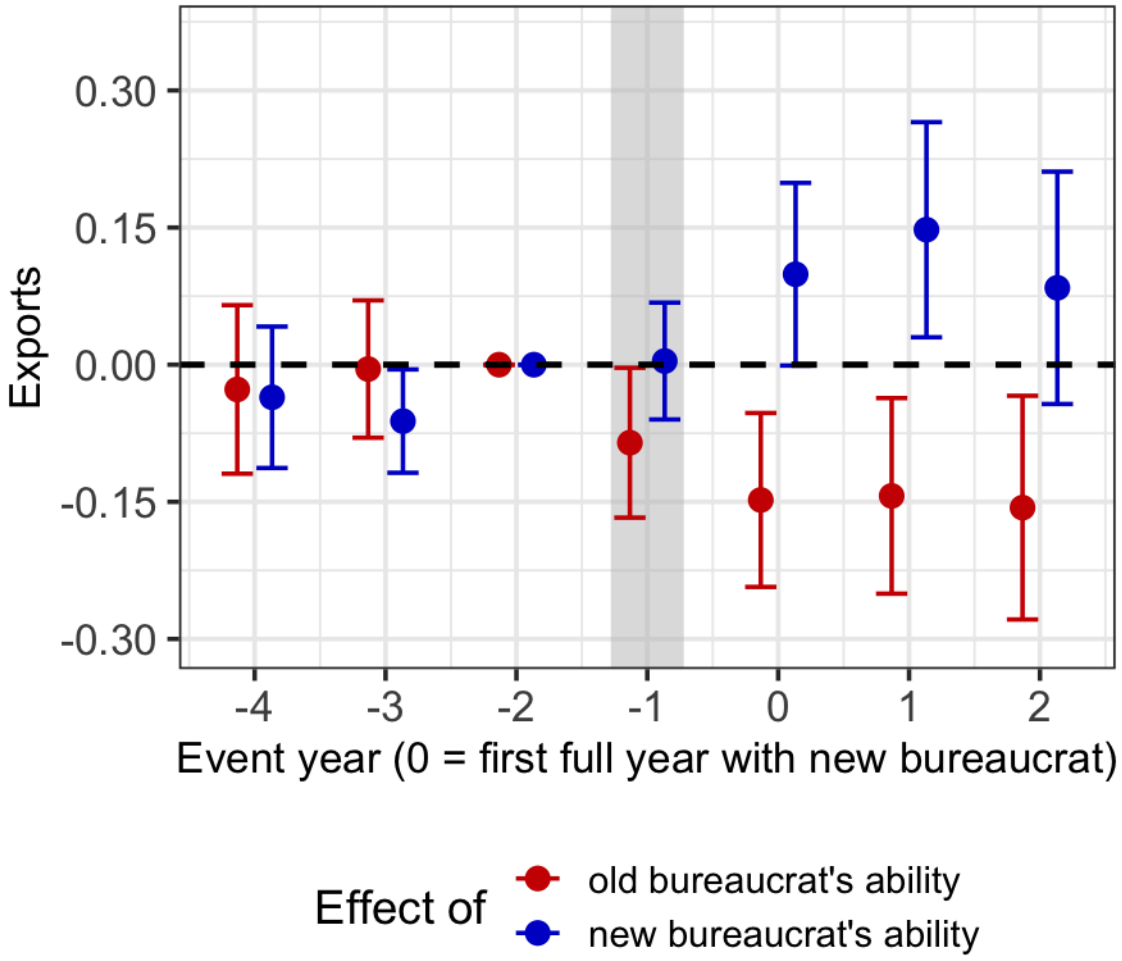
Notes: This figure displays the relationship between the benchmark bureaucrat fixed effects and those estimated under alternative transformations of exports in equation (1).

Figure A.7: CDF of Raw vs Shrunk Fixed Effects of Managers of Country Offices



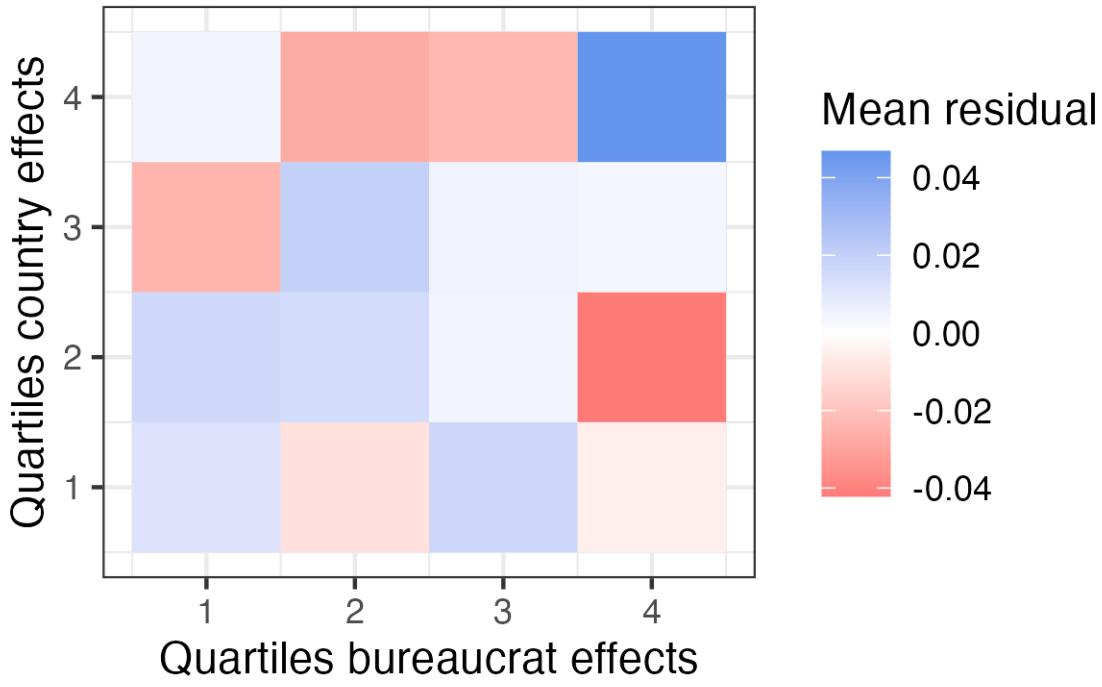
Notes: The figure shows the cumulative density function of bureaucrat fixed effects based on equation (3), in addition to covariance-shrunk bureaucrat fixed effects. The first annotation indicates that moving from the 25th to the 50th percentile would imply an increase in the inverse hyperbolic sine of exports to a country of 0.24 based on the corrected fixed effects compared to 0.36 under the raw fixed effects. The second annotation indicates that moving from the 50th to the 75th percentile would imply an increase in the inverse hyperbolic sine of 0.22 based on both the corrected and the raw fixed effects.

Figure A.8: Event study estimates: Out-of-sample bureaucrat fixed effects



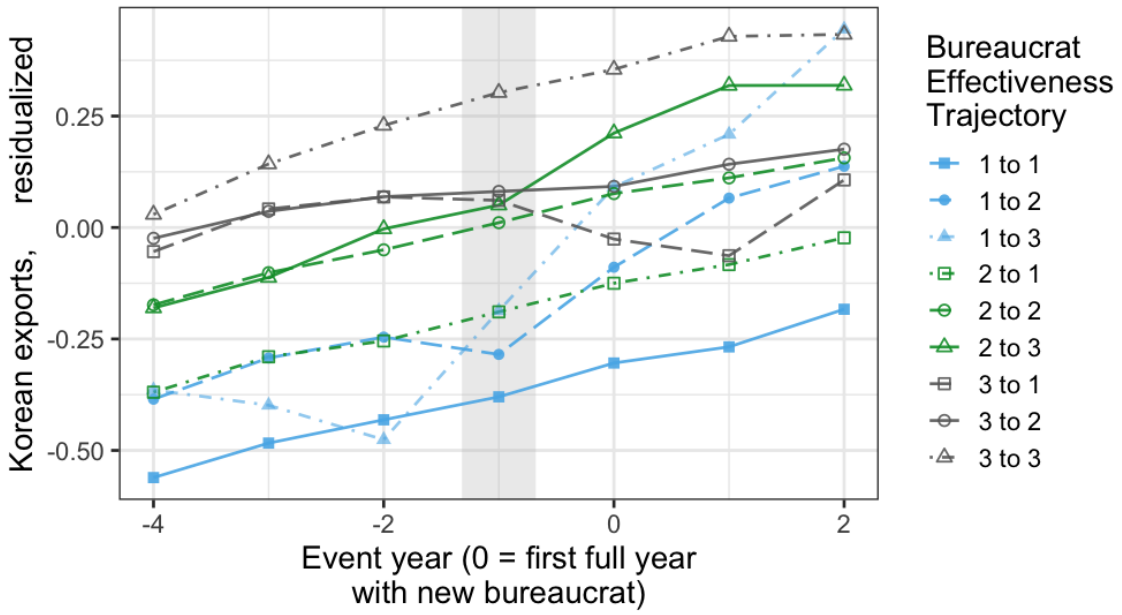
Notes: The figure shows the estimated effect of the change in bureaucrat fixed effects, estimated out of sample, on exports around the time that the manager of a country office changes. These estimates are $\hat{\beta}_k$ and $\hat{\delta}_k$ obtained from estimating equation (5). As out-of-sample fixed effects are not available for every bureaucrat, to maximize power, we report coefficients from two different models. First, we estimate equation (5) using *out-of-sample* estimates for the outgoing bureaucrat and *in-sample* estimates for the incoming bureaucrat. Second, we estimate equation (5) using *in-sample* estimates for the outgoing bureaucrat and *out-of-sample* estimates for the incoming bureaucrat. For each model, we only report the out-of-sample coefficients, as these are the ones of interest. For each model, the in-sample coefficients are almost symmetric to the out-of-sample ones. The horizontal axis indexes years in which bureaucrats work in a particular country. Year 0 is the first full year that the new bureaucrat manages the country office. Year -2 is the last full year that the old bureaucrat managed the office. The y axis measures the effect of bureaucrat effectiveness on exports. Bureaucrats effectiveness are fixed effects obtained after residualizing exports by product-country and product-year fixed effects.

Figure A.9: Residuals by estimated bureaucrat and organization effects. Absence of clear pattern which would point to misspecification.



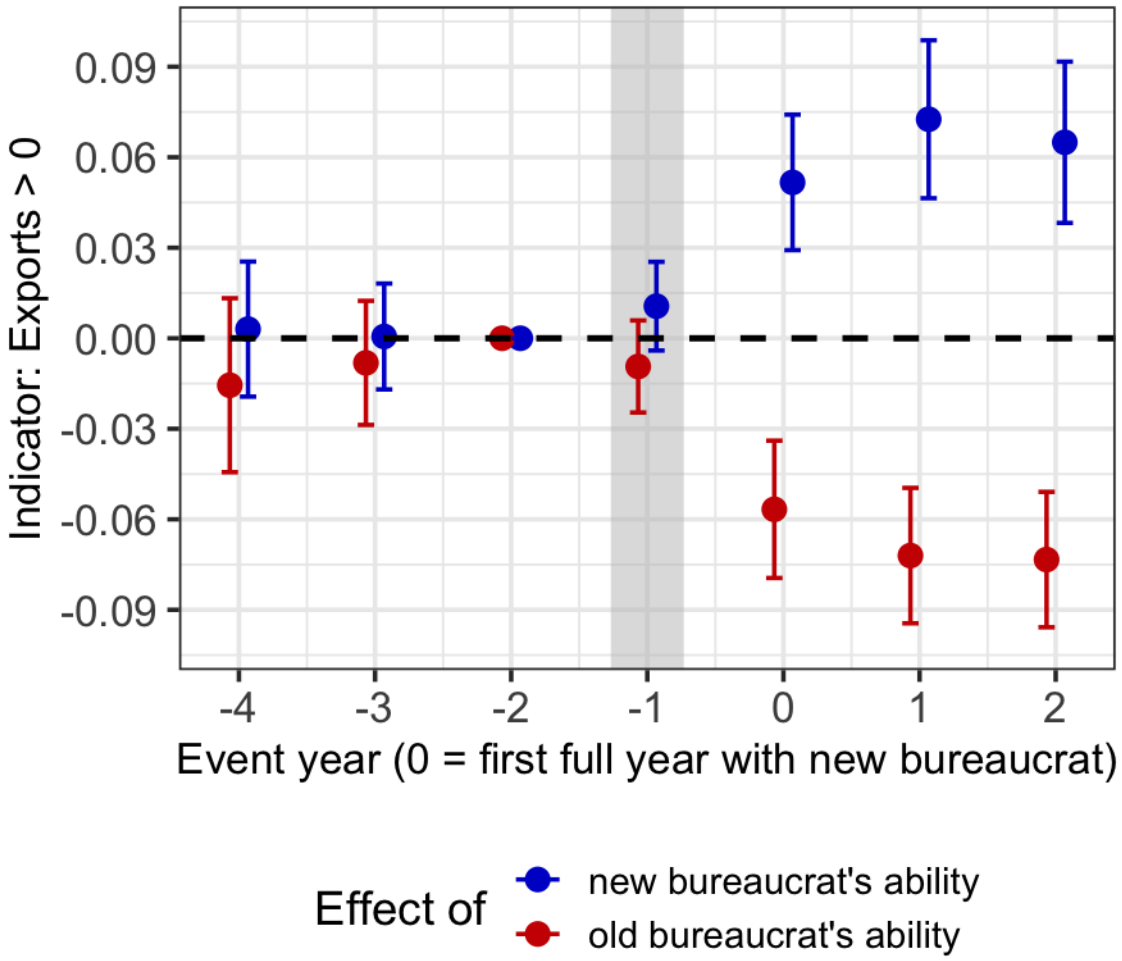
Notes: This figure shows mean residuals from equation 2 with cells defined by quartiles of estimated bureaucrat effect, interacted with quartiles of estimated country effect.

Figure A.10: Mean residualized exports around switches between bureaucrats. Effects consistent across terciles of new and old bureaucrats.



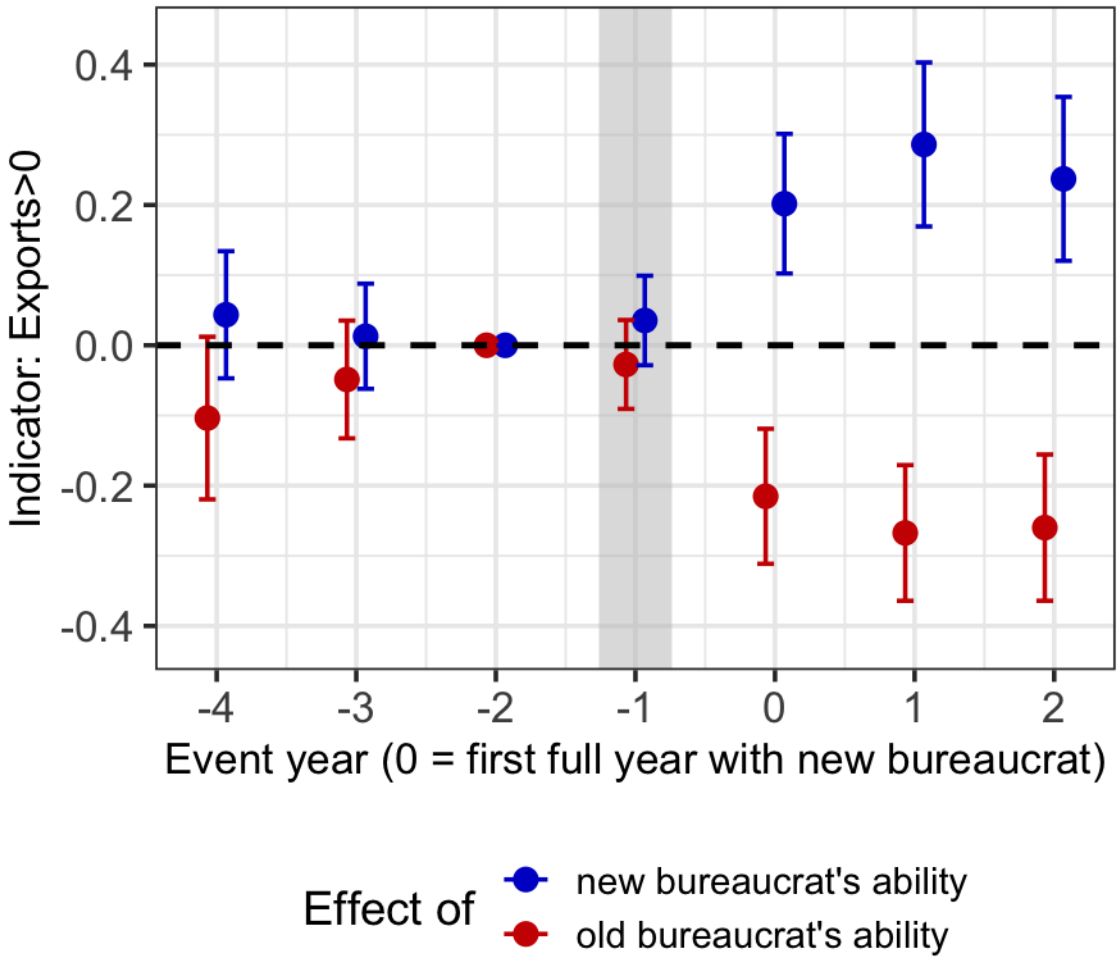
Notes: The figure shows time trends in exports around the time that the manager of a country office changes. The horizontal axis indexes years in which bureaucrats work in a particular country. Year 0 is the first full year that the new bureaucrat manages the country office. Year -2 is the last full year that the old bureaucrat managed the office. The y axis measures average residualized exports to a destination of a product. Exports are residualized by regressing product-specific exports to a country on country and product-year fixed effects. Bureaucrats are classified into terciles according to the fixed effects obtained after residualizing exports by product-country and product-year fixed effects.

Figure A.11: Event study – the extensive margin response to switches between bureaucrats



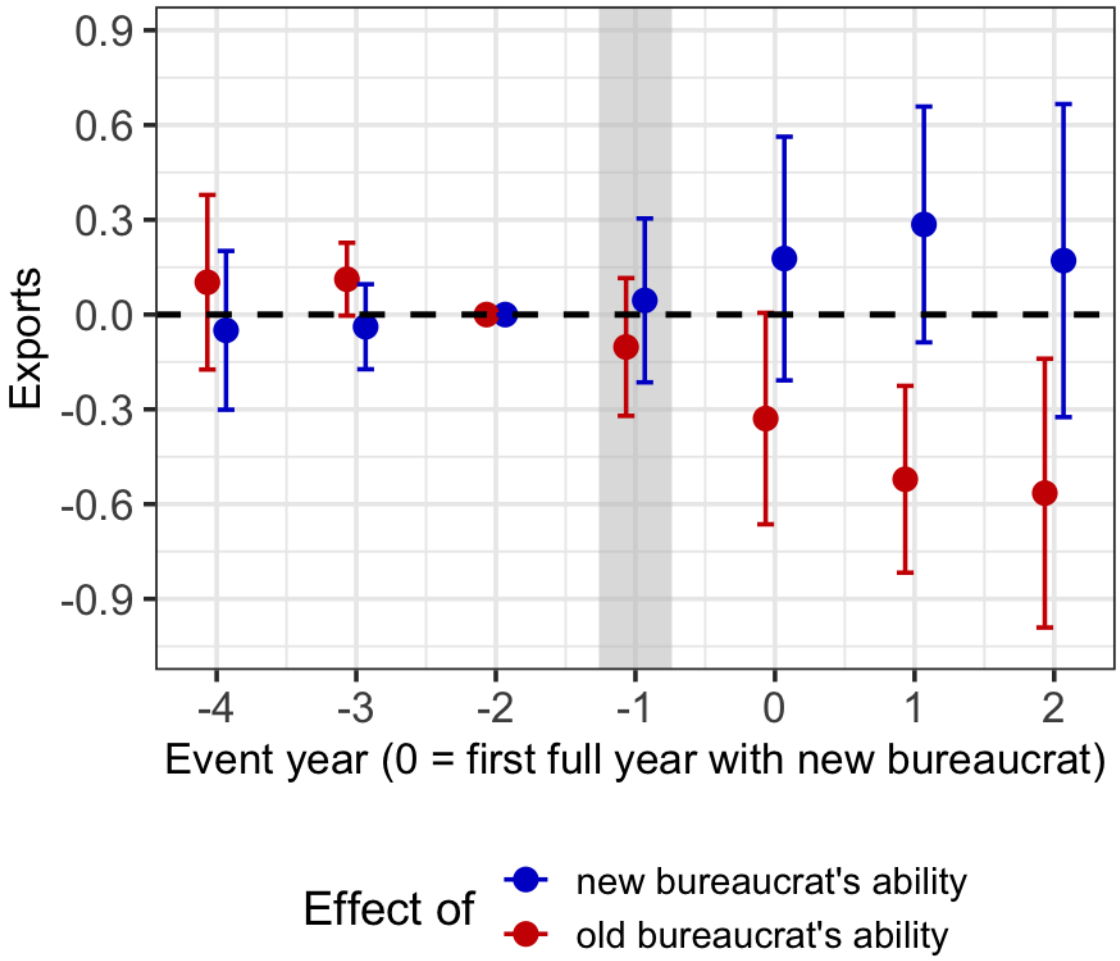
Notes: The figure shows the estimated effect of the change in bureaucrat fixed effects on the likelihood of positive exports in a given product around the time that the manager of a country office changes. These estimates are $\hat{\beta}_k$ and $\hat{\delta}_k$ obtained from estimating equation (5). Observations are included for a given event-horizon if South Korea exports this product to any country for all years in the event horizon. The horizontal axis indexes years in which bureaucrats work in a particular country. Year 0 is the first full year that the new bureaucrat manages the country office. Year -2 is the last full year that the old bureaucrat managed the office. The y axis measures the effect of bureaucrat effectiveness on exports. Bureaucrats effectiveness are fixed effects obtained after residualizing exports by product-country and product-year fixed effects.

Figure A.12: Large extensive margin response to bureaucrat effects for products with any change in extensive margin during event horizon



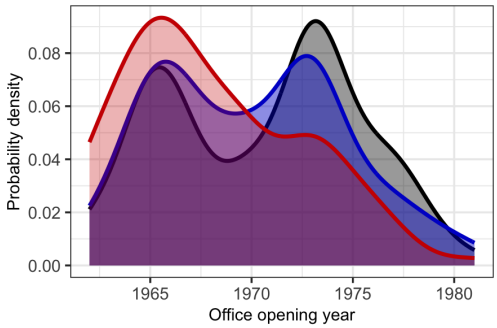
Notes: The figure shows the estimated effect of the change in bureaucrat fixed effects on the likelihood of positive exports in a given product around the time that the manager of a country office changes. These estimates are $\hat{\beta}_k$ and $\hat{\delta}_k$ obtained from estimating equation (5). Observations are included for a given event-horizon if South Korea exports this product to this country in one year during the event horizon but not all years in the event horizon. The horizontal axis indexes years in which bureaucrats work in a particular country. Year 0 is the first full year that the new bureaucrat manages the country office. Year -2 is the last full year that the old bureaucrat managed the office. The y axis measures the effect of bureaucrat effectiveness on exports. Bureaucrats effectiveness are fixed effects obtained after residualizing exports by product-country and product-year fixed effects.

Figure A.13: Event study – the intensive margin response to switches between bureaucrats

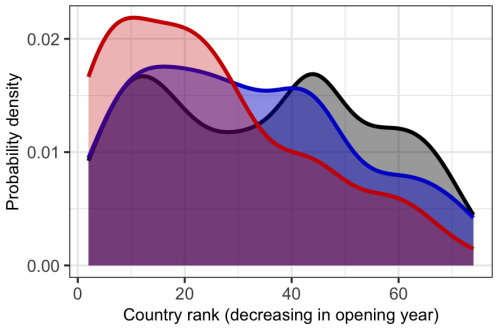


Notes: The figure shows the estimated effect of the change in bureaucrat fixed effects on exports around the time that the manager of a country office changes. This only includes the intensive margin effect as observations are included for a given event-horizon if South Korea exports this product to this country in all years during the event horizon. These estimates are $\hat{\beta}_k$ and $\hat{\delta}_k$ obtained from estimating equation (5). The horizontal axis indexes years in which bureaucrats work in a particular country. Year 0 is the first full year that the new bureaucrat manages the country office. Year -2 is the last full year that the old bureaucrat managed the office. Bureaucrats effectiveness are fixed effects obtained after residualizing exports by product-country and product-year fixed effects.

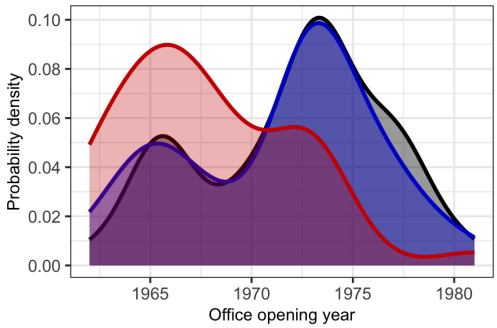
Figure A.14: As bureaucrats' careers progress they are appointed to more offices that opened earlier (proxying for importance).



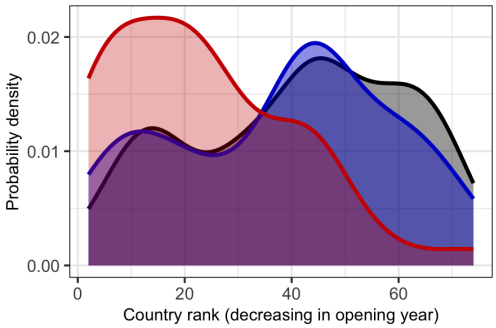
(a) PDF of bureaucrats' 1st, 2nd, 3rd appointment by opening year of the office.



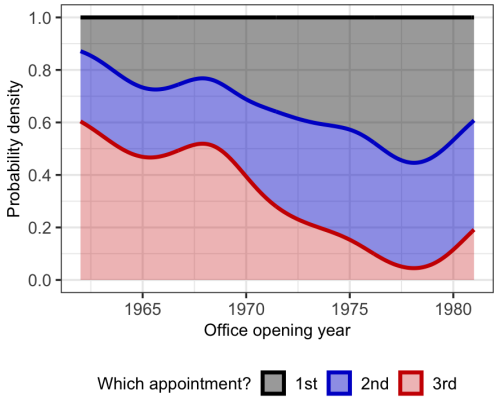
(b) PDF of bureaucrats' 1st, 2nd, 3rd appointment by opening rank of the office.



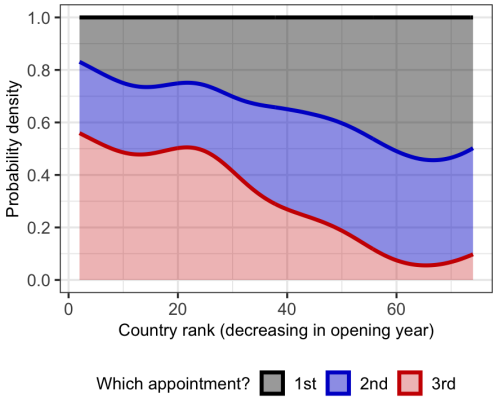
(c) Same as (a), but restricted to bureaucrats whose first appointment started no earlier than 1981.



(d) Same as (b), but restricted to bureaucrats whose first appointment started no earlier than 1981.

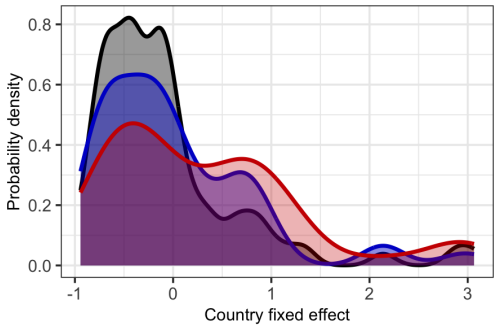


(e) Same as (c), but showing shares of appointments.

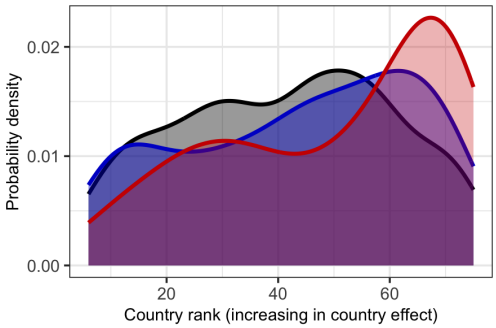


(f) Same as (d), but showing shares of appointments.

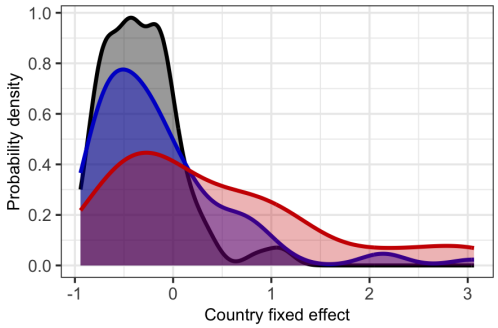
Figure A.15: As bureaucrats' careers progress they are appointed to to countries with higher fixed effects.



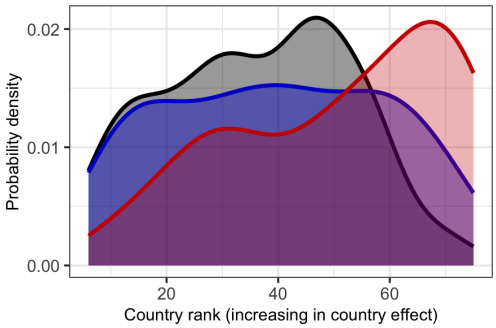
(a) PDF of bureaucrats' 1st, 2nd, 3rd appointment by country effect.



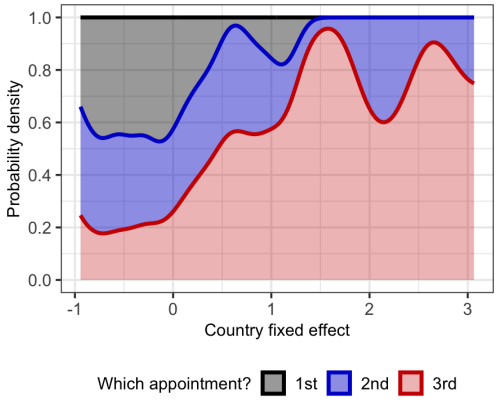
(b) PDF of bureaucrats' 1st, 2nd, 3rd appointment by *rank* of country effect.



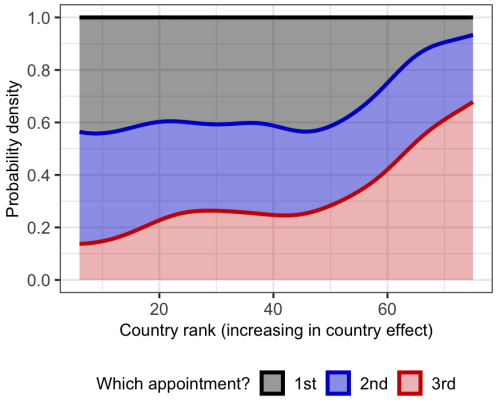
(c) Same as (a), but restricted to bureaucrats whose first appointment started no earlier than 1981.



(d) Same as (b), but restricted to bureaucrats whose first appointment started no earlier than 1981.

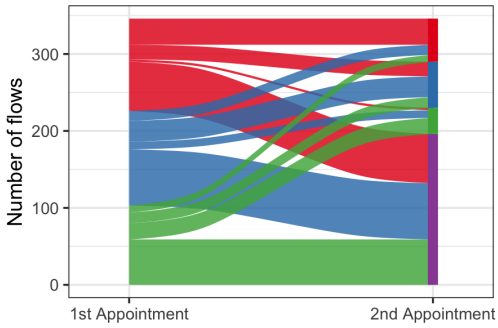


(e) Same as (c), but showing shares of appointments.

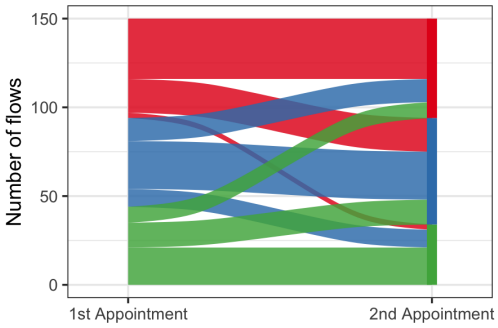


(f) Same as (d), but showing shares of appointments.

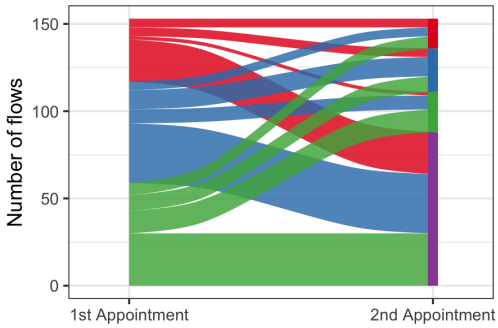
Figure A.16: Bureaucrat flows (by appointment & opening year)



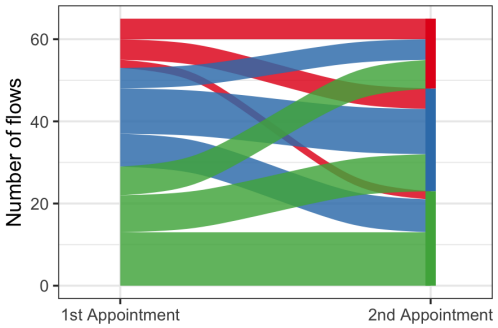
(a) All flows from 1st appointment.



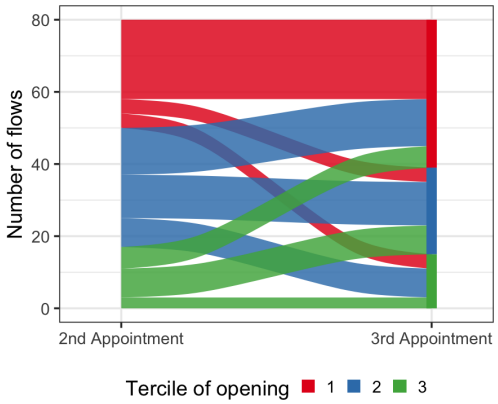
(b) Flows from 1st app. (no exit).



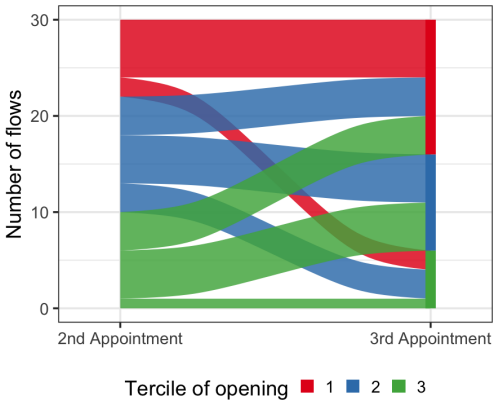
(c) Flows from 1st app. & bureaucrats without appointment before 1981.



(d) Flows (except exits) from 1st app. & bureaucrats without app. until 1981.



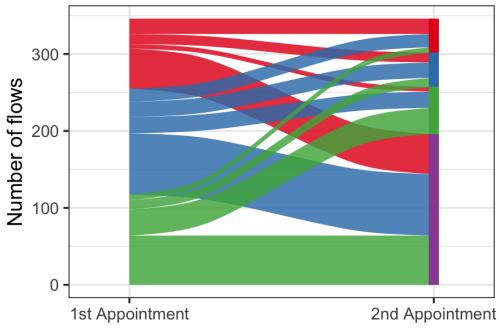
(e) Flows (except exits) from 2nd app.



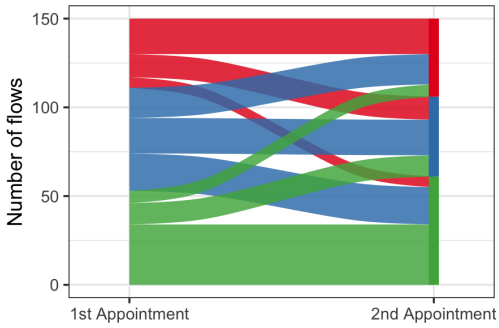
(f) Flows (except exits) from 2nd app. & bureaucrats without app. until 1981.

Notes: This figure shows the flow of bureaucrats from their 1st to 2nd appointment (2nd to 3rd in panels (e) and (f)). We split the offices into groups based on whether the office opening year is in the 1st, 2nd, or 3rd tercile of the original rollout of offices. We interpret being in an earlier tercile as a revealed preference measure of the importance that KOTRA attributes to an office.

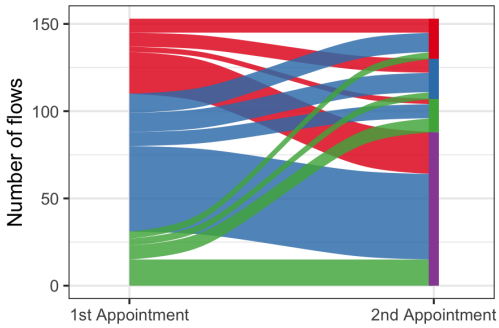
Figure A.17: Bureaucrat flows (by appointment & country effect)



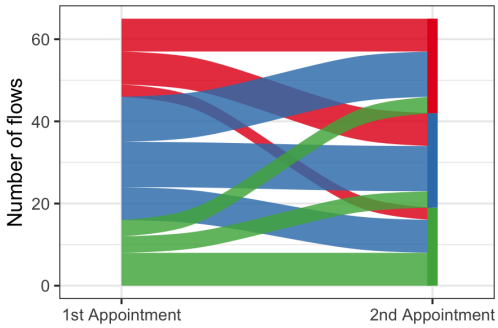
(a) All flows from 1st appointment.



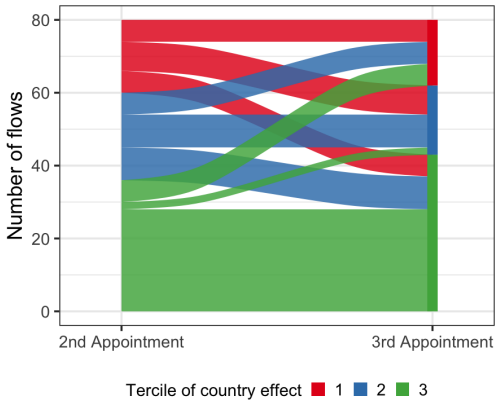
(b) Flows from 1st app. (no exit).



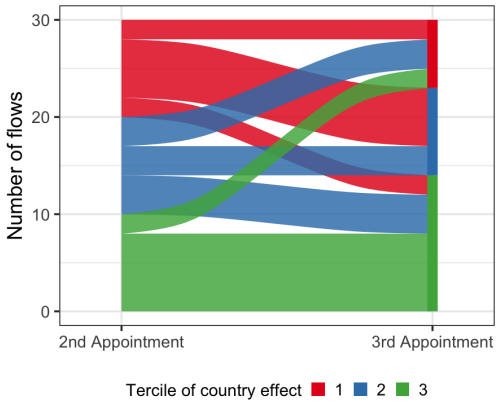
(c) Flows from 1st app. & bureaucrats without appointment before 1981.



(d) Flows (exc. exits) from 1st app. & bureaucrats without app. until 1981.



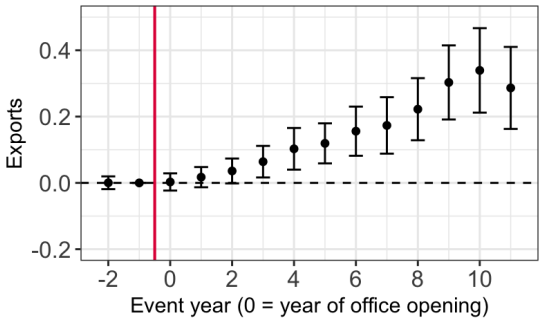
(e) Flows (except exits) from 2nd app.



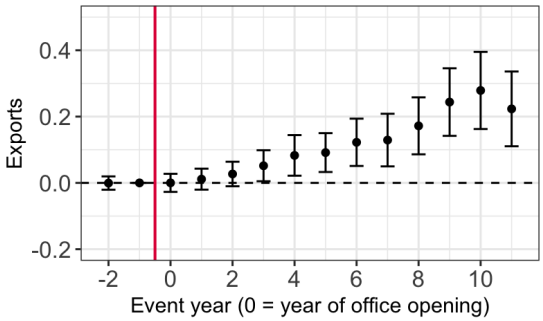
(f) Same as (e) for bureaucrats without app. until 1981.

Notes: This figure shows the flow of bureaucrats from their 1st to 2nd appointment (2nd to 3rd in panels (e) and (f)). We split the offices into terciles based on the country fixed effects. The 3rd tercile consists of the most important countries according to this metric.

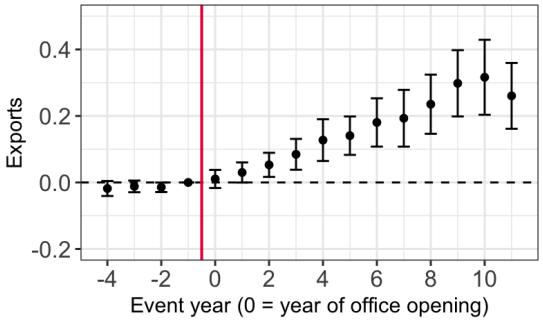
Figure A.18: Robustness: controls, sample, placebo



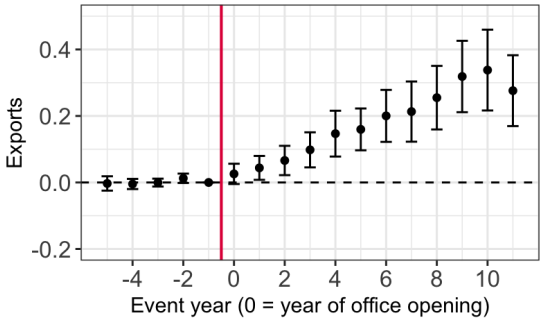
(a) Controlling for non-Korean exports.



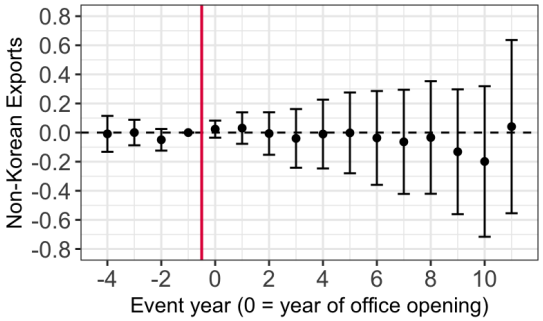
(b) Controlling for year × non-Korean exports



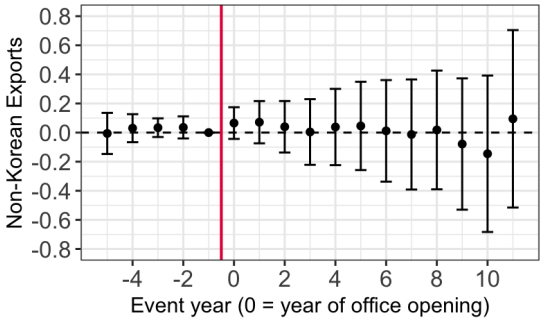
(c) Korean exports as outcome. Openings from 1966. Never-treated as control group.



(d) Korean exports as outcome. Openings from 1967. Never-treated as control group.



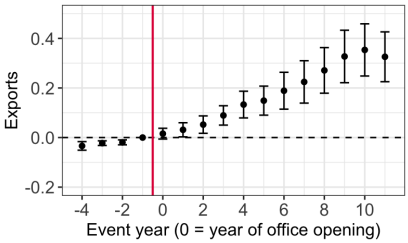
(e) Non-Korean exports as outcome. Openings from 1966. Never-treated as control group.



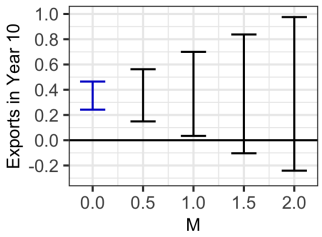
(f) Non-Korean exports as outcome. Openings from 1967. Never-treated as control group.

Notes: For panels (a)-(d), the outcome variable is the inverse hyperbolic sine of Korean exports to the country-year in question. For panels (e) and (f), the outcome is given by the inverse hyperbolic sine of non-Korean exports to the same country-year. An observation is at the product-country-year. Point estimates and standard errors are obtained from estimating equation (7), relying on a never-treated control group. Standard errors clustered at the country-level are reported around each point estimate. A product is included for all the years in which Korea exported it to any country. Product refers to 4-digit SITC Rev. 2 codes.

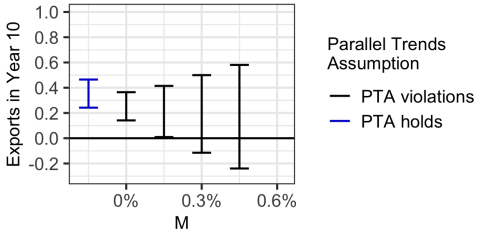
Figure A.19: Robustness: opening with not-yet-treated control



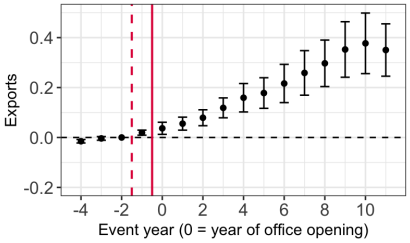
(a.i) CSA estimate, uncon
tional PTA.
0 periods of anticipation.



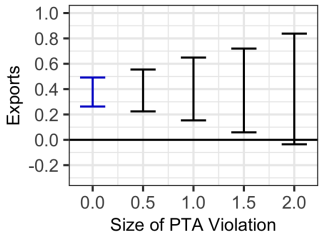
(a.ii) Sensitivity to PTA violation relative to largest pre-treatment violation of PT.



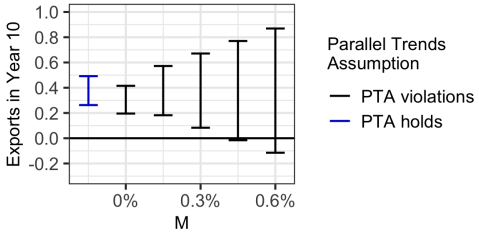
(a.iii) Sensitivity to PTA violations only bounding the extent to which the slope may change across consecutive periods.



(b.i) CSA estimate, uncon
tional PTA.
1 periods of anticipation.



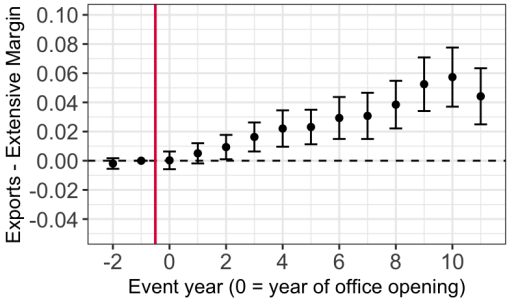
(b.ii) Sensitivity to PTA violation relative to largest pre-treatment violation of PT.



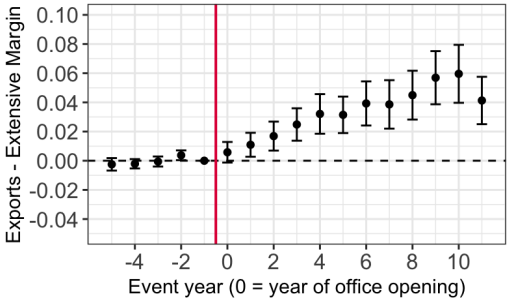
(b.iii) Sensitivity to PTA violations only bounding the extent to which the slope may change across consecutive periods.

Notes: The outcome variable is the inverse hyperbolic sine of South Korean exports to the country-year in question. The top panels report results assuming no anticipation. The bottom panel do so assuming one period of anticipation. Point estimates in (a.i) and (b.i), give the aggregation of treatment-group-specific estimates of the average treatment effect (ATT) using a not-yet-treated control group and Callaway and Sant'Anna (2021) estimator for Difference-in-Difference settings with staggered roll-out using the doubly-robust estimators form Sant'Anna and Zhao (2020). Bootstrapped standard errors are obtained clustering at the level of the destination country. Panels (a.ii-iii) report the sensitivity of the estimate in (a.i) to violations of the parallel trends assumption Rambachan and Roth (2023) It zooms in on the estimates in year 10. Panels (b.ii-iii) do the same for the estimate in (b.i). The blue bar in each panel corresponds to the 95% confidence interval of the year-10-estimate in the left panel. The black bars represent corresponding 95% confidence intervals when allowing for per-period violations of parallel trends. In panels (a.ii) and (b.ii), we bound the maximum post-treatment violation of parallel trends between consecutive periods by M times the maximum pre-treatment violation of parallel trends. In panels (a.iii) and (b.iii), we impose that the differential trends evolve smoothly over time by bounding the extent to which its slope may change across consecutive periods. Here, M represents the largest allowable change in the slope of an underlying linear trend between two consecutive periods. A product is included for all the years in which South Korea exported it to any country. Product refers to 4-digit SITC Rev. 2 codes.

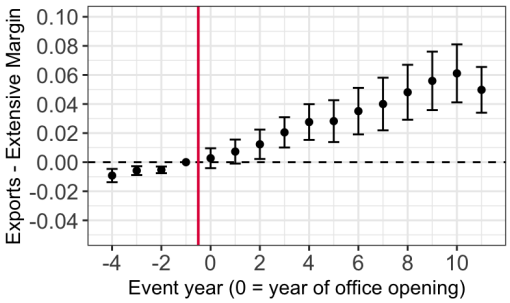
Figure A.20: Extensive margin effect of office opening



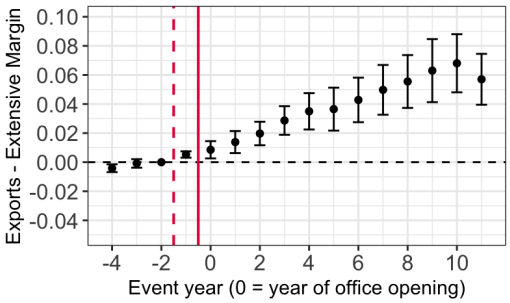
(a) Never-treated as control group. Openings from 1964.



(b) Never-treated as control group. Openings from 1967.



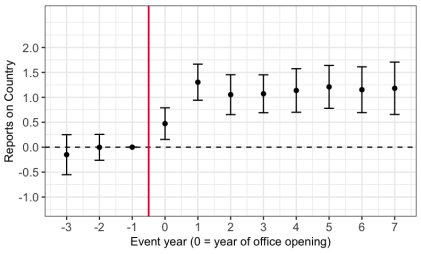
(c) "Not-yet" control. 0 period anticipation.



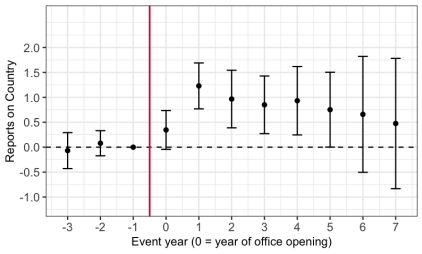
(d) "Not-yet" control. 1 period anticipation.

Notes: In each panel, the outcome variable is a dummy indicating whether South Korea had positive exports in a particular product-country-year – each panel hence corresponds to a linear probability model. An observation is at the product-country-year. For panels (a)-(b), point estimates and standard errors are obtained from estimating equation (7, relying on a never-treated control group. Standard errors clustered at the country-level are reported around each point estimate. In panels (c)-(d), point estimates give the aggregation of treatment-group-specific estimates of the average treatment effect (ATT) using a not-yet-treated control group and Callaway and Sant'Anna (2021) estimator for Difference-in-Differences settings with staggered roll-out using the doubly-robust estimators form Sant'Anna and Zhao (2020). Bootstrapped standard errors are obtained clustering at the level of the destination country. A product is included for all the years in which Korea exported it to any country. Product refers to 4-digit SITC Rev. 2 codes.

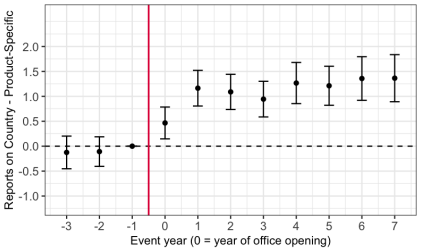
Figure A.21: Event-study estimates of office opening on KOTRA activity



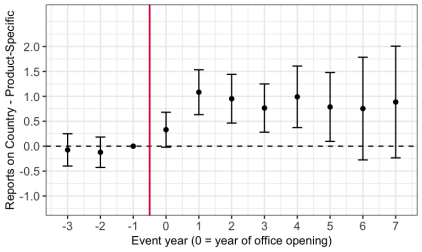
(a) Reports. Control: never-treated.



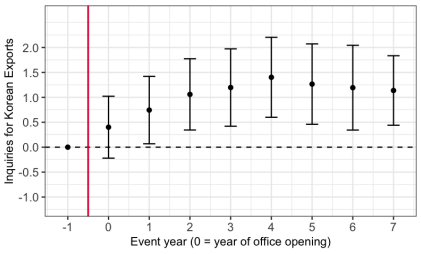
(b) Reports. Control: "not-yet".



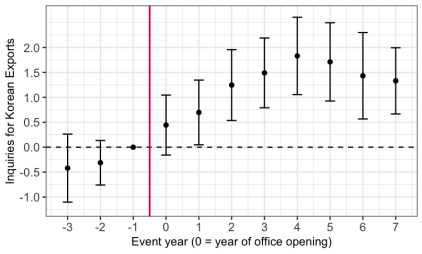
(c) Reports (product). C: never-treated.



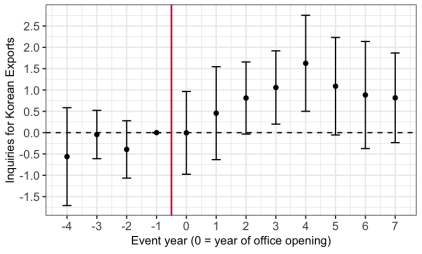
(d) Reports (product). C: "not-yet".



(e) Inquiries. Control: never-treated.



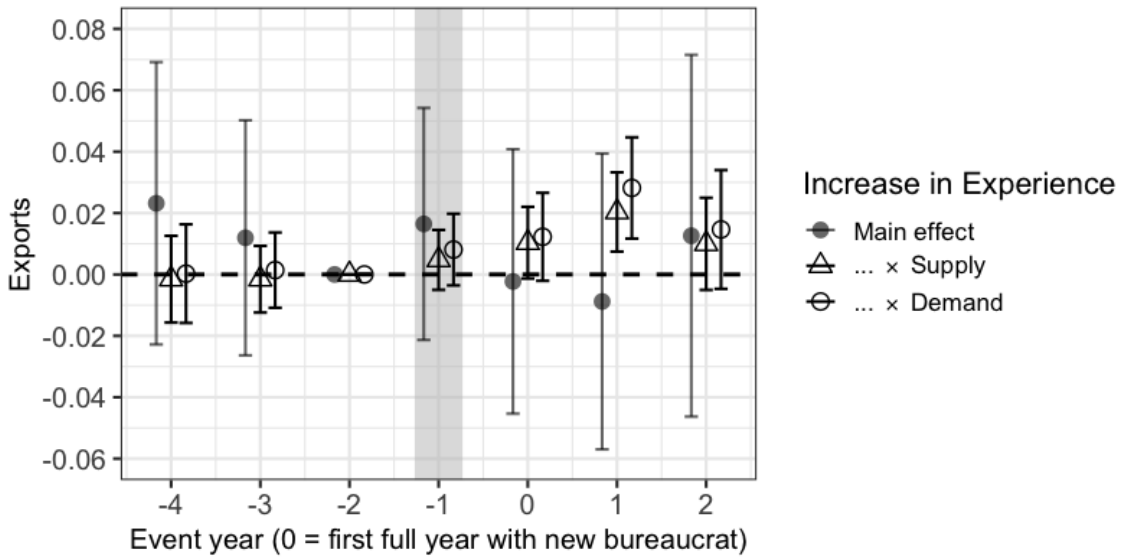
(f) Inquiries. Control: "not-yet".



(g) Inquiries. Control: never-treated. Openings from 1978.

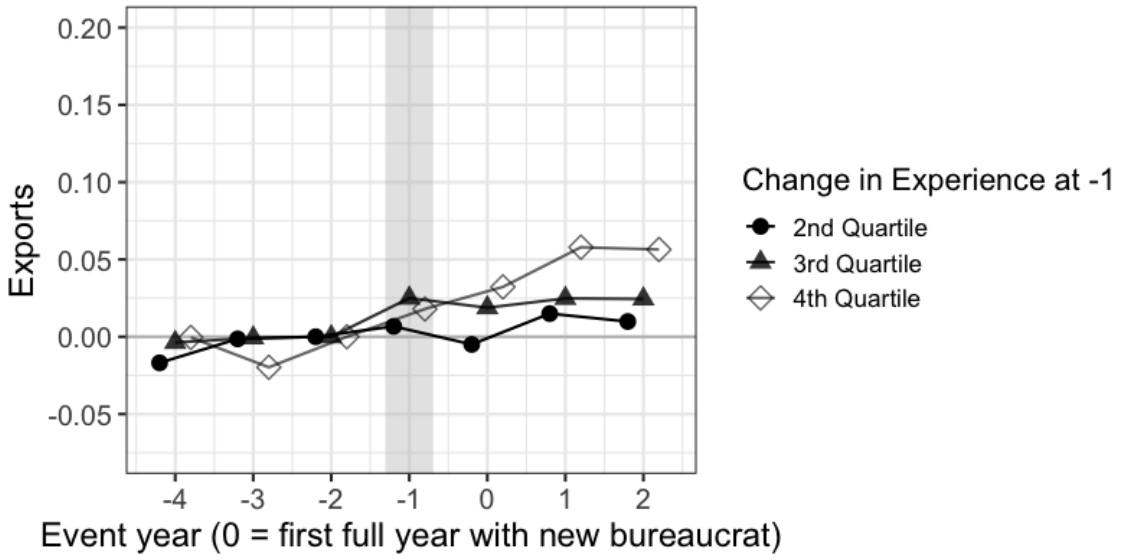
Notes: The left panels reports coefficients θ_k from estimating equation (7) when explaining three different measures of KOTRA activity regarding a specific country: number of reports written, number of product-specific reports, number of inquiries obtained - each transformed by the inverse hyperbolic sine. The right panels do the same following the approach by Callaway and Sant'Anna (2021). Instead of exports, we aim to explain three measures of KOTRA activity, each transformed using the inverse hyperbolic sine. (1) The number of reports about a country, (2) the number of product-specific reports - which may be more specific or informative, (3) the number of inquiries for trade with the country. The data on reports covers the years 1965 to 2001. We thus exclude events before 1968 from the analysis in panels (a)-(d). The data on inquiries covers the years 1974 to 1997. We thus exclude events before 1974 from the analysis in panels (e) and (f). Including events from 1975 comes at the cost of estimating only 1 pre-period in panel (e). Panel (g) takes the alternative approach of including multiple pre-periods, at the cost that the sample of treated countries is restricted to those with an event between 1978 and 1981.

Figure A.22: Event study – quasi-random experience$_p$ increases reaction to market conditions. Estimates of main effect become imprecise.



Notes: The figure shows the estimated effect of the change in the quasi-random component of bureaucrat experience when interacted with two kinds of shocks. The plotted coefficients are estimates of β_k, β_k^{demand}, and β_k^{supply} (12). The solid circles give the main effects on exports of an increase in experience. The hollow circles give the interaction with exports of the same product to the same destination by other countries (β_k^{demand}), our proxy for this destination's product-specific demand. The triangles give the interaction with South Korean exports of the same product to the other destinations (β_k^{supply}), our proxy for South Korea's product-specific supply. The horizontal axis indicates the years relative to a bureaucrat's appointment. Year 0 is the first full year that the new bureaucrat manages the country office. Year -2 is the last full year that the old bureaucrat managed the office.

Figure A.23: Event study – exports increasing in experience



Notes: The figure shows the estimated effect of the change in the quasi-random component of bureaucrat experience on exports around the time that the bureaucrat managing a country office changes. These estimates are $\hat{\beta}_{k,quartile_i}$ obtained from estimating an augmented version of equation (11) that allows for differential effects on exports for products that experience a 1st, 2nd, 3rd, and 4th quartile change in experience due to an event. The 1st quartile change is the omitted category. The solid dots indicate the effect on products experiencing a 2nd quartile change in experience compared to a 1st quartile change. The solid triangles indicate the effect on products experiencing a 3rd quartile change in experience. The hollow square does the same for products experiencing a 4th quartile change in experience. The horizontal axis indexes years in which bureaucrats work in a particular country. Year 0 is the first full year that the new bureaucrat manages the country office. Year -2 is the last full year that the old bureaucrat managed the office. The y axis measures the effect of bureaucrat experience on exports.

B Appendix Tables

Table B.1: Pre-determined market size determines office opening when distance is similar

	Opening	Non-Korean imports 1962	Predicted	Predicted (Omit own)
UK	1965	1	1965	1966
Italy	1966	4	1967	1967
Netherlands	1966	5	1967	1969
W Germany	1967	2	1966	1966
Switzerland	1967	8	1970	1972
France	1969	3	1966	1966
Sweden	1969	7	1969	1970
Austria	1970	12	1973	1973
Belgium	1972	6	1969	1969
Spain	1972	10	1972	1972
Denmark	1973	9	1972	1972
Norway	1973	11	1973	1973
Finland	1973	13	1973	1973
Greece	1973	15	1973	1973
Turkey	1973	16	1973	1974
Ireland	1973	14	1973	1973
Portugal	1974	17	1974	NA

Notes: The column 1st Opening displays the year in which a country's first office actually opened. The column Non-Korean imports in 1962 ranks the countries by the size of imports from countries other than Korea in 1962. The next column assigns the year of the nth 1st opening to the nth country as ranked by non-Korean imports in 1962. Italy is assigned the 4th opening year (1967). The final column does so while neglecting a country's own opening. Hence, Italy is assigned the 5th opening year (1967) - as this is the 4th when omitting the actual opening in Italy.

Table B.2: The effect of EP on exports depends on the individual bureaucrat. Bureaucrat effects do not differ between appointments.

	Exports			
	(1)	(2)	(3)	(4)
Share of Variation explained by FE				
Adj. R^2	0.345	0.442	0.460	0.464
R^2	0.355	0.451	0.469	0.473
Year-product FE	Yes	Yes	Yes	Yes
Country FE		Yes	Yes	Yes
Bureaucrat FE			Yes	
Bureaucrat-Country FE				Yes
Observations	1,772,452	1,772,452	1,772,452	1,772,452
Bureaucrats	397	397	397	397
Countries	87	87	87	87

Results from estimating equation (1) reported. An observation is a product-country-year. The dependent variable is exports after residualizing by product-year and country fixed effects. A country is included for all the years that it has an office and is linked to a bureaucrat. A product is included for all the years in which Korea exported it to any country. Product refers to 4-digit SITC Rev. 2 codes. S.D. of IHS exports : 2.45, s.d. of IHS exports | tp, c: 1.83. The increase in R^2 due to bureaucrat FE is most meaningfully compared to the increase due to country FE – 0.018 compared to 0.097. These levels are lower than reported in the variance decomposition as the latter bundles all observations within an appointment while this table retains separate observations for each product, thus including variation that cannot be explained by product-invariant explanatory variables such as country FE and bureaucrat FE.

Table B.3: The extensive margin's importance to each event changes little over time. Across decades, the intensive margin becomes relevant to more products.

Year of switch	Events	No. products with extensive margin change		No. products with exports> 0 throughout	
		Mean	Median	Mean	Median
1965-1969	21	96.0	76.0	17.6	8.0
1970-1974	61	119.9	108.0	30.4	16.5
1975-1979	88	138.1	124.5	37.4	27.0
1980-1984	117	169.4	153.5	62.1	47.0
1985-1989	102	163.3	149.0	52.3	24.0
1990-1994	112	144.6	144.0	82.9	55.0
1995-1999	132	154.8	150.0	127.8	89.5

This table gives the mean and median number of products across events. It first does so for products with extensive margin changes during the event horizon, i.e. products with both positive and 0 exports to the respective country. It also reports the number of products with only positive exports throughout the event horizon, i.e. products with positive exports to the respective country in each year of the event horizon.

C Further Diagnostics of Variation Explained by Bureaucrats

C.1 Misspecification checks

This section explores the additive separability between bureaucrat and country effects that is implicit in equation (3). It finds the following: (1) Residuals by quartiles of bureaucrats and countries do not indicate misspecification. (2) Bureaucrat effects are stable between appointments. (3) Upon switches between bureaucrats, expected jumps in exports occur consistently for many different types of transitions between high, middle, and low ability bureaucrats.

First, we observe that violations of additive separability would result in residuals with high absolute values for certain kinds of bureaucrat-country pairs (Fenizia, 2022; Otero and Muñoz, 2022). Following the literature, we divide our observations based on the quartile of the estimated manager fixed effect and the quartile of the estimated country fixed effect. For example, if – contrary to the linearity assumption – bureaucrats mattered more in small countries, we would expect large positive (negative) residuals for observations with top (bottom) quartile bureaucrats in bottom quartile countries. Figure A.9 shows that mean residuals do not exhibit any clear pattern such as the ones described above. Further, mean residuals are small for each combination of bureaucrat and country quartiles – between -0.05 and 0.05 in absolute value. This allays concerns about the assumption of additive separability.

Second, we explore how much the effect of a bureaucrat differs across their appointments. If bureaucrat effects differed greatly between appointments, this could indicate misspecification because either (1) bureaucrat-country are not additively separable, i.e. there are strong match effects, or (2) the estimated bureaucrat effects mainly pick up noise that is not correlated between appointments.[74] Table B.2 reports the variation explained by the different levels of fixed-effects when estimating equation (1), which identifies the causal effect of the two sets of fixed effects under the assumptions discussed above.[75] Adding bureaucrat fixed effects increases R^2 by 0.018, about 18.6% of the increase in explanatory power from adding country fixed effects.[76] To understand whether bureaucrat effects differ between appointments, we

[74] Both of these points, especially (2), also constitute a reason to test whether bureaucrat fixed effects are predictive out of sample (see section 4.4.2).

[75] While informative, these are subject to some of the criticisms addressed by the Kline, Saggio, and Sølvsten (2020) bias correction reported in table 2.

[76] Similar to the results from the variance decomposition, the explanatory power of individual effects is somewhat smaller than in other recent papers studying the role of public sector managers. The absolute increase in R^2 is smaller than other recent papers, studying managers of organizations that process insurance claims (increase in R^2 of 0.11, Fenizia), or hospital CEOs (0.09, Otero

compare the explanatory power when including appointment fixed effects (column 4) compared to column 3 which reports results from our main specification that assumes that bureaucrat and country effects are additively separable. The increase in explanatory power from this is negligible, suggesting that bureaucrat effects are relatively stable across appointments, which assuages concerns that the productivity of a bureaucrat-country match is not approximated well by the linear combination of the bureaucrat fixed effect. Further, if bureaucrat fixed effects mainly picked up statistical noise, allowing for appointment-specific effects would likely increase explanatory power more than observed.

Third, we provide a further non-parametric check that our bureaucrat fixed effects obtain meaningful variation across the different types of transitions between high and low ability bureaucrats. Appendix figure A.10 shows time trends in residualized exports around the year when an office experiences a change in the manager. It classifies switches between bureaucrats into terciles of effectiveness of the new and old bureaucrat, closely following Card, Heining, and Kline (2013), Card, Cardoso, and Kline (2016) and Best, Hjort, and Szakonyi (2023). These are obtained from average de-trended exports of a product during a bureaucrat's appointments, i.e. bureaucrat fixed effects after residualizing exports by product-country and product-year fixed effects.

Appendix figure A.10 finds that the main takeaways from figure 5 are present for transitions across all terciles of incoming and outgoing bureaucrats bureaucrats. First, in the pre-periods, exports are highest when the outgoing bureaucrat is in the top tercile and lowest when the outgoing bureaucrat is in the bottom tercile. Second, in the post-period, the effect of the outgoing bureaucrat's tercile becomes less important, the effect of the incoming bureaucrat's tercile becomes dominant. In year one – the second full year of the incoming bureaucrat – exports are lowest when the incoming bureaucrat is in the bottom tercile. They are highest when the incoming bureaucrat is in the top tercile. Third, exports change sharply, and in the expected direction, precisely when a destination switches to a less or more effective bureaucrat. Exports increase the most upon a switch to the highest tercile and (relatively) decrease the most upon a switch to the lowest tercile. The figure shows little sign that exports are differentially increasing in countries that subsequently switch to a better bureaucrat, and vice versa. This suggests that drift in effectiveness and switches are uncorrelated.

Overall, this section assuages concerns regarding the additive separability between bureaucrat and country effects implicit in equation (3).

and Muñoz). Relative to the explanatory power increase from adding country or organization fixed effects, the increase in R^2 due to bureaucrats is slightly smaller than other recent papers studying bureaucrats who run organizations that process insurance claims (23.4%, Fenizia) and public hospitals (28.0%, Otero and Muñoz).

C.2 How Estimated Ability Affect Extensive and Intensive Margin

This section unpacks the effect on the inverse hyperbolic sine of exports into the extensive and intensive margin. We find that bureaucrat effects cause increases both along the extensive and the intensive margin. Hence, both margins together explain the increase in the inverse hyperbolic sine of exports implied by the fixed effects.

Appendix figure A.11 reports the event study estimates of bureaucrat effects estimated from equation (5) with the dependent variable replaced by a dummy indicating whether South Korean exports of a particular product to this country-year exceeded 0. There is no indication of differential pre-trends. In event years 0 and 1, the new bureaucrat's ability increases the likelihood of positive exports of a given product by 5-7 percentage points, a sizable effect. The old bureaucrat's ability decreases it by the same amount, suggesting that losing bureaucrat ability has symmetric effects to gaining such ability.

Appendix figure A.12 reports the estimates using only the sample of products with extensive margin changes. For this sample, the new bureaucrat's ability increases exports by 22-31 percentage points – a very large effect. Losing a bureaucrat has a symmetric effect. There are again no differential pre-trends, especially regarding the effect of the new bureaucrat's ability.

Appendix table B.3 shows that the number of products with extensive margin changes remains roughly constant across decades. So the extensive margin response remains similarly important over time. However, appendix table B.3 shows an increase over time in the number of products for which only the intensive margin matters.[77] Appendix figure A.13 replicates figure 5 using data on only these products for which only the intensive margin matters. As expected, the estimates become noisier. However, pre-trends remain absent, the point estimates go in the expected direction, and are quantitatively similar to figure 5. Due to the decreased statistical power, only the coefficients on the old bureaucrat's effect remain statistically significant.

Overall, this section shows that a bureaucrat estimated to be high ability – using the inverse hyperbolic sine of exports – increases both the intensive and extensive margin of exports.

[77] The omitted – shrinking – category contains products without any exports throughout the event horizon.

C.3 Sensitivity of Fixed Effects to Alternative Transformations of Exports

This section investigates how much the bureaucrat fixed effect estimates would change under alternative transformations of export values.

Appendix figure A.6 panel (a) displays a scatterplot comparing the benchmark bureaucrat fixed effects estimates for $y_{cpt} = \text{exports}_{cpt} = \text{IHS}(\text{export_value}_{cpt})$, to alternative estimates based on $y_{cpt} = \mathbb{1}\{\text{export_value}_{cpt} > 0\}$. The alternative fixed effects purely account for the share of products with any positive Korean export value to country c in year t. Naturally, the precise estimates change greatly due to this substantial change in the outcome variable. Nevertheless, panel (a) shows a strongly increasing, largely linear relationship between the benchmark fixed effect estimates and the estimates based on the extensive margin.

Appendix figure A.6 panels (b) and (c) display a scatter plot comparing the benchmark estimates to alternative estimates based on the following transformation, following Chen and Roth (2023):

$$y_{cpt} = \begin{cases} log(\text{export_value}_{cpt}), & \text{if export_value}_{cpt} > 0, \\ -m, & \text{if export_value}_{cpt} = 0. \end{cases}$$

Under this transformation, the weight of the extensive margin is explicitly assigned via the choice of m. Panel (b) displays alternative vs. benchmark estimates based on rather extreme values of m: 0.05, 2.00, 10.00. While the estimates differ based on the value of m, they remain close to the 45-degree line. Thus, benchmark fixed effects are very predictive of fixed effects under these alternative transformations. There is slightly less dispersion when valuing the extensive margin less ($m = 0.05$) and somewhat more when valuing the extensive margin greatly ($m \in \{2, 10\}$).

Panel (c) shows that the benchmark estimates' dispersion of fixed effects lies between estimates based on $m = 0.50$ and $m = 1.00$, providing bounds for weight implicitly assigned to the extensive margin by the inverse hyperbolic sine transformation. It's further noteworthy that these two values of m result in very similar fixed effect estimates, speaking to the robustness of our results to different valuations of the extensive margin.

D Connected Set and Leave-One-Out Connected Set

Appendix figure A.5 illustrates the connected set – connected via moves of bureaucrats connecting all countries. Panel (c) also highlights that if the movement of a single bureaucrat (b_3) connects two separate connected sets – e.g., the bottom-left to the top-right – then the estimated difference in fixed effect between countries, and hence bureaucrats, in the two sets of countries is strongly affected by any shock that occurs during b_3's appointments. For instance, if there is a positive shock of size $\hat{\epsilon}$ in country c_2 during b_3's appointment to c_2, estimating equation (1) would yield a positive bias in the fixed effect estimate for $\hat{\gamma}_{c_2}$ (relative to $\hat{\gamma}_{c_1}$ and $\hat{\gamma}_{c_3}$). This would spill over into a negative bias in $\hat{\beta}_{b_2}$, and hence a positive bias in $\hat{\beta}_b$ for all bureaucrats b that are ever only appointed to countries c_2 or c_4. These biases result in the limited mobility bias: the variance of estimated bureaucrat fixed effects overstates the variance in exports due to individual bureaucrats.

Panel (e) displays a country-bureaucrat graph where Mexico, Peru, and the U.S. constitute a single leave-one-out connected set. This is the sample of countries and bureaucrats that remains connected even when removing any single appointment (bureaucrat-country pair) from the data. By restricting attention to such a leave-one-out connected set, the limited mobility bias is greatly attenuated (Kline, Saggio, and Sølvsten, 2020). More importantly, under weak assumptions the leave-one-out connected set allows us to correct the variance in bureaucrat fixed effects and obtain consistent estimates of the variance in ability (Kline, Saggio, and Sølvsten, 2020).

Appendix figure A.5, panels (b), (d), and (f), highlight how many connections offices or countries can have with only three different bureaucrats. The panels display all the connections between the UK and other countries because of only three consecutive appointments of managers to London (1981, 1984, and 1987).

E Covariance Shrinkage and Bootstrap Bias Correction

In this section, we slightly change the notation in two ways for simplicity and clarity.

1. Instead of $\theta_{b(c,t)}$ and γ_c, we call bureaucrat fixed effects θ_i, for bureaucrat $i = 1, ..., n$ and country fixed effects γ_j, for country $j = 1, ..., C$.

2. We distinguish the dimension over which a variance, covariance, or expectation is computed through subscripts. For example, $\mathsf{Var}_i(\hat{\theta}_i)$ is the variance of $\hat{\theta}_i$

across bureaucrats, $\mathsf{Cov}_{ij}(\theta_i, \gamma_j)$ is covariance of θ_i and γ_j across bureaucrats and countries, and $\mathsf{Var}(\hat{\theta}_i)$ – no subscript – is the variance of $\hat{\theta}_i$ for an individual bureaucrat i. Based on this notation, the variance decomposition equation (3) becomes[78]

$$\mathsf{Var}_{ij}[(\text{exports}|pt)_{ij}] = \mathsf{Var}_i(\theta_i) + \mathsf{Var}_j(\gamma_j) + 2\mathsf{Cov}_{ij}(\theta_i, \gamma_c) + \mathsf{Var}_{ij}(\epsilon_{ij})$$

E.1 Bootstrap bias correction

The plug-in estimators of the variances and covariance of bureaucrat and country fixed effects, $\mathsf{Var}_i(\hat{\theta}_i)$, $\mathsf{Var}_j(\hat{\gamma}_j)$, and $\mathsf{Cov}_{ij}(\hat{\theta}_i, \hat{\gamma}_j)$, are biased due to sampling errors in the fixed effects estimates. The bootstrap bias correction computes the degree of bias by estimating the variances and covariances of the sampling errors via bootstrap.

Let us first focus on the variance of bureaucrat fixed effects. Equation (2) will produce an estimate of θ_i: $\hat{\theta}_i = \theta_i + v_i$, where v_i is sampling error. The variance of the estimated bureaucrat fixed effects, $\mathsf{Var}_i(\hat{\theta}_i) = \mathsf{Var}_i(\theta_i) + \mathsf{Var}_i(v_i) = \sigma^2_\theta + \sigma^2_v$, will overstate the true dispersion in bureaucrat quality, σ^2_θ.

By the law of total variance, $\sigma^2_v \equiv \mathsf{Var}_i(v_i) = \mathsf{E}_i(\mathsf{Var}(v_i)) + \mathsf{Var}_i(\mathsf{E}(v_i)) = \mathsf{E}_i(\sigma^2_{v_i}) + \mathsf{Var}_i(0) = \mathsf{E}_i(\sigma^2_{v_i})$. Hence, an estimate of σ^2_v is given by a weighted average of the sampling-error variances of individual bureaucrats:

$$\sigma^2_v = \mathsf{E}_i(\sigma^2_{v_i}) \Rightarrow \hat{\sigma}^2_v = \frac{\sum_{i=1}^n \hat{\sigma}^2_{v_i} w_i}{\sum_{i=1}^n w_i}$$

where the weights w_i reflect the weight of bureaucrat i (the number of country-product-year observations of i) in the original sample.

The variance of the sampling error for bureaucrat i, $\hat{\sigma}^2_{v_i}$, is estimated by bootstrapping[79] equation (2) B[80] times to get the variance of $\hat{\theta}_i$ across bootstrap samples $k = 1, ..., B$:

$$\sigma^2_{v_i} = \mathsf{Var}(\hat{\theta}_i) \Rightarrow \hat{\sigma}^2_{v_i} = \widehat{\mathsf{Var}}_k(\hat{\theta}^k_i) = \frac{\sum_{k=1}^B (\hat{\theta}^k_i - \bar{\hat{\theta}}_i)^2 w^k_i}{\sum_{k=1}^B w^k_i}$$

where $\hat{\theta}^k_i$ is the estimated fixed effect of i in bootstrap sample k, the weights w^k_i are the weight of i in bootstrap sample k, and $\bar{\hat{\theta}}_i = \frac{\sum_{k=1}^B \hat{\theta}^k_i w^k_i}{\sum_{k=1}^B w^k_i}$.

Then, $\hat{\sigma}^2_\theta = \widehat{\mathsf{Var}}_i(\hat{\theta}_i) - \hat{\sigma}^2_v$. $\hat{\sigma}^2_\theta = 0.100$, reported in column 3 of Table 2.

The variance of the country fixed effects can be estimated similarly. Where $\hat{\gamma}_j =$

[78] Recall that fixed effects estimated on the raw data and those estimated on the data collapsed at the bureaucrat-country level are identical.

[79] We cluster-bootstrap at the appointment (bureaucrat-country match) level, the level of the variation identifying the bureaucrat and country fixed effects.

[80] We choose $B = 500$, which we confirm to be a sufficiently large number so that the sampling-error variance and covariance estimates stabilize.

$\gamma_j + u_j$ is the estimated fixed effect of country j, with u_j representing the sampling error, we can estimate the dispersion of country fixed effects, σ_γ^2, by:

$$\hat{\sigma}_\gamma^2 = \widehat{\mathsf{Var}}_j(\hat{\gamma}_j) - \hat{\sigma}_u^2.$$

$\hat{\sigma}_\gamma^2 = 0.682$, reported in column 3 of Table 2.

The covariance of the estimated bureaucrat and country fixed effects is also biased:

$$\begin{aligned}\mathsf{Cov}_{ij}(\hat{\theta}_i, \hat{\gamma}_j) &= \mathsf{Cov}_{ij}(\theta_i, \gamma_j) + \mathsf{Cov}_{ij}(\theta_i, u_j) + \mathsf{Cov}_{ij}(\gamma_j, v_i) + \mathsf{Cov}_{ij}(v_i, u_j) \\ &= \mathsf{Cov}_{ij}(\theta_i, \gamma_j) + \mathsf{Cov}_{ij}(v_i, u_j) \\ &= \sigma_{\theta\gamma} + \sigma_{vu}\end{aligned}$$

By the law of total covariance, $\sigma_{vu} \equiv \mathsf{Cov}_{ij}(v_i, u_j) = \mathsf{E}_{ij}(\mathsf{Cov}(v_i, u_j)) + \mathsf{Cov}_{ij}(\mathsf{E}(v_i), \mathsf{E}(u_j)) = \mathsf{E}_{ij}(\sigma_{v_i u_j}) + \mathsf{Cov}_{ij}(0, 0) = \mathsf{E}_{ij}(\sigma_{v_i u_j})$. Then,

$$\sigma_{vu} = \mathsf{E}_{ij}(\sigma_{v_i u_j}) \Rightarrow \hat{\sigma}_{vu} = \frac{\sum_{i=1}^{n}\sum_{j=1}^{C} \hat{\sigma}_{v_i u_j} w_{ij}}{\sum_{i=1}^{n}\sum_{j=1}^{C} w_{ij}} \tag{13}$$

where w_{ij} reflect the weight of the (i, j) pair (the number of product-year observations of each (i, j) pair) in the original sample.

The covariance of the sampling errors for bureaucrat i and country j in an (i, j) pair[81] is again estimated from the bootstrap results:

$$\sigma_{v_i u_j} = \mathsf{Cov}(v_i, u_j) \Rightarrow \hat{\sigma}_{v_i u_j} = \widehat{\mathsf{Cov}}_k(\hat{\theta}_i^k, \hat{\gamma}_j^k) = \frac{\sum_{k=1}^{B}(\hat{\theta}_i^k - \bar{\bar{\hat{\theta}}}_i)(\hat{\gamma}_j^k - \bar{\bar{\hat{\gamma}}}_j) w_{ij}^k}{\sum_{k=1}^{B} w_{ij}^k} \tag{14}$$

where w_{ij}^k is the weight of (i, j) in bootstrap sample k, and $\bar{\bar{\hat{\theta}}}_i = \frac{\sum_{k=1}^{B} \hat{\theta}_i^k w_{ij}^k}{\sum_{k=1}^{B} w_{ij}^k}$, $\bar{\bar{\hat{\gamma}}}_j = \frac{\sum_{k=1}^{B} \hat{\gamma}_j^k w_{ij}^k}{\sum_{k=1}^{B} w_{ij}^k}$.

Finally, $\hat{\sigma}_{\theta\gamma} = \widehat{\mathsf{Cov}}_{ij}(\hat{\theta}_i, \hat{\gamma}_j) - \hat{\sigma}_{vu}$. $\hat{\sigma}_{\theta\gamma} = -0.044$, reported in column 3 of Table 2.

E.2 Shrinkage

Although $\hat{\theta}_i$ are unbiased estimates of θ_i, they are inconsistent in small samples. Shrinking $\hat{\theta}_i$ makes them biased but reduces their variance. The shrinkage factor for each bureaucrat is chosen to minimize their mean-squared-error:

$$\lambda_i^* = \arg\min_{\lambda_i} \underbrace{\mathsf{E}\left[\left(\theta_i - \lambda_i \hat{\theta}_i\right)^2\right]}_{\mathrm{MSE}_i^{\mathrm{Sh}}} = \frac{\sigma_\theta^2}{\sigma_\theta^2 + \sigma_{v_i}^2}$$

[81] If i and j are not matched, i.e. bureaucrat i is never appointed to country j, then $\sigma_{v_i u_j} = 0$.

The shrinkage estimator of bureaucrat i's fixed effect is therefore

$$\hat{\theta}_i^{\text{Sh}} = \frac{\sigma_\theta^2}{\sigma_\theta^2 + \sigma_{v_i}^2} \cdot \hat{\theta}_i$$

The variance of the shrunk bureaucrat fixed effects theoretically understates the true variance:

$$\begin{aligned}
\mathsf{Var}_i(\hat{\theta}_i^{\text{Sh}}) &= \mathsf{E}_i\left[\mathsf{Var}(\hat{\theta}_i^{\text{Sh}})\right] + \mathsf{Var}_i\left(\mathsf{E}(\hat{\theta}_i^{\text{Sh}})\right) \quad \text{(Law of total variance)} \\
&= \mathsf{E}_i\left[\mathsf{Var}(\hat{\theta}_i^{\text{Sh}})\right] + \mathsf{Var}_i\left(\mathsf{E}\left(\frac{\sigma_\theta^2}{\sigma_\theta^2 + \sigma_{v_i}^2}\hat{\theta}_i\right)\right) \\
&= \mathsf{E}_i\left[\mathsf{Var}(\hat{\theta}_i^{\text{Sh}})\right] + \mathsf{Var}_i\left(\frac{\sigma_\theta^2}{\sigma_\theta^2 + \sigma_{v_i}^2}\mathsf{E}(\hat{\theta}_i)\right) \\
&= \mathsf{E}_i\left[\mathsf{Var}(\hat{\theta}_i^{\text{Sh}})\right] + \mathsf{Var}_i\left(\frac{\sigma_\theta^2}{\sigma_\theta^2 + \sigma_{v_i}^2} \cdot 0\right) \\
&= \mathsf{E}_i\left[\mathsf{Var}(\hat{\theta}_i^{\text{Sh}})\right] \\
&= \mathsf{E}_i\left[\mathsf{Var}\left(\frac{\sigma_\theta^2}{\sigma_\theta^2 + \sigma_{v_i}^2} \cdot \hat{\theta}_i\right)\right] \\
&= \mathsf{E}_i\left[\left(\frac{\sigma_\theta^2}{\sigma_\theta^2 + \sigma_{v_i}^2}\right)^2 \mathsf{Var}(\hat{\theta}_i)\right] \\
&= \mathsf{E}_i\left[\left(\frac{\sigma_\theta^2}{\sigma_\theta^2 + \sigma_{v_i}^2}\right)^2 (\sigma_\theta^2 + \sigma_{v_i}^2)\right] \\
&= \mathsf{E}_i\left[\frac{\sigma_\theta^2}{\sigma_\theta^2 + \sigma_{v_i}^2} \cdot \sigma_\theta^2\right] \\
&= \mathsf{E}_i\left[\frac{\sigma_\theta^2}{\sigma_\theta^2 + \sigma_{v_i}^2}\right] \cdot \sigma_\theta^2 < \sigma_\theta^2
\end{aligned} \tag{15}$$

To compute $\hat{\theta}_i^{\text{Sh}}$ in practice, σ_θ^2 and $\sigma_{v_i}^2$ are not known so we replace them with $\hat{\sigma}_\theta^2$ and $\hat{\sigma}_{v_i}^2$, found in Appendix section E.1.

E.3 Covariance-shrinkage

Covariance-shrinkage, developed by Best et al. (2023), is an extension of the common shrinkage method applicable in the two-way fixed effects setup, which explicitly accounts for the correlation between the two dimensions of fixed effects.

Covariance shrinkage weights the full vector of bureaucrat and country fixed effects to minimize the mean-squared-error for each bureaucrat or country fixed effect. The covariance shrinkage matrix is found by stacking the minimization problem for all

fixed effects:

$$\boldsymbol{\Lambda}^* = \arg\min_{\boldsymbol{\Lambda}} \underbrace{\mathsf{E}[(\boldsymbol{\rho} - \boldsymbol{\Lambda}\hat{\boldsymbol{\rho}})'(\boldsymbol{\rho} - \boldsymbol{\Lambda}\hat{\boldsymbol{\rho}})]}_{\text{MSE}^{\text{CovSh}}} \tag{16}$$

where $\boldsymbol{\rho} = (\theta_1, \theta_2, ..., \theta_n, \gamma_1, ..., \gamma_C)'$.

$$\text{MSE}^{\text{CovSh}} = \mathsf{E}(\boldsymbol{\rho}'\boldsymbol{\rho}) - 2\mathsf{E}(\boldsymbol{\rho}'\boldsymbol{\Lambda}\hat{\boldsymbol{\rho}}) + \mathsf{E}\left((\boldsymbol{\Lambda}\hat{\boldsymbol{\rho}})'\boldsymbol{\Lambda}\hat{\boldsymbol{\rho}}\right) \tag{17}$$

$$= \text{Tr}(\boldsymbol{\Sigma}_{\boldsymbol{\rho}\boldsymbol{\rho}}) - 2\text{Tr}(\boldsymbol{\Lambda}\boldsymbol{\Sigma}_{\hat{\boldsymbol{\rho}}\boldsymbol{\rho}}) + \text{Tr}(\boldsymbol{\Lambda}\boldsymbol{\Sigma}_{\hat{\boldsymbol{\rho}}\hat{\boldsymbol{\rho}}}\boldsymbol{\Lambda}') \tag{18}$$

$$\frac{\partial \text{MSE}^{\text{CovSh}}}{\partial \boldsymbol{\Lambda}}\Big|_{\boldsymbol{\Lambda}=\boldsymbol{\Lambda}^*} = -2\boldsymbol{\Sigma}_{\hat{\boldsymbol{\rho}}\boldsymbol{\rho}} + 2\boldsymbol{\Lambda}^*\boldsymbol{\Sigma}_{\hat{\boldsymbol{\rho}}\hat{\boldsymbol{\rho}}} = 0 \tag{19}$$

$$\Rightarrow \boldsymbol{\Lambda}^* = \boldsymbol{\Sigma}_{\hat{\boldsymbol{\rho}}\boldsymbol{\rho}}\boldsymbol{\Sigma}_{\hat{\boldsymbol{\rho}}\hat{\boldsymbol{\rho}}}^{-1} \tag{20}$$

where $\boldsymbol{\Sigma}_{\hat{\boldsymbol{\rho}}\boldsymbol{\rho}}$ is the covariance matrix between $\hat{\boldsymbol{\rho}}$ and $\boldsymbol{\rho}$, and $\boldsymbol{\Sigma}_{\hat{\boldsymbol{\rho}}\hat{\boldsymbol{\rho}}}$ is the variance-covariance matrix of $\hat{\boldsymbol{\rho}}$.

Specifically,

$$\boldsymbol{\Sigma}_{\hat{\boldsymbol{\rho}}\boldsymbol{\rho}} = \begin{pmatrix} \sigma_\theta^2 \mathbf{I}_n & \sigma_{\theta\gamma}\mathbf{1}_{n\times C} \\ \sigma_{\theta\gamma}\mathbf{1}_{C\times n} & \sigma_\gamma^2 \mathbf{I}_C \end{pmatrix} \tag{21}$$

because

$$(\boldsymbol{\Sigma}_{\hat{\boldsymbol{\rho}}\boldsymbol{\rho}})_{ij} = \mathsf{Cov}(\hat{\rho}_i, \rho_j) = \mathsf{Cov}(\rho_i, \rho_j) = \begin{cases} \mathsf{Var}(\theta_i) = \sigma_\theta^2 & \text{if } i = j, i \le n \\ \mathsf{Cov}(\theta_i, \theta_j) = 0 & \text{if } i \ne j, i \le n, j \le n \\ \mathsf{Cov}(\theta_i, \gamma_{j-n}) = \sigma_{\theta\gamma} & \text{if } i \ne j, i \le n, j > n \\ \mathsf{Var}(\gamma_{j-n}) = \sigma_\gamma^2 & \text{if } i = j, j > n \\ \mathsf{Cov}(\gamma_{i-n}, \gamma_{j-n}) = 0 & \text{if } i \ne j, i > n, j > n \\ \mathsf{Cov}(\gamma_{i-n}, \theta_j) = \sigma_{\theta\gamma} & \text{if } i \ne j, i > n, j \le n \end{cases}$$

Moreover,

$$\boldsymbol{\Sigma}_{\hat{\boldsymbol{\rho}}\hat{\boldsymbol{\rho}}} = \boldsymbol{\Sigma}_{\hat{\boldsymbol{\rho}}\boldsymbol{\rho}} + \begin{pmatrix} \mathsf{diag}(\{\sigma_{v_i}^2\}_{i=1}^n) & \boldsymbol{\Gamma} \\ \boldsymbol{\Gamma}' & \mathsf{diag}(\{\sigma_{u_j}^2\}_{j=1}^C) \end{pmatrix} \tag{22}$$

where $\boldsymbol{\Gamma} = \left[\sigma_{v_i u_j}\right]$ is a $n \times C$ matrix of the covariances of sampling errors, because

$$(\boldsymbol{\Sigma}_{\hat{\boldsymbol{\rho}}\hat{\boldsymbol{\rho}}})_{ij} = \mathsf{Cov}(\hat{\rho}_i, \hat{\rho}_j) = \mathsf{Cov}(\rho_i, \rho_j) + \begin{cases} \mathsf{Var}(v_i) = \sigma_{v_i}^2 & \text{if } i = j, i \le n \\ \mathsf{Cov}(v_i, v_j) = 0 & \text{if } i \ne j, i \le n, j \le n \\ \mathsf{Cov}(v_i, u_{j-n}) = \sigma_{v_i u_{j-n}} & \text{if } i \ne j, i \le n, j > n \\ \mathsf{Var}(u_{j-n}) = \sigma_{u_{j-n}}^2 & \text{if } i = j, j > n \\ \mathsf{Cov}(u_{i-n}, u_{j-n}) = 0 & \text{if } i \ne j, i > n, j > n \\ \mathsf{Cov}(u_{i-n}, v_j) = \sigma_{v_j u_{i-n}} & \text{if } i \ne j, i > n, j \le n \end{cases}$$

and $\sigma_{v_i u_j} = 0$ if (i, j) is not a matched bureaucrat-county pair.

The covariance-shrunk fixed effect of bureaucrat i is

$$\hat{\theta}_i^{\text{CovSh}} = \boldsymbol{\Lambda}_i^{*\prime} \hat{\boldsymbol{\rho}}$$

where $\boldsymbol{\Lambda}_i^*$ is the vector of the i^{th} row of $\boldsymbol{\Lambda}^*$, i.e.

$$\boldsymbol{\Lambda}^* = \begin{bmatrix} \boldsymbol{\Lambda}_1^{*\prime} \\ \vdots \\ \boldsymbol{\Lambda}_{n+C}^{*\prime} \end{bmatrix} \tag{23}$$

Thus, $\boldsymbol{\Lambda}_i^* = \boldsymbol{\Sigma}_{\hat{\boldsymbol{\rho}}\hat{\boldsymbol{\rho}}}^{-1} \boldsymbol{\sigma}_{\rho_i \hat{\boldsymbol{\rho}}}$, where $\boldsymbol{\sigma}_{\rho_i \hat{\boldsymbol{\rho}}}$ is the vector of the covariance between θ_i and $\hat{\boldsymbol{\rho}}$.

As with standard shrinkage, the variance of the covariance-shrunk bureaucrat fixed effects also understates the true variance, though by less:

$$\begin{aligned}
\mathsf{Var}_i(\hat{\theta}_i^{\text{CovSh}}) &= \mathsf{E}_i\left(\mathsf{Var}(\hat{\theta}_i^{\text{CovSh}})\right) + \mathsf{Var}_i\left(\mathsf{E}(\hat{\theta}_i^{\text{CovSh}})\right) \\
&= \mathsf{E}_i\left(\mathsf{Var}(\hat{\theta}_i^{\text{CovSh}})\right) + 0 \\
&= \mathsf{E}_i\left(\mathsf{Var}(\boldsymbol{\Lambda}_i^{*\prime}\hat{\boldsymbol{\rho}})\right) \\
&= \mathsf{E}_i\left(\mathsf{E}\left[(\boldsymbol{\Lambda}_i^{*\prime}\hat{\boldsymbol{\rho}} - \mathsf{E}(\boldsymbol{\Lambda}_i^{*\prime}\hat{\boldsymbol{\rho}}))^2\right]\right) \\
&= \mathsf{E}_i\left(\mathsf{E}\left[(\boldsymbol{\Lambda}_i^{*\prime}\hat{\boldsymbol{\rho}})^2\right]\right) \\
&= \mathsf{E}_i\left(\boldsymbol{\Lambda}_i^{*\prime}\mathsf{E}(\hat{\boldsymbol{\rho}}\hat{\boldsymbol{\rho}}')\boldsymbol{\Lambda}_i^*\right) \\
&= \mathsf{E}_i\left(\boldsymbol{\Lambda}_i^{*\prime}\boldsymbol{\Sigma}_{\hat{\boldsymbol{\rho}}\hat{\boldsymbol{\rho}}}\boldsymbol{\Lambda}_i^*\right) \\
&= \mathsf{E}_i\left(\left(\boldsymbol{\Sigma}_{\hat{\boldsymbol{\rho}}\hat{\boldsymbol{\rho}}}^{-1}\boldsymbol{\sigma}_{\theta_i\hat{\boldsymbol{\rho}}}\right)'\boldsymbol{\Sigma}_{\hat{\boldsymbol{\rho}}\hat{\boldsymbol{\rho}}}\boldsymbol{\Lambda}_i^*\right) \\
&= \mathsf{E}_i\left(\boldsymbol{\sigma}'_{\theta_i\hat{\boldsymbol{\rho}}}\boldsymbol{\Lambda}_i^*\right) \\
&= \mathsf{E}_i\left(\boldsymbol{\Lambda}_i^{*\prime}\boldsymbol{\sigma}_{\theta_i\hat{\boldsymbol{\rho}}}\right)
\end{aligned} \tag{24}$$

Denote the minimized mean-squared-error of bureaucrat i as $\text{MSE}_i^{\text{CovSh}*}$. Since mean-squared-error is always positive,

$$\begin{aligned}
\text{MSE}_i^{\text{CovSh}*} &= \mathsf{E}\left[(\theta_i - \boldsymbol{\Lambda}_i^{*\prime}\hat{\boldsymbol{\rho}})^2\right] \\
&= \mathsf{E}(\theta_i^2) - 2\boldsymbol{\Lambda}_i^{*\prime}\mathsf{E}(\theta_i\hat{\boldsymbol{\rho}}) + \mathsf{E}\left[(\boldsymbol{\Lambda}_i^{*\prime}\hat{\boldsymbol{\rho}})^2\right] \\
&= \sigma_\theta^2 - 2\boldsymbol{\Lambda}_i^{*\prime}\boldsymbol{\sigma}_{\theta_i\hat{\boldsymbol{\rho}}} + \boldsymbol{\Lambda}_i^{*\prime}\boldsymbol{\sigma}_{\theta_i\hat{\boldsymbol{\rho}}} \\
&= \sigma_\theta^2 - \boldsymbol{\Lambda}_i^{*\prime}\boldsymbol{\sigma}_{\theta_i\hat{\boldsymbol{\rho}}} > 0
\end{aligned} \tag{25}$$

$$\Rightarrow \boldsymbol{\Lambda}_i^{*\prime}\boldsymbol{\sigma}_{\theta_i\hat{\boldsymbol{\rho}}} < \sigma_\theta^2 \quad \forall i = 1, ..., n$$

Thus, $\mathsf{Var}_i(\hat{\theta}_i^{\text{CovSh}}) = \mathsf{E}_i(\boldsymbol{\Lambda}_i^{*\prime}\boldsymbol{\sigma}_{\theta_i\hat{\boldsymbol{\rho}}}) < \sigma_\theta^2$.

We also know that $\text{MSE}_i^{\text{CovSh}*} < \text{MSE}_i^{\text{Sh}*}$, because $\lambda_i\hat{\theta}_i \subseteq \boldsymbol{\Lambda}_i\hat{\boldsymbol{\rho}}$, i.e. minimizing bureaucrat i's mean-squared-error by weighting $\hat{\boldsymbol{\rho}}$ would do better than minimizing

it by weighting $\hat{\theta}_i$ alone. Then,

$$\begin{aligned} & 0 < \text{MSE}_i^{\text{CovSh*}} = \sigma_\theta^2 - \mathsf{Var}(\hat{\theta}_i^{\text{CovSh}}) < \text{MSE}_i^{*} = \sigma_\theta^2 - \mathsf{Var}(\hat{\theta}_i^{\text{Sh}}) \\ \Rightarrow\ & \mathsf{Var}(\hat{\theta}_i^{\text{Sh}}) < \mathsf{Var}(\hat{\theta}_i^{\text{CovSh}}) < \sigma_\theta^2 \\ \Rightarrow\ & \mathsf{E}_i\left[\mathsf{Var}(\hat{\theta}_i^{\text{Sh}})\right] < \mathsf{E}_i\left[\mathsf{Var}(\hat{\theta}_i^{\text{CovSh}})\right] < \sigma_\theta^2 \\ \Rightarrow\ & \mathsf{Var}_i(\hat{\theta}_i^{\text{Sh}}) < \mathsf{Var}_i(\hat{\theta}_i^{\text{CovSh}}) < \sigma_\theta^2 \end{aligned} \tag{26}$$

Therefore, the variance of covariance-shrunk bureaucrat fixed effects underestimates the true dispersion of bureaucrat ability, but by less than the variance of the (standard) shrunk ones.

Again, to compute $\hat{\theta}_i^{\text{CovSh}}$ in practice, we do not know the true variances and covariances σ_θ^2, σ_γ^2, $\sigma_{\theta\gamma}$, $\sigma_{v_i}^2$, $\sigma_{u_j}^2$, and $\sigma_{v_i u_j}$, so we replace them with the sample analogs found in Appendix section E.1.

E.4 A 2×2 Example of the Covariance Shrinkage Weights

For illustrative purposes, I derive the covariance-shrinkage factor for the simple case with 1 bureaucrat and 1 country: $\boldsymbol{\rho} = (\theta_1, \gamma_1)'$

$$\boldsymbol{\Sigma}_{\hat{\boldsymbol{\rho}}\boldsymbol{\rho}} = \begin{bmatrix} \sigma_\theta^2 & \sigma_{\theta\gamma} \\ \sigma_{\theta\gamma} & \sigma_\gamma^2 \end{bmatrix} \tag{27}$$

$$\boldsymbol{\Sigma}_{\hat{\boldsymbol{\rho}}\hat{\boldsymbol{\rho}}} = \begin{bmatrix} \sigma_\theta^2 + \sigma_{v_1}^2 & \sigma_{\theta\gamma} + \sigma_{v_1 u_1} \\ \sigma_{\theta\gamma} + \sigma_{v_1 u_1} & \sigma_\gamma^2 + \sigma_{u_1}^2 \end{bmatrix} \tag{28}$$

$$\begin{aligned} \boldsymbol{\Lambda}^* &= \boldsymbol{\Sigma}_{\hat{\boldsymbol{\rho}}\boldsymbol{\rho}} \boldsymbol{\Sigma}_{\hat{\boldsymbol{\rho}}\hat{\boldsymbol{\rho}}}^{-1} \\ &= \frac{1}{(\sigma_\theta^2 + \sigma_{v_1}^2)(\sigma_\gamma^2 + \sigma_{u_1}^2) - (\sigma_{\theta\gamma} + \sigma_{v_1 u_1})^2} \begin{bmatrix} \sigma_\theta^2 & \sigma_{\theta\gamma} \\ \sigma_{\theta\gamma} & \sigma_\gamma^2 \end{bmatrix} \begin{bmatrix} \sigma_\gamma^2 + \sigma_{u_1}^2 & -(\sigma_{\theta\gamma} + \sigma_{v_1 u_1}) \\ -(\sigma_{\theta\gamma} + \sigma_{v_1 u_1}) & \sigma_\theta^2 + \sigma_{v_1}^2 \end{bmatrix} \end{aligned}$$

$$\begin{pmatrix} \hat{\theta}_1^{\text{CovSh}} \\ \hat{\gamma}_1^{\text{CovSh}} \end{pmatrix} = \boldsymbol{\Lambda}^* \begin{pmatrix} \hat{\theta}_1 \\ \hat{\gamma}_1 \end{pmatrix} \tag{29}$$

Focusing on bureaucrat 1's fixed effect,

$$\hat{\theta}_1^{\text{CovSh}} = \boldsymbol{\Lambda}_{11}^* \hat{\theta}_1 + \boldsymbol{\Lambda}_{12}^* \hat{\gamma}_1 \tag{30}$$

where

$$\boldsymbol{\Lambda}_{11}^* = \frac{\sigma_\theta^2(\sigma_\gamma^2 + \sigma_{u_1}^2) - \sigma_{\theta\gamma}(\sigma_{\theta\gamma} + \sigma_{v_1 u_1})}{(\sigma_\theta^2 + \sigma_{v_1}^2)(\sigma_\gamma^2 + \sigma_{u_1}^2) - (\sigma_{\theta\gamma} + \sigma_{v_1 u_1})^2}, \quad \boldsymbol{\Lambda}_{12}^* = \frac{\sigma_{\theta\gamma}\,\sigma_{v_1}^2 - \sigma_\theta^2\,\sigma_{v_1 u_1}}{(\sigma_\theta^2 + \sigma_{v_1}^2)(\sigma_\gamma^2 + \sigma_{u_1}^2) - (\sigma_{\theta\gamma} + \sigma_{v_1 u_1})^2}$$

Equipped with the precise formula of the covariance shrinkage weights ($\boldsymbol{\Lambda}_{11}^*$ and $\boldsymbol{\Lambda}_{12}^*$), we can derive some comparative statics or compare them to the shrinkage factor (λ_1^*) to understand the covariance shrinkage method better. In general, covariance shrinkage improves on shrinkage by additionally utilizing the information about θ_1

contained in $\hat{\gamma}_1$, while taking into account the noisiness of $\hat{\gamma}_1$ and the strength of the correlation.

- If $\sigma_{\theta\gamma} = \sigma_{v_1 u_1} = 0$ ($\hat{\gamma}_1$ contains no information about θ_1), then $\hat{\theta}_1^{\text{CovSh}} = \hat{\theta}_1^{\text{Sh}}$.
- $\frac{\partial \mathbf{\Lambda}_{11}^*}{\partial \sigma_{v_1}^2} < 0$: Similarly to shrinkage, $\hat{\theta}_1$ is shrunk more if it is estimated with greater noise (higher $\sigma_{v_1}^2$).
- $\frac{\partial \mathbf{\Lambda}_{12}^*}{\partial \sigma_{u_1}^2} < 0$: Greater noise in estimating γ_1 (higher $\sigma_{u_1}^2$) reduces the covariance shrinkage weight on $\hat{\gamma}_1$.
- It is likely that $\mathbf{\Lambda}_{12}^* > 0$. Because the denominator is positive, the sign of $\mathbf{\Lambda}_{12}^*$ follows the sign of $(\sigma_{\theta\gamma}\,\sigma_{v_1}^2 - \sigma_\theta^2\,\sigma_{v_1 u_1})$. Moreover, limited mobility bias implies $\sigma_{v_1 u_1} < 0$ (Andrews et al., 2008).
  - If $\sigma_{\theta\gamma} > 0$, $\mathbf{\Lambda}_{12}^*$ is clearly positive.
  - Even if $\sigma_{\theta\gamma} < 0$, $\mathbf{\Lambda}_{12}^*$ is still positive under the likely scenario that $\hat{\theta}_1$ is more informative about θ_1 than $\hat{\gamma}_1$ is. The specific condition is that the signal-to-noise ratio (SNR) of $\hat{\theta}_1$ is larger than the SNR of $\mathsf{Cov}(\hat{\theta}_1, \hat{\gamma}_1)$, i.e. $\frac{\sigma_\theta^2}{\sigma_{v_1}^2} > \frac{|\sigma_{\theta\gamma}|}{|\sigma_{v_1 u_1}|} = \frac{\sigma_{\theta\gamma}}{\sigma_{v_1 u_1}}$. In that case, $\mathbf{\Lambda}_{12}^* > 0$ and $\mathbf{\Lambda}_{11}^* > \lambda_1^*$.[82] Intuition: Standard shrinkage reduces mean-squared-error by shrinking the noisy estimator $\hat{\theta}_1$ towards its mean value, $E(\theta_1) = 0$. Less shrinkage is needed when the SNR of $\hat{\theta}_1$ is high ($\hat{\theta}_1$ carries reliable information about θ_1). With covariance shrinkage, the SNR of the covariance also comes into play. $\mathbf{\Lambda}_{11}^* > \lambda_1^*$ reflects that $\hat{\theta}_1$ has higher SNR than the covariance, i.e. $\hat{\theta}_1$ is more informative about the value of θ_1 than $\hat{\gamma}_1$ is. At the same time, a positive weight on $\hat{\gamma}_1$ ($\mathbf{\Lambda}_{12}^* > 0$) counters $\mathbf{\Lambda}_{11}^* \hat{\theta}_1$ diverging away from 0 by more than $\lambda_1^* \hat{\theta}_1$, because $\sigma_{\theta\gamma} < 0$.
  - If $\sigma_{\theta\gamma} < 0$ and the SNR of $\hat{\theta}_1$ is *smaller* than the SNR of $\mathsf{Cov}(\hat{\theta}_1, \hat{\gamma}_1)$, i.e. $\frac{\sigma_\theta^2}{\sigma_{v_1}^2} < \frac{|\sigma_{\theta\gamma}|}{|\sigma_{v_1 u_1}|} = \frac{\sigma_{\theta\gamma}}{\sigma_{v_1 u_1}}$, then $\mathbf{\Lambda}_{12}^* < 0$ and $\mathbf{\Lambda}_{11}^* < \lambda_1^*$.
- If $\mathbf{\Lambda}_{12}^* > 0$, then $\frac{\partial \mathbf{\Lambda}_{12}^*}{\partial \sigma_{v_1 u_1}} < 0$: The positive weight on $\hat{\gamma}_1$ is higher the more closely connected the bureaucrat and country are (the stronger the negative covariance between the sampling errors of $\hat{\theta}_1$ and $\hat{\gamma}_1$ is).

[82] $(\mathbf{\Lambda}_{11}^* - \lambda_1^*) \propto (\sigma_{\theta\gamma} + \sigma_{v_1 u_1})(\sigma_{\theta\gamma}\,\sigma_{v_1}^2 - \sigma_\theta^2\,\sigma_{v_1 u_1})$.

E.5 Potential Pitfalls of Covariance Shrinkage

In general, the covariance shrinkage estimator improves upon the noisy raw fixed effect estimators by minimizing mean-squared-error. Yet, the covariance shrinkage method comes with two caveats.

1. The covariance-shrunk fixed effects are weighted averages of all raw fixed effects. Hence, it is possible for some of them to blow up – contrary to the idea of "shrinkage" toward the mean value 0 – if they non-trivially weigh a particularly large (positive or negative) raw fixed effect. The issue is compounded if they happen to non-trivially weigh *multiple* raw fixed effects that are large and of the same sign. Given that covariance shrinkage weights tend to be larger on closely connected nodes in the bureaucrat-country graph, this may occur, for instance, for a bureaucrat who got assigned to multiple large export destination countries by chance.

2. If $\mathbf{\Sigma}_{\hat{\boldsymbol{\rho}}\hat{\boldsymbol{\rho}}}$ is near-singular, often the case with large covariance matrices, then $\mathbf{\Sigma}_{\hat{\boldsymbol{\rho}}\hat{\boldsymbol{\rho}}}^{-1}$ can contain some extreme values. Then, some covariance-shrunk fixed effects might explode due to these extremely large covariance shrinkage weights, even if the raw fixed effects they are weighting are small in size.

Both caveats imply that covariance shrinkage may produce a few extreme values. In that case, the variance of the covariance-shrunk fixed effects may not reliably measure the true dispersion of bureaucrat or country fixed effects. However, other characteristics of the distribution that are less sensitive to outliers, such as the median or the quartiles, would still remain informative.

E.6 Comparison with Best et al. (2023)

We discovered a minor error in Best et al. (2023)'s derivation of the covariance shrinkage matrix. Specifically, the error is in the covariance matrix between $\hat{\boldsymbol{\rho}}$ and $\boldsymbol{\rho}$. In their version,

$$\mathbf{\Sigma}_{\hat{\boldsymbol{\rho}}\boldsymbol{\rho}}^{\text{BHS}} = \mathsf{diag}(\sigma_\theta^2 \mathbf{1}_n, \sigma_\gamma^2 \mathbf{1}_C),$$

so the covariance terms ($\sigma_{\theta\gamma}$) were omitted.

To gauge the impact of the error, we investigate the difference in the covariance shrinkage weights in the Best et al. (2023) version and the correctly derived version in Appendix Table E.4.

In Panel A, we list the top-10 covariance shrinkage weights in each version for $\hat{\theta}_1^{\text{CovSh}}$, the covariance shrunk fixed effect of bureaucrat 1 (`B1`; arbitrarily selected). Bureaucrat and country identifiers are assigned based on alphabetical order (e.g., `C1`:

Afghanistan). In the correct version, the largest weight is put on the fixed effect of bureaucrat 1 himself (B1), followed by countries he was appointed to (C36 and C55 – the shortest-path distance between B1 and C36 or C55 is 1 in the country-bureaucrat graph). The fourth largest weight goes to B4, who was also appointed to C36 – the shortest-path distance between B4 and B1 is 2. More generally, more closely connected nodes in the graph are assigned larger weights, reflecting their stronger correlations with $\hat{\theta}_1$. In the Best et al. (2023) version, however, the ordering of the weights and the shortest-path distances are not as well-aligned.

In Panel B, we report the summary statistics on the covariance shrinkage weights for all bureaucrat and country fixed effects. The summary statistics are more sensible in the correct version than in the Best et al. (2023) version, again highlighting that more closely connected nodes are assigned larger weights: 1) the share of bureaucrats and countries whose top covariance shrinkage weight is on themselves is higher at 0.970; 2) the average share of matched pairs that get the next-largest weights[83] is higher at 0.782; 3) the average weight on non-matched pairs[84] is lower and closer to zero, at 0.0004 on average, implying fewer instances of non-matched pairs getting sizable weights.

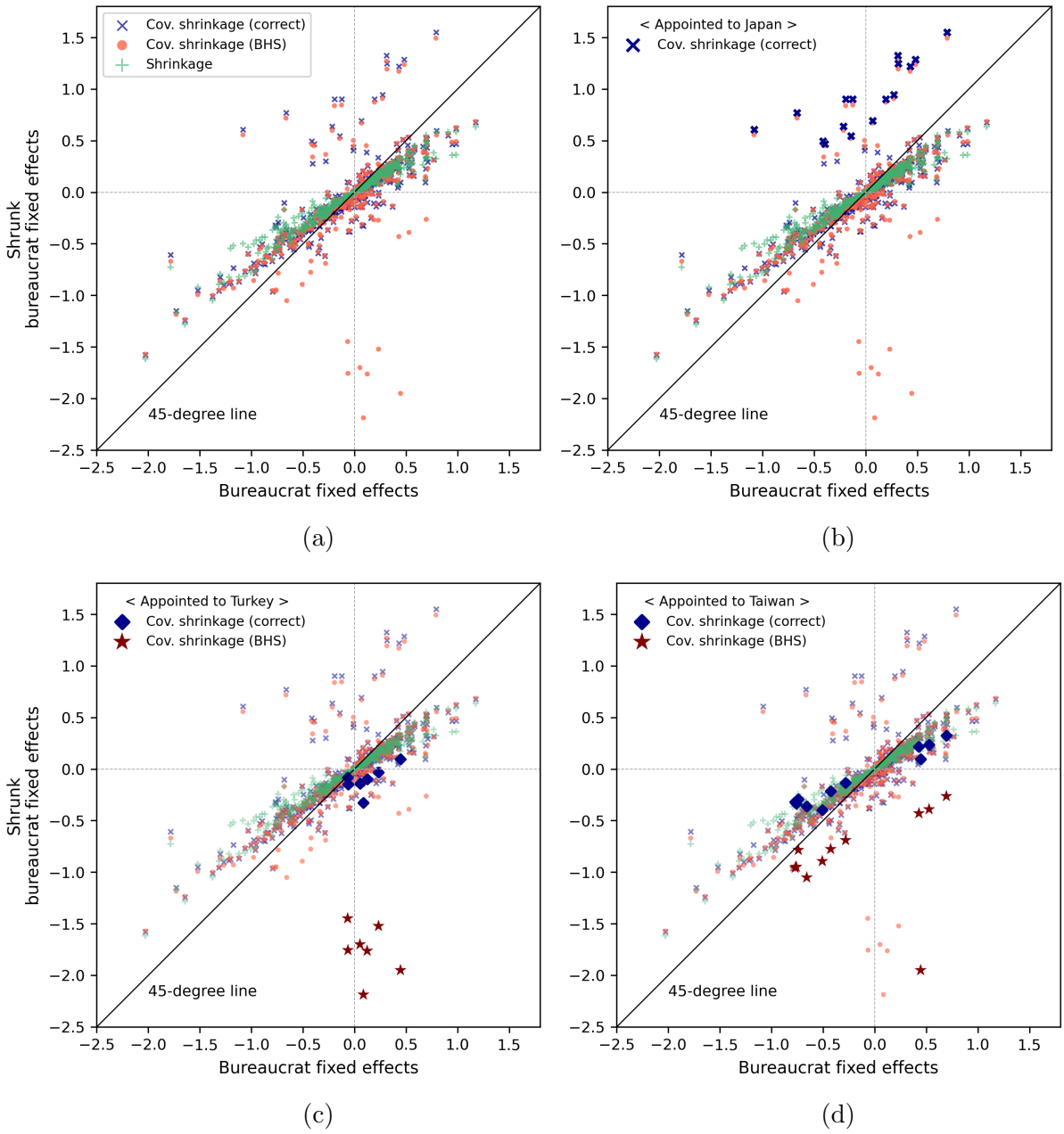
Appendix figure E.24 visually compares the two versions of covariance shrinkage to each other, as well as to standard shrinkage. Shrinkage always compresses the bureaucrat fixed effects toward zero, such that all points lie between the 45-degree line and the horizontal axis. Both covariance shrinkage versions mostly compress the bureaucrat fixed effects, too. Even for the fixed effects that get expanded away from zero (lying between the 45-degree line and the vertical axis) or switch in sign (lying in the second and fourth quadrants), most remain clustered around zero. However, covariance shrinkage produces a small number of extreme values, related to caveats under Appendix section E.5. Panel (b) shows that the covariance-shrunk (either version) fixed effects of bureaucrats appointed to Japan, one of Korea's largest trade partners, blow up because they absorb Japan's large fixed effect. Panels (c) and (d) show that the two versions of covariance shrinkage occasionally diverge. The Best et al. (2023) version produces extreme values for bureaucrats appointed to Turkey and Taiwan, the two countries that increase the condition number of $\boldsymbol{\Sigma}_{\hat{\boldsymbol{\rho}}\hat{\boldsymbol{\rho}}}$ the most.[85]

[83]The calculation of the share of matched pairs getting the next-largest weights is illustrated through the following example. For B1 with two matched countries (C36, C55), the next-2 largest weights after the top weight go to both C36 and C55 in the correct version (share=1), but they go to only C36 in the Best et al. (2023) version (share=0.5). The statistic in the table is the average of these shares for all bureaucrats and countries.

[84]Non-matched pairs refer to all bureaucrats and countries besides oneself and the countries or bureaucrats that they are directly matched to. E.g. for B1, they are all bureaucrats and countries besides himself, C36, and C55.

[85]For covariance matrices, a high condition number often indicates near-singularity, which can

Figure E.24: Comparison of Different Shrinkage Methods



Notes: This figure shows the scatter plots of the raw bureaucrat fixed effects (horizontal axis) against the fixed effects shrunk using correctly-derived covariance shrinkage, the Best, Hjort, and Szakonyi (2023) version of covariance shrinkage, or standard shrinkage (vertical axis). Panel (a) is the benchmark figure, while panels (b), (c), and (d) each highlight the bureaucrats appointed to Japan, Turkey, and Taiwan, respectively. Panel (b) shows that covariance shrinkage (both versions) sometimes produces extreme values. Panels (c) and (d) show that the two versions of covariance shrinkage occasionally diverge.

Table E.4: Comparison of covariance shrinkage formulae

Panel A: Top-10 covariance shrinkage weights for shrinking $\hat{\theta}_1$

Correct version			**Best et al. (2023) version**		
i	Cov. shrinkage weight on i	Shortest-path distance to B1	i	Cov. shrinkage weight on i	Shortest-path distance to B1
B1	0.754	0	B1	0.755	0
C36	0.052	1	C36	0.055	1
C55	0.043	1	C80	0.051	5
B4	0.019	2	C55	0.045	1
B49	0.019	2	B203	0.044	4
B187	0.019	2	B83	0.042	4
B397	0.017	2	B251	0.036	6
B145	0.017	2	B179	0.034	6
B38	0.017	2	B91	0.033	6
B87	0.017	2	B16	0.032	6

Panel B: Summary statistics on covariance shrinkage weights

	Average across all bureaucrats and countries	
	Correct version	**Best et al. (2023) version**
$\mathbb{1}$(Top weight is on oneself)	0.970	0.945
Share of matched pairs getting largest weights	0.782	0.683
Weight on oneself	0.686	0.674
Average weight on matched pairs	0.074	0.085
Average weight on others	0.0004	0.0027

Notes: The table compares the covariance shrinkage weights in two versions: 1) when they are derived correctly ($\mathbf{\Lambda}^* = \mathbf{\Sigma}_{\hat{\boldsymbol{\rho}}\boldsymbol{\rho}} \left(\mathbf{\Sigma}_{\hat{\boldsymbol{\rho}}\hat{\boldsymbol{\rho}}} + \mathbf{\Sigma}\right)^{-1}$, where $\mathbf{\Sigma} = \left[\mathsf{Cov}_k(\hat{\rho}_i^k, \hat{\rho}_j^k)\right]_{ij}$ is the covariance matrix of the bootstrap estimates), and 2) when they follow Best et al. (2023) ($\mathbf{\Lambda}^* = \mathsf{diag}(\sigma_\theta^2 \mathbf{1}_n, \sigma_\gamma^2 \mathbf{1}_C) \left(\mathsf{diag}(\sigma_\theta^2 \mathbf{1}_n, \sigma_\gamma^2 \mathbf{1}_C) + \mathbf{\Sigma}\right)^{-1}$). In Panel A, we list the top-10 covariance shrinkage weights in each version for $\hat{\theta}_1^{\text{CovSh}}$, the covariance shrunk fixed effect of bureaucrat 1 (B1; arbitrarily selected). Bureaucrat and country identifiers are assigned based on alphabetical order (e.g., C1: Afghanistan). In Panel B, we report the summary statistics on the covariance shrinkage weights for all bureaucrat and country fixed effects. Both panels show that closely connected nodes are more consistently assigned larger weights in the correct version than in the Best et al. (2023) version. See the text description for details.

F Robustness of Appointments by Country Importance

This section conducts robustness checks on the finding in Appendix figure A.14, that bureaucrats are appointed to the least important countries during their first

lead to extreme or unreliable results in matrix inversion. We compare the condition number of $\mathbf{\Sigma}_{\hat{\boldsymbol{\rho}}\hat{\boldsymbol{\rho}}}$ and $\mathbf{\Sigma}_{\hat{\boldsymbol{\rho}}\hat{\boldsymbol{\rho}}}^{\text{BHS}}$ as we remove one country from the sample at a time. The condition number falls the most dramatically when Turkey is removed, followed by Taiwan, for both matrices, but by more for $\mathbf{\Sigma}_{\hat{\boldsymbol{\rho}}\hat{\boldsymbol{\rho}}}^{\text{BHS}}$. Attesting to the importance of connection closeness for the covariance shrinkage weights, these two countries are connected through one common bureaucrat.

appointments, the most important countries during their third appointments, with second appointments forming an intermediate case. This main message is equally striking when reporting these distributions differently. Panels (c)-(f) restrict attention to bureaucrats whose first appointment started no earlier than 1981. This is to avoid that some results are due to a mechanical association between an office's total number of appointments and the duration of its existence. Panels (e) and (f) report the share of appointments to a given opening year (rank). This clearly shows the much higher likelihood of *late* offices to be managed by bureaucrats in their first appointment – around 50% of appointments to these offices – compared to *early* offices – around 25% of appointments to these offices. For bureaucrats in their third appointment, the numbers are similarly stark. Only around 15% of appointments to *late* offices are of bureaucrats in their third appointment, while the share for *early* offices is around 50%.

Appendix figure A.15 displays similar results when measuring a country's importance as a market for South Korean exports by its fixed effect based on equation (2). The density of bureaucrats' first appointments is higher in low fixed effect countries than for third appointments. The opposite holds for bureaucrats' third appointments, with 2nd appointments forming an intermediate case. Panels (e) and (f) again provide the starkest contrast. Among countries with a negative fixed effect, 40% of appointments are bureaucrats' first appointments, 20% of appointments are bureaucrats' third appointments. This reduces to 10-20% of first appointments for the 15 countries with the highest fixed effects. For third appointments, the share among the top 15 countries is much higher: 40-70%.

Appendix figures A.16 and A.17 highlight how bureaucrats move from their first appointment to second and third appointments, or to exiting KOTRA. First, around half of bureaucrats exit after their first appointment. The exit rate appears unrelated to the tercile of the office of the bureaucrat's first appointment. This holds when classifying into terciles both by opening year as well as country effects. Second, between appointments one and two, bureaucrats move between all terciles of countries – despite some persistence. Third, between appointments two and three, few bureaucrats stay in the least important countries: almost all bureaucrats leave the third tercile of openings (latest openings) and the first tercile of country effects (lowest fixed effect). The opposite is not true: bureaucrats largely stay in the most important countries – the first tercile of openings and the third tercile of country effects.[86]

It should be noted that more important countries may also be more desirable for bureaucrats. So such patterns of appointments are also broadly in line with an alter-

[86]From appointments one to two, there is already somewhat limited mobility from the first to third tercile of openings. This is not as striking between terciles of country effects.

native mechanism where progressively better postings are used as career incentives.

Overall, there is a clear pattern of bureaucrats being moved towards more important countries as their careers progress.

G Robustness of Office Opening Results

G.1 Assessing Longer Pre-Trends by Restricting the Sample

This section investigates concerns that the results in 9 may be influenced by the choice of excluding events occurring before 1967. Appendix figure A.18 reports estimates when expanding attention to events that happen from 1966 (from 1964) on. Panel (c) points to parallel trends between periods -4 and -2 with a moderate uptick in period -1. Panel (d) more convincingly finds the same convincing parallel trends reported in figure 9. The uptick in period -1 is discussed further below when we check the sensitivity of our estimates to parallel trend violations following Rambachan and Roth (2023).

G.2 Not-Yet-Treated Control Group

This section allays concerns that the estimated effect of opening an office is driven by the choice of the never-treated as the control group. Appendix figure A.19 uses a not-yet-treated control group instead of the never-treated used by the main estimation strategy. These figures report estimates following the approach proposed by Callaway and Sant'Anna (2021), which allows for consistent estimates in cases where the two-way fixed effects approaches with a simple treatment indicator fail. The estimates in appendix figure A.19 are of very similar magnitude and precision to the main estimation strategy. However panel (a.i) finds negative coefficients that are statistically significant, albeit small, in periods -4 to -2. This leads us to investigate the sensitivity of the estimates to violations of the parallel trends assumption. We do so for the main estimate using the not-yet-treated control, reported in panel (a.i), and an estimation (panel (a.ii)) that treats period -1 as the first treated period – i.e. allowing for one period of anticipation. The latter is reported in panel (b.i).

Panels (a.ii) and (b.ii) show that the estimates of the effect on exports ten years after an office opening remain statistically significant when allowing for parallel trends violations up to one time (1.5 times with one period of anticipation) the largest pre-treatment violation of parallel trends. Panels (a.iii) and (b.iii) show that the estimates remain significant when allowing for slope changes of 0.15% ($\approx 0.4\%$) between consecutive periods (Rambachan and Roth, 2023).

One period of anticipation would suggest that KOTRA has an effect on exports in the year before opening an office. A statistically significant negative effect in period -2 may point to violations of the parallel trends assumption. On the one hand, the measured opening year is meant to capture the year when the office becomes operational, i.e. the first year in which it can affect exports. If the office can have no effect

in year -1, the jump in exports in this year could be explained by KOTRA choosing to open offices in the year when a country becomes a more important destination market. This would constitute a violation of the parallel trends assumption that would upwardly bias the estimated treatment effect. Panels (a.ii) and (a.iii) report the sensitivity of the estimated parallel trends assumption when concluding that the jumps in year -1 cannot be a causal effect of the office openings. On the other hand, setting up an office already requires resources dedicated to a country that may have a direct or indirect effect on exports. A direct effect would occur if in year -1 the KOTRA bureaucrats setting up the office already engage in KOTRA's usual activities. This appears plausible if testing different strategies to promote exports is an important component of the activities of setting up an office. Appendix figure A.21 shows that this is not the case regarding reports – which do not go up prior to an office opening — though it may be the case regarding obtaining inquiries from potential importers. An indirect effect may occur in year -1 if the (planned) presence of a KOTRA office partly functions as a signal that is interpreted as indicating export potential by potential Korean exporters. An effect due to coordination would be interesting, as an important role of industrial policy is to coordinate industrial activity, often justified by making reference to potential positive externalities.

G.3 Extensive Margin

This section investigates whether the results in figure 9 are artifacts of transforming the raw export values using the inverse hyperbolic sine. To do so, it investigates the product-level extensive margin of exports. Concretely,were-estimate equation (7) with y_{cpt} changed to a dummy indicating whether there are positive exports from South Korea of product p to country c in year t. The question under investigation becomes: Does a KOTRA office in a country increase the likelihood of positive exports of a particular product from South Korea to the country? Or alternatively: Does a KOTRA office in a country raise the share of products that Korea exports to that country?

Appendix figure A.20 reports the estimated effects of office openings on the extensive margin. It indicates a 5 percentage point increase in the likelihood of a product being exported to a destination country 10 years after an office opening. While the magnitude is not directly comparable, the trajectory of the point estimates in panel (a) is very similar to the main results reported in figure 9. Again, the pre-treatment coefficient is very close to zero, corroborating that the parallel pre-trends in figure 9 are not due to the inverse hyperbolic sine somehow obscuring differential pre-trends. On the other hand, there appears to be a treatment effect due to the office opening: the effect on the outcome variable slopes upwards starting with the office opening.

The estimated coefficients become economically sizable as early as one year after the opening and statistically significant at the 5%-level two years after the opening. As before, the coefficients stabilize after around ten years.

The results remain qualitatively similar when restricting attention to openings between 1967 and 1981, which allows for estimating coefficients in the five years prior to the opening (panel (b)).

Panels (c) and (d) replicate these results using the not-yet-treated control group. Panel (c) does so while assuming 0 periods of anticipation. While the estimated coefficients in the post-period are very similar to the ones in panels (a) and (b), panel (c) casts some doubts on the parallel trends assumption: the estimated coefficients for 2, 3, and 4 years prior to the opening are all negative and statistically significant. Panel (d) shows that this anticipation effect seems to mainly occur in the year prior to the office opening. Hence, the discussion about the timing of KOTRA's effect from section G.2 applies here.