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Abstract

Does bureaucratic capacity matter for growth miracles? This paper investigates how much the effect of an industrial policy during South Korea's growth miracle depends on bureaucratic capacity. We find that the bureaucrats implementing the policy greatly change its effect on exports – the variable targeted by the policy and key to South Korea's economic success. These bureaucrats manage offices that promote exports on appointments to 87 countries between 1965, when South Korea was one of the world's poorest countries, and 2000. We exploit the three-yearly rotation of managers between countries to show that increasing bureaucrat ability by one standard deviation causes a 37% increase in exports. This effect is comparable to the policy's average effect – estimated from office openings. Hence, this industrial policy entirely depends on bureaucratic capacity: It has no effect when implemented by a bureaucrat one standard deviation below average. We find evidence for a key mechanism via which better bureaucrats increase exports: transmitting information about market conditions. Under better bureaucrats South Korean exports increase more strongly with a country's import demand – taking advantage of this demand. Finally, we investigate whether bureaucrat experience increases South Korean exports. We isolate quasi-random variation in experience: a product's import demand growth during the bureaucrat's first appointment. Such experience increases exports in subsequent appointments of this bureaucrat. This highlights that organizational capacity grows endogenously, implying a novel channel for path dependence in organizational capacity.

JEL classification: O11, O12, D73, L52, F13

Key Words: Economic Development, Bureaucracy, Industrial Policy, Managers, Government, Political Economy, Export Promotion, Trade Policy

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1 Introduction

State and bureaucratic capacity are strongly associated with economic development (Besley, Burgess, Khan, and Xu (2022) – BBKX). Less is known about *how* bureaucratic capacity causes economic growth. Explanations of East Asia's growth miracles suggest one channel: bureaucracies are central to industrial policy success (Juhász, Lane, and Rodrik (2023) – JLR)[1]. Understanding to what extent the effect of industrial policy depends on bureaucratic capacity is crucial in determining what lessons low- and middle-income countries can draw from development success stories such as South Korea.

In this paper, we make two contributions. First, we provide evidence that the effect of an industrial policy on economic growth and development crucially depends on bureaucratic capacity. Second, we show that learning-by-doing can build 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 outcome of interest and the policy's effect. 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. For state-of-the-art 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 is satisfied if each bureaucrat only works on this policy, and the policy's outcome is measurable in each location – ideally, this outcome is closely linked to economic growth.

[1]Qualitative political economy of the rapid economic growth in East Asia emphasize the positive role of industrial policy and the development of state capacity for carrying out complex policies, in particular in South Korea (Johnson, 1982; Amsden, 1989; Wade, 1990; Evans, 1995; Woo-Cumings, 1999). At the presence of market failures, such as production externalities, agglomeration failures, and public provision of production inputs, the state needs to intervene for firm growth by enacting industrial policy (JLR).

To satisfy these conditions we pick an appropriate context: South Korean overseas export promotion. First, this 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. This large connected set is due to the frequent movement of bureaucrats relative to the period over which we track appointments (1965-2000). Fourth, in each country the policy has a sole target: exports to that country, an important development outcome.[2] This setting also is 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 and South Korea's growth in exports is a particularly remarkable phenomenon to be explained. Qualitative political economy accounts link this growth to intervention by a capable state. This paper provides a quantification of such accounts, thus shedding light on the lessons today's low- and middle-income countries can draw from South Korea's development success.

We first find that the policy had a substantial effect on average – motivating the study of differences between bureaucrats. We use the offices' staggered roll-out to estimate the effect of opening an office.[3] Exports increase by 38% in the ten years after an office opening. The number of products with positive exports increases by 5 percentage points. We are able to rule out the two most plausible alternative interpretations for the results: (1) Demand: Import demand in a country does not increase after an office opens. (2) Strategic timing of openings to coincide with counterfactual increases in exports: The scope for strategic timing

[2]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 (2021) For support that demand-side factors may be decisive in economic development, see Goldberg and Reed (2020). 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.

[3]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 due to the combination of dynamic treatment effects with a 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).

is very limited as pre-determined gravity variables – distance and market size – largely explain the year in which a country's office opens: For European countries, i.e. mostly holding constant distance, the correlation between pre-determined market size and year of office opening is 0.87. This appears sensible for an organization that aims to increase South Korean exports. It is hard to imagine time–varying factors that trump the static differences in export potential between countries – e.g., between the UK and Denmark.

Second, we show that the effect of this 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, Creecy, and Kramarz, 2002) 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 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 a *limited mobility bias* in plug-in variance decompositions (Kline, Saggio, and Sølvsten, 2020).

Estimating these large differences in bureaucrat ability relies on the bureaucrat fixed effects being estimated without bias. This requires the key assumption that bureaucrat ability is uncorrelated with underlying trends in the outcome variable.[4] We alleviate concerns regarding this assumption by combining information about the appointment process with numerous diagnostic checks (Card, Cardoso, and Kline, 2016). First, the three-yearly rotation means bureaucrat appointments cannot be timed perfectly. Hence, there would be differential pre-trends if good bureaucrats were appointed because of underlying trends. Second, the three-yearly rotation means losing a bureaucrat is determined three years prior – at the time of the bureaucrat's appointment. Losing a bureaucrat is, thus, more convincingly exogenous to export trends than gaining a bureaucrat. Hence, if our estimated bureaucrat effects were biased because of strategic appointments of good bureaucrats to countries with high export trends, an event-study should find much larger effects to gaining (compared to

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

losing) a bureaucrat. Our diagnostic checks alleviate these concerns: We find parallel pre-trends and symmetric effects of gaining and losing a bureaucrat. Further diagnostic checks alleviate other potential concerns.

The large differences between office managers are explained by one key mechanism: Better bureaucrats cause offices to more effectively achieve the goal of connecting export supply and import demand. We show that, upon the appointment of a high–ability bureaucrat, exports of products go up much more strongly when this product's import demand (export supply) increases simultaneously.

The large differences between office managers are taken into account by the organization when managing its human resources. First, bureaucrats are much less likely to be re-appointed if exports underperform during their first appointment. Second, first (later) appointments as country manager are to less (more) important countries. Hence, the most important offices are rarely run by underperforming bureaucrats.[5] [6] [7]

Third, we provide evidence that bureaucrat experience increases Korean exports. This points to learning-by-doing as a channel to build bureaucratic capacity. But it also highlights a novel channel for path dependence in this capacity. We isolate quasi-random variation in bureaucrat experience: a 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. In isolating this quasi-random component of experience, we alleviate the main endogeneity concerns in correlations of bureaucrat experience and exports. Similar to differences in bureaucrat ability, experience in a product causes exports to increase more strongly when import demand (export supply) go up.

This paper contributes to understanding the bureaucratic determinants of economic

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

[6] This suggests the organization may largely use offices in less important countries to experiment with inexperienced bureaucrats. Only bureaucrats who prove themselves during their first appointments go on to later appointments in important countries.

[7] As long as the ranking of countries' importance is time-invariant such an appointment process satisfies the identifying assumptions for bureaucrat fixed effects. Further, these patterns of appointments suggest a strategy for organizations to manage their human resources when it is hard to predict employee abilities based on observable characteristics. Teachers, and managers more broadly, form other examples where such a strategy may apply because observable characteristics insufficiently explain the substantial variation in their performance.

growth (BBKX). First, it provides evidence that bureaucrats matter for South Korean exports – whose rapid growth was key to the country's growth miracle. Second, it finds that an industrial policy heavily depends on the bureaucrats implementing it, providing evidence for the oft-hypothesized but under-researched link between state capacity and industrial policy. Overall, this paper provides novel evidence that bureaucratic capacity is important in drawing lessons from episodes of rapid growth and policy success, such as the East Asian miracles.

We build on the literature on bureaucratic determinants of economic growth (BBKX) 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 lower down in 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] This paper is able to quantify bureaucrats' role in industrial policy success during a growth miracle by combining advantages of the two approaches. It studies bureaucrats who manage offices which each target the same outcome variable: South Korean exports to the respective location. Further, the responsibility of these offices is defined at a geographic level – each office is evaluated based on reaching export targets to the country where it is located.

Second, we build on research that providing estimates of the causal effects of a given industrial policy (Juhász, Lane, and Rodrik, 2023; Juhász, 2018; Liu, 2019; Lane, 2024;

[8] To which the empirical tools from the "credibility revolution" (Angrist and Pischke, 2010) are particularly appropriate. 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 BBKX, 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)". 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 2020). 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.

Choi and Shim, 2022; Choi and Levchenko, 2021). The policy under study provides lessons for industrial policy more broadly as export promotion (1) is a key component of many industrial policies (Juhász, Lane, Oehlsen, and Pérez, 2022), (2) shares characteristics with many other components of industrial policies, (3) can itself be thought of as an industrial policy in the sense of promoting economic activity "X but not Y" (Juhász, Lane, and Rodrik, 2023).[10] This paper differs from the above-mentioned research in seeking to understand the variation in an industrial policy that is due to bureaucratic capacity, concretely, the people implementing the policy, providing evidence for the oft-hypothesized but under-researched link between state capacity and industrial policy.

Third, the paper builds to 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).[11] Fenizia (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).[12] This paper differs from these efforts in studying managers implementing an economic policy. By showing that managers gain capacity via learning-by-doing, this paper sheds light on a previously understudied mechanism via which managers determine state and organizational capacity.

Fourth, we build on research on trade and economic development, in particular relating to the effect of (export) demand shocks on firms (Atkin, Khandelwal, and Osman, 2017;

[10] Clearly, in the case of export promotion, X is much less narrowly defined than a policy promoting the "battery" sector. However, the benefits from export promotion are clearly geared towards certain sectors – most obviously tradable sectors more so than non-tradables. The example of South Korean export promotion is helpful in highlighting further sectoral implications: In 1965, overseas export promotion could not benefit the South Korean car industry because South Korea did not produce any cars that could be exported. Instead, a main beneficiary at that time was the textile industry which had many growing firms intent on reaching new export markets. In the 1970s, heavy and chemical industries could benefit most from export promotion as they were growing fast – partly due to the government's HCI drive. At this time, the textile industry was more mature and may have benefited less from export promotion (Volpe Martincus and Carballo, 2008).

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

[12] 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).

Alfaro-Ureña, Manelici, and Vasquez, 2022).[13] Goldberg and Reed (2020) complement this by showing that demand-side constraints that may hamper development in many countries.[14] Similar to industrial policy, existing research on export promotion has focused on estimating average effects (Munch and Schaur, 2018; Hayakawa, Lee, and Park, 2014; Bagir, 2020). Other research investigates which firms or sectors benefit from concrete policy tools (Volpe Martincus and Carballo, 2008, 2010, 2012). The latter agenda includes research analyzing which market failures are addressed by the same policy we study (Kim and Kim, 2024). This paper differs from and contributes to this literature by focusing on the differences in effects on exports that are due to bureaucratic capacity.

The rest of the paper proceeds as follows. Sections 2 describes the institutional background. Section 3 introduces the data. Section 4 discusses the effect of office openings. Section 5 shows how much industrial policy depends on individual bureaucrats. Section 6 focuses on experience as one factor determining differential effectiveness between bureaucrats. Section 7 concludes.

2 Institutional Background

Our study period commences at a time when South Korea was one of the world's poorest countries. During our period of study, Korea's real GDP per capita increased from $1,304 (1961) to $25,421 (2001).[15] In 1961, the average income in South Korea was below most countries in Sub-Saharan Africa.[16] In 2001, South Korea's average income was above Portugal's. It is often argued that this growth is due to a well-functioning, activist state that conducted successful industrial policies.[17] On the other hand, the Korean state was described

[13] The larger literature is reviewed by Atkin and Donaldson (2022), Atkin et al. (2022).

[14] Kim and Kim (2024) suggest that information provision by KOTRA – the organization we study – directly alleviates the search frictions faced by Korean exporters.

[15] Both in 2017 USD. Relative to the U.S., this corresponds to an increase from 1/15 of real GDP per capita in 1961 to 1/2 in 2001. Data from Penn World Tables.

[16] The countries with higher GDP per capita in 1961 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, Cape Verde, the Seychelles.

[17] Wade (1990) and Cheng et al. (1998) as cited by BBKX; Amsden (1989); Juhász, Lane, and Rodrik (2023). See also the well-known popular book by Studwell (2013).

as aid-dependent and corrupt until at least the mid-1960s (Kim and Vogel, 2011).[18] This makes Korea an interesting case for understanding the role of state capacity in economic development broadly and growth miracles specifically.

South Korea's growth was particularly stark in exports – the outcome variable directly targeted by the policy under study. Figure 1 displays Korea's growth of exports per capita between 1952 and 2001. 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. Figure 1 also suggests that this export growth was not due to global convergence, as other developing countries did not see growth relative to the U.S.[19]

This paper sheds light on one policy targeting this transformative growth in exports that is central to narratives of Korea's broader economic success. Export promotion as a prominent area of state activism is highlighted by a representative survey of Korean manufacturers in 1976. These manufacturers reported "foreign marketing" as the policy area where government intervention most markedly improved under President Park Chung-hee (1961-1979), compared to President Syngman Rhee (1948-1960) (Jones and Il, 1980).

2.1 South Korean Bureaucratic Capacity

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 starting in 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

[18] 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).

[19] 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.

and adaptation in South Korea's industrial policies, "only possible to governments possessing a well-trained bureaucracy" (p. xxxi, foreword by Edward S. Mason).[20]

However, drawing a causal connection from South Korean bureaucratic capacity to subsequent economic growth is complicated by historical research describing 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).[21] The limited capacity of the state is further underlined by Kim and Baik (2011): "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."

It should, be noted that these vastly different perspectives may, at least partly, be due to reporting biases or reverse causality. However, it is also possible that a change in South Korea's bureaucratic capacity around 1960 was an important cause for South Korea's subsequent economic growth. The U.S. ambassador indeed described the "breathless" speed with which reforms were implemented 1961-1963. Nevertheless, policy-making remained deeply dysfunctional.[22] While many reforms were successfully implemented, others had deeply disruptive effects on the economy and were reverted as soon as this became politically feasible. There was "no blueprint [...] with clear objectives and well-defined steps to harness the state apparatus for political stability and economic growth" (Kim, 2011).

Overall, it is plausible – but far from conclusive – that a rise in state capacity causally

[20]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.

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

[22]Policy-making during this period also relied heavily on the Korean Central Intelligence Agency – an agency whose primary goals and expertise did not concern economic welfare, but instead regarded military intelligence as well as protecting the regime from domestic protest movements.

lead to part of the subsequent growth miracle. While bureaucratic capacity may be central to South Korea's growth miracle, it is hard to causally establish this link. This provides a further motivation for this paper's goal of quantifying the importance of bureaucratic capacity for the effect of one important policy pursued by the South Korean government.

2.2 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 sales promotion and research, a campaign of public relations and advertising, [and] information service to exporters and importers" (Udell, 1965). Figure 2 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.[23]

While many government policies aimed to increase South Korean 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 orthogonal to most other industrial policies by the South Korean government, which may have 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. First, KOTRA's "Investigation/Research" division investigated factors related to export supply and demand: (1) South Korea's capability to supply a product for exports and (2) the import demand in the 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. They received export inquiries from domestic companies and import inquiries from foreign ones. The latter were then publicized 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

[23]Figure 3 also includes Kazakhstan, where the opening occured in 2001.

the organization of a South Korean pavilion at international trade fairs, which were viewed as a means to produce great export results within short periods of time by allowing exporters to engage in direct conversations with local buyers. To assist with this, the overseas offices coordinate logistics. They also carried out strategic tasks such as recruiting, selecting, and briefing exporters representing their products at the fairs. At the same time, they disseminate information about these exporters and their products to attract potential buyers to the South Korean pavilion or individual firms. The bureaucrats did this by running ads, sending letters and making phone calls to promising exporters and foreign buyers, and reaching out to trade associations. The domestic companies were selected to be producers of goods with newly trending styles and designs that matched the marketability of the venues of the fairs.

Each of these three functions correspond to data on KOTRA office activity described in section 3.3. The data include market reports investigating export capability and import demand, importer requests, and sales and attendance of firms at KOTRA-organized trade fair pavilions.

Compared to other bureaucracies, KOTRA's overseas offices have a large degree of discretion regarding how to carry out the task of promoting exports. For this reason, this paper's results focus on KOTRA's ultimate outcome of interest: exports. Clearly, it is 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 via attending 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. So rather than having a centrally mandated list of tasks to fulfill,[24] KOTRA office directors have more in common 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), as cited by Xu (2018)). However, compared to these bureaucrats responsible for a multitude of policies and outcomes, KOTRA bureaucrats are implementing exactly one policy with one rather narrowly defined target that can largely be summarized into the measure of exports during their ap-

[24]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).

pointments. The primary performance measure, as assessed by the head office, is whether export targets are met. This makes studying KOTRA bureaucrats much less susceptible to the multi-tasking problem faced when evaluating the effectiveness of most bureaucrats with regional responsibilities. Moreover, the outcome targeted by overseas offices, exports to the country, is an outcome of direct importance for economic growth and development.

2.3 KOTRA: Assignment to 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 available at the country level.[25]

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 deemed 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.1 plots the distribution of appointment durations. Between appointments, managers return to South Korea, typically 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.2 plots the distribution of gaps between appointments. This rotation limits discretion in appointments as not all bureaucrats are available when a particular

[25]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.

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 IP

One reason for studying export promotion is the narrative of South Korea's development as being export-driven, as well as export promotion's 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..

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 (2024), 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.

Appendix figure A.3 displays how the targeting of KOTRA's activity changed over time. Before the HCI drive, only 15-25% of product-specific reports discuss HCI products. During the HCI drive, this share increases rapidly, reaching close to half of all reports in the late 1970s. After the HCI drive, the share of reports targeting these sectors remains relatively constant. This supports the view that export promotion was used as part of Korea's overall industrial policy.

At the same time, it is worth noting that national sectoral industrial policies, such as the HCI drive, target particular sectors. This paper studies a policy that differentially affects destination markets. Hence, it can be thought of as largely orthogonal to most other industrial policies – especially after controlling for product-year trends.

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 relevant source regarding bureaucrat appointments are contemporaneous reports on appointments to KOTRA's overseas offices in major South Korean newspaper. 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 was not reported by either 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 corroborated 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 further 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*.[26] However, this is remedied by a great diversity in first names.[27] 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 corresponded to distinct bureaucrats. 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 wrongly 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. (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".

In addition to these export data at the country-product-year level, we obtained and digitized firm-level export data for the years 1968 to 1977 from KOTRA's archival publications. These data contain observations at the firm-country-product-year level.

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

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

3.3 Bureaucrat Output

We complement the data on exports with measures of concrete bureaucrat activity digitized from KOTRA documents.

First, we extract data on KOTRA's activity as a provider of "information service" such as market reports and transmission of importer requests to potential importers. We extract the market reports and importer requests from around 7,936 daily publications covering almost every weekday from 1965 to 2001. Of the 80,000 market reports, we are able to link 45,000 to both a 2-digit product and a country. The remaining reports are either not product-specific or do not discuss specific countries. Of the 200,000 inquiries, we are able to link 170,000 to both a 4-digit product, a country, and a specific office.

Second, we observe attendance and sales during trade fairs where a Korean representation was organized by KOTRA. This data covers 893 trade fairs attended by KOTRA between 1969 and 1997, including 192 events where KOTRA's responsible for a fair changes from one year to the next. On average, the Korean representation was composed of 2-3 KOTRA bureaucrats, usually headed by the local office director, and 15 Korean exporting firms. Overall, the data contains 34,000 encounters between a KOTRA bureaucrat and a Korean firm, i.e., bureaucrat and firm attend the same trade fair. Our data hence allows us to observe firms' fair attendance often including their sales deals at the fair, as well as certain firm characteristics, at least the firm's history in attending other KOTRA facilitated fairs and the bureaucrats the firm encountered at those fairs.

4 The Effect of Office Opening on Exports

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 5 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.[28] 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 2 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.[29] The empirical analysis in this section will focus on the initial office openings (1962–1981). Figure 3 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.

4.1 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.

$$y_{cpt} = \lambda_{pt} + \gamma_{cp} + X^T_{cpt} + \sum_{k \neq -1} \theta_k D^k_{ct} + \epsilon_{cpt} \tag{1}$$

As a first step, we estimate the specification given by equation (1). λ_{pt} indicates product-year fixed effects, γ_{cp} indicates country fixed effects that may differ at the product-level. D^k_{ct} are dummies equal to 1 if year t is k years after the first office opened in country c. θ_k

[28] 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).

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

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 (1) 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.[30] Alleviating concerns about parallel trends violations, there is little indication of differential pre-trends (discussed in section 4.2) and no "effect" on non-South Korean exports to a destination (discussed in section **??**). 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

[30] 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).

(1978) dip – would lead to overestimates. We find no indication for this.[31]

4.1.1 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 (1), 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.[32] Sections 4.2 and **??** 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

[31] 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 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 **??**.

[32] 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.

receive an office, we include in the sample all observations between 1962 and 1992. In estimating equation (1), this sample serves to estimate λ_{pt}. [33].

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, section **??** 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.[34]

4.2 Results: Effect of Office Opening on Exports

Figure 4 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.4 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 4 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%[35] higher relative to the control group.

[33] 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

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

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

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.

4.3 Robustness

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

Appendix C 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.

4.3.1 No Increase in Export 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 (1). The coefficients from this regression are reported in figure 5.[36]. 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

[36] As well as appendix figure A.4, panels (e) and (f).

regarding this placebo outcome.

Second, we re-estimate equation (1) while controlling for non-South Korean exports to a country (also transformed using the inverse hyperbolic sine). Appendix figure A.4 (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.[37] Panel (b) shows that estimates are largely unchanged when allowing the effect of non-South Korean exports to differ by year.

4.3.2 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.[38]

Within Europe, distance from South Korea does not vary much between countries[39], so the main predictor for office openings from a gravity equation would be the size of each destination's market.[40] 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-determined from the perspective of the roll-out of KOTRA offices to Europe. If KOTRA's

[37] 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.

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

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

[40] 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.

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 6 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.

5 Bureaucrats and South 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 4. This implies that the industrial policy under study is ineffective if every bureaucrat's ability is reduced by one standard deviation.

5.1 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). This requires a two-step procedure: (1) obtaining unbiased estimates of bureaucrat fixed effects – the identification strategy to do so is described in this section, (2) using the estimated fixed effects to obtain measures of the variation in exports explained by bureaucrat abilities, correcting for the fact that raw

fixed effects contain measurement error – described in section 5.2.

$$y_{cpt} = \lambda_{pt} + \gamma_c + \theta_{b(c,t)} + \epsilon_{cpt} \tag{2}$$

We model the inverse hyperbolic sine of South Korean exports,[41] henceforth "exports", associated with country c, product p, year t, and the bureaucrat assigned to that country–year – $b(c,t)$. Exports are explained by the sum of a product–year component – λ_{pt} – , a bureaucrat component – $\theta_{b(c,t)}$ – , a country component – γ_c – , and an error term – ϵ_{cpt} .[42] As in other parts of the paper, we aim to explain exports at the product-level. This is in line with KOTRA's goal of reaching – usually product–specific – export targets. It further avoids that results for a country-year may be driven by a couple of dominant export products.

Equation 2 identifies the bureaucrat and country fixed effects only within the largest connected set.[43] It further 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 uncorrelated with 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. larger or richer countries, would not violate the identifying assumptions.

5.1.1 Descriptives: Connected Set & Leave-One-Out Connected Set

Table 1 describes the structure of the sample. The full sample contains 974 appointments of 475 bureaucrats to 138 offices. We restrict attention to the 87 main country offices in order to create a one-to-one mapping from KOTRA offices to export flows. The largest connected set among these contains all appointments to 86 out of 87 countries.[44] This connected set

[41] We explore robustness to the inverse hyperbolic sine transformation in appendix D.2. We find that bureaucrat fixed effects are predictive of changes in both the extensive and intensive margin.

[42] 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.

[43] Appendix E further illustrates the concept of a connected set and leave-one-out connected set in our setting.

[44] 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

contains 728 appointments of 398 managers of whom 194 saw appointments to multiple offices.[45]

The bureaucrat–country graph is interconnected enough such that 75 countries and 93% of appointments form part of a leave-one-out connected set. The reason behind this is that most country offices remains open for decades: Over this time they experience the appointment of many different office managers. Column (4) indicates that 72 offices have more than three distinct office managers, 61 offices have more than five managers, 49 offices even have more than seven distinct managers.

Our preferred estimation uses only the appointments in this largest leave-one-out connected set to obtain the raw fixed effects. This has two advantages. (1) It allows for explicitly correcting for the *limited mobility bias* that would result if one simply computed the variance of individual fixed effects Kline, Saggio, and Sølvsten (2020). (2) Kline, Saggio, and Sølvsten (2020) show that zooming in on the leave-one-out connected set directly reduces the *limited mobility bias* substantially.

5.1.2 Assumption: Bureaucrat Appointments Orthogonal to Export Trends

This section discusses how factors influencing bureaucrat appointments relate to the central assumption that bureaucrat appointments are quasi–random with respect to export trends.

The central factor generating movement of bureaucrats is their three-yearly rotation schedule. As highlighted in appendix figure A.1, 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 – at least sometimes – appointed a good bureaucrat to country c in year t because of increasing export trends. This would violate the identifying assumption. Now suppose country c is due to get a new bureaucrat in year $t+1$ – and not in year t. In this case, KOTRA may appoint a good bureaucrat to country c in year $t+1$. If this were the

because its office opening occurred shortly before the end of our sample period.

[45] 194 movers is slightly larger than the 184 movers in the balanced analysis sample of Fenizia (2022). Compared to Fenizia (2022), our power is enhanced because most of our countries and bureaucrats are part of the same connected set, even a leave-one-out connected set.

case, we should observe differential trends prior to the appointment of a good bureaucrat. Section 5.4 tests this hypothesis and finds no such differential pre-trends.

Second, KOTRA has some discretion – limited by factors discussed below – in deciding to appoint bureaucrat b – not b' – to country c in year t. Importantly, this decision also pre-determines that country c loses bureaucrat b – not b' – in year $t+3$. If bureaucrats were moved between countries – at least partly – due to underlying export trends, the greater discretion at the start of an appointment – compared to its end – would imply that the "effect" attributed to gaining bureaucrat b should exceed the "effect" attributed to losing b. Section 5.4 tests this hypothesis and finds that the effects of gaining and losing bureaucrat b are almost perfectly symmetric. This rules out a number of alternative hypotheses that would imply violations of the identifying assumption. Apart from strategic appointments due to export trends, this includes the "bureaucrat as coordination device" hypothesis: If the South Korean government decided to invest more resources into exporting to country c at the same time as bureaucrat b is appointed to country c, this would constitute a violation of the identifying assumption. However, the symmetric effects of gaining/losing a bureaucrat go against this unless the complimentary resources were withdrawn at the same time as a bureaucrat was moved away. As KOTRA does not have control over which bureaucrat moves away from c in $t+3$, it appears implausible that such symmetry would arise if KOTRA times the appointment of better bureaucrats with an increased investment into exporting to a particular country.

In qualitative interviews, KOTRA employees mention two further factors constraining the discretion in 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 largely homogeneous between bureaucrats, KOTRA's HR manages discontent by rotating bureaucrats between low- and high-desirability appointments. In most cases both a country's language and its income relative to other countries change little over time. So the above-mentioned factors suggest appointments may be correlated with country fixed effects.[46] These constraints on

[46] As discussed above such a correlation involving the time-invariant country effects would not constitute a violation of the identifying assumptions.

the appointment of bureaucrats make it harder to appoint bureaucrats because of anticipated export trends.

Lastly, despite the above-mentioned constraints, one may wonder why we do not find evidence that bureaucrats are strategically appointed to country-years with high import demand. One reason for this may be that time-invariant country characteristics are much more important than trends: The time-varying demand-shocks that make Portugal a more important export destination than the UK would have to be very large. This is in line with the roll-out of export promotion offices largely following pre-determined gravity variables as reported in figure 6.

5.1.3 Further Discussion of Regression Equation

Equation (2) implies the assumption that the inverse hyperbolic sine (ihs) of South Korean exports is linear in bureaucrat and country effects. Section 5.4 presents results in support of this ihs-linear specification. To better interpret the results based on the inverse hyperbolic sine, appendix D.2 shows how the fixed effects translate into extensive and intensive margin changes to exports.

5.2 Estimating the Variation Explained by Bureaucrats

This section explains how we decompose the variance in South Korean exports to estimate how much of it is explained by differences in bureaucrat ability.

Our preferred approach follows Kline, Saggio, and Sølvsten (2020) to obtain a variance decomposition that directly corrects for the *limited mobility bias* that arises in two-way fixed-effects specification when moves between different countries occur infrequently.

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

$$(\text{exports}|pt)_{cpt} = \text{exports}_{cpt} - \hat{\lambda}_{pt} = \theta_{b(c,t)} + \gamma_c + \epsilon_{cpt} \tag{4}$$

As variation in residualized exports within spells 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 after removing the effect of product-year dummies from the value of exports to obtain $(\text{exports}|pt)_{cpt}$ as described in equation (4) where $\hat{\lambda}_{pt}$ is estimated from equation (2).[47]

Our primary object of interest is the variation explained by the bureaucrats: $\text{Var}(\theta_{b(c,t)})$. The challenge in obtaining an estimate for $\text{Var}(\theta_{b(c,t)})$ is that this would be overstated by a naive estimator that simply calculates the (observation–weighted) variance in estimated bureaucrat fixed effects: $\text{Var}(\widehat{\theta}_{b(c,t)})$. Kline, Saggio, and Sølvsten (2020) derive the bias from this plug-in estimator under unrestricted heteroskedasticity ($bias_{KSS}$), building on previous approaches which required homoskedastic error terms (Andrews, Gill, Schank, and Upward, 2008).

This bias is a linear combination of each observation's variance weighted to account for the observation's influence on $\text{Var}(\widehat{\theta}_{b(c,t)})$.

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.[48] We report the variance decomposition according to equation (3).

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 shrinks the raw fixed effects by bootstrapping the estimation of equation (4) to distinguish the true, signal variance in bureaucrat effects and the variance of their sampling error (Chetty, Friedman, and Rockoff, 2014a; Best, Hjort, and Szakonyi, 2023). This has the advantage of yielding shrunk fixed

[47] In fact, the two-way fixed-effects estimation is performed on the data that is already collapsed at the spell level. The bureaucrat and country fixed effects estimated on this collapsed data are perfectly correlated with those that are estimated on 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 calculation of $(\text{exports}|pt)_{cpt}$ follows Chetty, Friedman, and Rockoff (2014b) who explain that to remove the effect of pt without biasing the bureaucrat effects θ and country effects γ, $\hat{\lambda}_{pt}$ 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. E.g., $\hat{\lambda}_{cars,1965}$ is very small compared to $\hat{\lambda}_{cars,1995}$. Table B.2 highlights the importance of these factors as year-product fixed effects explain 35.5% of the variation in exports.

[48] While 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, 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 Andrews, Gill, Schank, and Upward (2008) correction method delivers extremely similar results for either measure of connectedness.

effects for each bureaucrat, hence allowing us to compare different parts of the distribution, e.g. the 20th and 50th percentile. To obtain the bootstrapped samples our preferred approach draws appointments from the set of all appointments.[49]

5.3 Result: Bureaucrats Are Crucial to Policy Success

Table 2 reports the main results from the variance decomposition (equation (3)) after correcting for the *limited mobility bias* (Kline, Saggio, and Sølvsten, 2020). Figure 7 reports the cumulative distribution function of raw bureaucrat fixed effects obtained from estimating equation (3).[50]

Table 2 reports that bureaucrats explain a substantial amount of variation in Korean exports: One standard deviation of bureaucrat ability – their true fixed effect – is estimated to be 0.316,[51] implying a difference in exports of 37% (Column 1). Moreover, we can compare it to the policy's average effect of 0.321 (38%) estimated from the office openings.

This paper set out to answer what makes an industrial policy successful. The effect described above suggests that an important part of the answer is: the bureaucrats who implement it. The policy under study has no effect when implemented by a bureaucrat whose ability is one standard deviation below average.[52]

5.3.1 Effect Size Discussion

The differences in exports due to individual bureaucrats are large. Similar to the office opening effect, increasing ability by one standard deviation amounts to roughly the effect of counterfactually moving London as close to Seoul as Mumbai actually is.

However, columns (1)-(2) also highlight that the office managers explain about 1/7 as much variation as countries. This suggests that in explaining South Korea's exports, individuals are substantially less important than offices/countries. They are also less important

[49] Alternative approaches yield similar or less conservative shrinkage factors. These include (ii) drawing countries from the set of all countries, (iii) drawing years from the set of all years, (iv) drawing country-year-product observations from the set of all country-year-products observations.

[50] As explained above, the variance and standard deviation based on these would overestimate bureaucrat importance. The same holds true regarding the difference between percentiles p_1 and p_2.

[51] $0.316 = 0.100^{1/2}$

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

than managers in other recent studies who explain 1/3 as much variation in the processing of social insurance claims as the offices they manage (Fenizia, 2022) and 3/4 as much variation in mortality as the public hospitals they manage (Otero and Muñoz, 2022).[53]

Moreover, South Korean exports were growing at more than 35% annually[54] so the effect of increasing bureaucrat ability by one standard deviation is similar to average annual export growth.

Columns (1)-(2) find a negative correlation between bureaucrat and country fixed effects suggesting that better bureaucrats work in smaller countries. Overall, bureaucrat and country fixed effects explain 88% of the spell-level variation in exports (after subtracting time-trends).

5.3.2 Robustness: Placebo, Multi-Appointment Bureaucrats

Next, we perform a "placebo check" on the validity of the variance decomposition exercise. The fixed effects of these *placebo* bureaucrat should *not* have any explanatory power. Columns (5)-(6) show the results when bureaucrats are randomly shuffled to countries while preserving the number of different appointments for each bureaucrat. Both the variation in bureaucrat fixed effects, as well as the covariance between bureaucrat and country fixed effects, go to zero. This assuages concerns that the results in columns (1)-(2) are spurious. If they were spurious, we would expect columns (5)-(6) to resemble them.

To allay concerns that the fixed effects of single-appointment bureaucrats may suffer from aggravated overfitting[55] and therefore magnify the variation in bureaucrat fixed effects, we also report in columns (3)-(4) the variance decomposition results excluding them. The standard deviation in bureaucrat effects drops to around 0.237 – about 75% of the equivalent

[53] 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. Studying managers outside of the public sector, Metcalfe, Sollaci, and Syverson (2023) find that they explain 58% as much variation as store fixed effects in determining the sales of retail stores.

[54] Between 1962 and 1981, South Korean exports increased from 57 million to 21 billion U.S. Dollars, implying an annual growth rate of 36.5%.

[55] For a single-appointment bureaucrat, their fixed effect value equals the residualized export value to the country they were appointed to during their appointment. While this makes overfitting an obvious concern, it should also be noted that the Bonhomme, Holzheu, Lamadon, Manresa, Mogstad, and Setzler (2023) algorithm is designed to handle an abundance of individuals with one spell only in the sample.

for the whole sample. Section 5.6 suggests that the lowest ability bureaucrats are endogenously not re-appointed. Hence, the smaller (but comparable) variation in ability among re-appointed bureaucrats may not point to a bias but rather a novel fact: selection of bureaucrats reduces the variation in exports due to the remaining bureaucrats. That the variation in bureaucrat fixed effects in the placebo is no larger when including the single-appointment bureaucrats (columns (5)-(6)) than when excluding them (columns (7)-(8)) supports the reliability of the preferred decomposition results in columns (1)-(2).

Our alternative shrinkage approach estimates that the true difference in ability between bureaucrats at the 20th and 50th percentile is 0.324 (implying a 38% increase in exports).[56] As this difference is again similar to the effect of an office opening, an office causes an increase in exports only to the extent that its manager is better than the 20th percentile. Moving from the median bureaucrat to the 90th percentile has a similar effect to moving from the 20th percentile to the median.

5.4 Diagnostics

This section conducts a number of diagnostic checks to allay concerns about the validity of the fixed effect estimates according to equation (3). It starts by alleviating concerns that bureaucrat appointments are not orthogonal to export trends, i.e., the error term in equation (3). First, we investigate the plausibility of the assumption that bureaucrat appointments are orthogonal to underlying export trends. Second, we investigate the additive separability assumption built into equation (3). Finally, we further allay concerns that the bureaucrat fixed effects are driven by noise.[57]

5.4.1 Bureaucrat Appointments Orthogonal to Export Trends?

This section combines KOTRA's three-yearly appointment schedule with event-study estimations to allay concerns that the appointment of KOTRA bureaucrats may not be orthogonal

[56] We bootstrap to obtain the sample variance in each bureaucrat fixed effect. Our preferred approach bootstraps over appointments, as our data can be thought of as a random sample of all feasible combinations of bureaucrat–country matches. This shrinkage estimator also allows for estimating the standard deviation in bureaucrat ability: 0.383, about 20% greater than the estimate obtained following Kline et al. (2020)

[57] The placebos reported in columns (5)-(8) of table 2 also serve to highlight that bureaucrat fixed effects are not driven by noise.

to underlying export trends (see 5.1).

First, as explained in section 5.1, if KOTRA tried to appoint high ability bureaucrats because of increasing export trends, we would expect to observe differential trends prior to the appointment of good bureaucrats.

$$y_{ept} = \eta_{ep} + \lambda_{pt} + \sum_{k \neq -2} \Big(\alpha_k + \boldsymbol{\beta_k} \ \mathbf{1}\{\Delta\hat{\theta}_e \text{ in top tercile}\} + \boldsymbol{\delta_k} \ \mathbf{1}\{\Delta\hat{\theta}_e \text{ in middle tercile}\} \Big) \mathbf{1}\{t = T + k\} + \epsilon_{ept} \tag{5}$$

To test for such differential pre-trends we estimate equation (5), which explains exports as a function of the difference in bureaucrat ability due to the switch from the old to the new bureaucrat – an *event*. Following the literature (Fenizia, 2022; Otero and Muñoz, 2022), we divide the events into terciles depending on the change in bureaucrat fixed effects due to them. β_k (δ_k) is the effect in event time k of a change in the top (middle) tercile relative to one in the bottom tercile. e indicates the event. 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. Equation (5) obtains the event-study estimates while controlling for trends using product-year fixed effects – λ_{pt} – and for pre-event levels of exports using event-product fixed effects – η_{ep}. In obtaining the event study estimates, we normalize by the last full year in which the *old* bureaucrat was in charge: $T - 2$.

Figure 8 shows that top (middle) tercile transitions are not predicted by differential pre-trends compared to a bottom tercile transition. They do, however, imply a jump in exports by 30% (11%) upon the appointment of the new bureaucrat. In combination with the three-yearly rotation, the parallel pre-trends allays concerns about the orthogonality condition. If KOTRA was assigning bureaucrats because of trends, we would not expect to see such parallel pte-trends. Figures 9 and 11 provide further re-assurance that pre-trends are parallel. Both are discussed in detail below.

In addition to the above, the three-yearly rotation implies that the year of losing a high ability bureaucrat is largely pre-determined at the time of this bureaucrat's appointment. There is close to no discretion regarding the time when country c loses bureaucrat b. Conditional on the appointment starting in year t, it almost always ends in year $t + 3$. If

our estimated bureaucrats effects were biased because good bureaucrats were strategically sent to countries with high ϵ_{cpt}, an event-study should find much "larger" effects to gaining (compared to losing) bureaucrat b.

The same would be true if ϵ_{cpt} was high due to some action taken by KOTRA or another Korean government body. Suppose KOTRA always sends bureaucrat b to a country when it also increases the funding for export promotion to this country. This would also induce a jump in exports upon the appointment of bureaucrat b. However, it would be surprising if KOTRA timed the withdrawal of such funds to also exactly coincide with losing bureaucrat b – given that KOTRA has much less control over the time of losing bureaucrat b in year t, which is largely determined three years ahead of time.

$$y_{ept} = \eta_{ep} + \lambda_{pt} + \sum_{k \neq -2} \left(\alpha_k + \boldsymbol{\beta_k} \hat{\theta}_e^{\text{new}} + \boldsymbol{\delta_k} \hat{\theta}_e^{\text{old}} \right) \mathbf{1}\{t = T + k\} + \epsilon_{ept} \tag{6}$$

To test whether the effects of gaining and losing a bureaucrat are symmetric, we estimate equation (6), which explains exports as a time-varying function of the fixed effects of the new bureaucrat ($\hat{\theta}_e^{\text{new}} = \hat{\theta}_{b(c,T)}$) and the old bureaucrat ($\hat{\theta}_e^{\text{old}} = \hat{\theta}_{b(c,T-1)}$). Other than distinguishing between $\hat{\theta}_e^{\text{new}}$ and $\hat{\theta}_e^{\text{old}}$, this specification follows equation (5).

Figure 9 plots the event-study estimates ($\hat{\beta}_k$ and $\hat{\delta}_k$) obtained from equation (6). 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. As several concerns about the orthogonality condition would imply weaker effects when losing a bureaucrat, this symmetry alleviates such concerns.

Pre-trends 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 is only relative to South Korean exports to other countries. Given that South Korean exports were growing at more than 35% annually[58] and all the regression equations include product-year fixed effects, losing a good bureaucrat

[58] Between 1962 and 1981, South Korean exports increased from 57 million to 21 billion U.S. Dollars, implying an annual growth rate of 36.5%.

means exports drop *only relative to this trend*.

5.4.2 Out-of-Sample Predictiveness of Fixed Effects

This section assesses whether the fixed effects we estimate are also predictive out of sample. We find that this is the case, allaying concerns about overfitting.[59]

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. E.g., to estimate the fixed effects of bureaucrats appointed to the UK, we obtain their fixed effects when excluding the UK from the sample. This comes at a cost. For a bureaucrat with n appointments, the out-of-sample fixed effects are estimated on $n-1$ appointments. This means, only for about half of all bureaucrats are out-of-sample fixed effects defined – the other half are only ever the office managers in one country. A quarter of bureaucrats have a total of two appointments, meaning their out-of-sample fixed effects are estimated from only one appointment. Only the interconnectedness of our data makes it possible to estimate such out-of-sample fixed effects. When estimating fixed effects while 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.

First, figure 10 displays a binned scatterplot of residual exports and in-sample as well as out-of-sample fixed effects. By construction, the slope for the in-sample fixed effects equals 1. More interestingly, out-of-sample out-of-sample fixed effects explain exports with a coefficient of 0.52. This 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.

Second, appendix figure A.9 replicates figure 9 using out-of-sample, i.e. *other–country*, fixed effects. Upon a switch between bureaucrats new and old ability still statistically significantly predict exports in the expected way even when ability is estimated only using other countries.

Overall, this section provides support to the interpretation that bureaucrat fixed effects

[59]The limited additional explanatory power from allowing bureaucrat effects to differ between appointments also suggests this is not a first order concern.

identify the causal impact of an individual bureaucrat on exports. Given that estimated bureaucrat effects are predictive out-of-sample, it seems implausible that the fixed effects are driven by correlations between bureaucrat appointments and underlying export trends.

5.5 Mechanism: Good Bureaucrats Increase Exports When Import Demand Increases

This section investigates whether the increase in exports upon the appointment of a high ability bureaucrat is due to an increased elasticity to market conditions. We show that upon the switch to a more effective bureaucrat, South 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 most – but not all – of the effect of high ability bureaucrats comes from more effectively exploiting market conditions, e.g., by relaying information about destination market demand.

$$
\begin{aligned}
y_{ecpt} = \eta_{ep} + \lambda_{pt} + \psi_d^0 \text{demand}_{cpt} + \psi_s^0 \text{supply}_{cpt} + \psi_{d,new}^0 \text{demand}_{cpt} \times \hat{\theta}_e^{\text{new}} + \\
\psi_{s,new}^0 \text{supply}_{cpt} \times \hat{\theta}_e^{\text{new}} + \psi_{d,old}^0 \text{demand}_{cpt} \times \hat{\theta}_e^{\text{old}} + \psi_{s,old}^0 \text{supply}_{cpt} \times \hat{\theta}_e^{\text{old}} + \\
\sum_{k \neq -2} \Big[\alpha_k + \psi_{dk} \text{demand}_{cpt} + \psi_{sk} \text{supply}_{cpt} + \boldsymbol{\beta_k}\ \hat{\theta}_e^{\text{new}} + \boldsymbol{\delta_k}\ \hat{\theta}_e^{\text{old}} + \\
\boldsymbol{\beta_k^{demand}}\ \text{demand}_{cpt} \times \hat{\theta}_e^{\text{new}} + \boldsymbol{\beta_k^{supply}}\ \text{supply}_{cpt} \times \hat{\theta}_e^{\text{new}} + \\
\boldsymbol{\delta_k^{demand}}\ \text{demand}_{cpt} \times \hat{\theta}_e^{\text{old}} + \boldsymbol{\delta_k^{supply}}\ \text{supply}_{cpt} \times \hat{\theta}_e^{\text{old}} \Big] \mathbf{1}\{t = T + k\} + \epsilon_{ecpt}
\end{aligned}
\tag{7}
$$

We estimate equation (7), which explains changes in exports around a new appointment. This estimating equation includes all the components from equation (6). In addition, it includes main effects and interactions of "demand" and "supply" with the incoming and outgoing 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 effect of market conditions on South Korean exports 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 ability of the new or old bureaucrat. The new parameters of interest

are β_k^{demand} , β_k^{supply} , δ_k^{demand}, δ_k^{supply} which give the difference in elasticity of South Korean Exports with respect to market conditions due to the estimated ability of the new or old bureaucrat.

Figure 11 plots the estimates of β_k, β_k^{demand}, β_k^{supply}, δ_k, δ_k^{demand}, and δ_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 new bureaucrat's fixed effect and going against the old bureaucrat's fixed effect. The response of South Korean exports to market conditions increases by around 5 percentage points when the bureaucrat ability increases by one standard deviation. This implies an increase in the reaction of South Korean exports to market conditions by around 20% (from a base of around 25%).

Figure 11 also is informative about pre-trends. The absolute values in the pre-period are never statistically significant at the five percent level and much smaller in absolute values than the estimates in the post-period.

The point estimates for the effect of incoming and outgoing ability due to the change in bureaucrat mostly remain statistically significant. They are, however, reduced to about 1/10 of their size in figure 9, suggesting that much (but not all) of the effect of high ability bureaucrats is due to the increased elasticity of South Korean exports to market conditions, e.g. by relaying information about local conditions (demand) and identifying opportunities based on market developments common to South Korean exporters across destination markets (supply).

Figure 11 further assuages concerns regarding the orthogonality of bureaucrat appointments and export trends by showing that there are no differential pre-trends and symmetric effects due to gaining and losing a bureaucrat, corroborating the findings reported above (in section 5.4).

Overall, this section provides additional support that more effective bureaucrats causally impact exports. It does so by highlighting a mechanism via which this takes place: Switching to a more effective bureaucrat causes a sharp increase in the elasticity of South Korean exports to market conditions. Losing an effective bureaucrat causes a sharp decrease of similar magnitude.

5.6 Extension: Performance in 1st Office & Careers

This section finds that residualized exports during a bureaucrat's first appointment, part of their estimated fixed effects, are predictive of bureaucrats' careers. Figure 12 reports the probability density function of residualized exports, splitting the sample by the total number of appointments a bureaucrat has over their career. This distribution has a substantially fatter left tail for bureaucrats with only one career appointment. While not causal, this result suggests that bureaucrats' careers within KOTRA are a function of their fixed effects. One explanation for this is that KOTRA uses a metric correlated with our fixed effects in their decision to re-appoint bureaucrats. On the other hand, bureaucrat appointments are an equilibrium outcome giving rise to further explanations.

We next regress bureaucrats' number of appointments on residualized exports during their first appointment, part of a bureaucrat's fixed effect investigated in the preceding parts of section 5. By including fixed effects for the year of a bureaucrat's first appointment we rule out various omitted variables biases as explanations for the estimated effect, most prominently: (1) The number of appointments could depend mechanically on the time between a bureaucrat's first appointment and the end of our sample. (2) Bureaucrats could differ systematically by their first year of appointment. Including fixed effects for the year of first appointment, we find a positive significant effect of residualized exports during a bureaucrat's first appointment on number of appointments of 0.240 (standard error: 0.112). This effect is robust to alternative specifications. We find a positive significant effect of 0.430 (standard error: 0.109) when regressing on a dummy that indicates residualized exports above the 25th percentile.

Overall, we find that residualized exports during a bureaucrat's first appointment are associated with a greater number of subsequent appointments as manager of an overseas office. Allaying concerns that this may be due to differences in bureaucrat cohorts or bureaucrat tenure, this effect holds among bureaucrats whose first appointment began in the same year.

5.6.1 New Bureaucrats Appointed to Less Important Locations

This section provides descriptives about bureaucrat appointments that support the view that the principal appoints untested bureaucrats to less important countries. Over time, the low-ability bureaucrats are selected out. Hence, KOTRA offices in the most important countries are mostly led by high-ability bureaucrats.

Appendix figure A.12 shows the distribution of bureaucrats between offices in their first, second, and third appointment across offices with different opening years. The opening year proxies a country's importance to the extent that KOTRA first opens offices in more important countries – as highlighted by the rollout of offices to countries with larger import markets, displayed in figure 6. Panels (a) and (b) show that as a bureaucrat's career progresses, they are more likely to be appointed to important countries – with early office openings.[60] In a bureaucrat's first appointment, they are more likely to be appointed to countries whose office opened after 1970 (when offices had already opened in 35 countries – the more important ones). In a bureaucrat's third appointment, the opposite is true. The second appointment forms an intermediate case. Appendix F shows that this result is robust for a number of alternative measures of office importance.

6 The Effect of Bureaucrat Experience

Chapter 5 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

[60] Office openings come in waves. To ensure this does not lead to misleading conclusions, panels (b), (d), and (f) plot the probability density relative to the rank of a country's opening year.

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).[61] $T_1(b)$ and $C_1(b)$ indicate the year and country of bureaucrat b's first appointment.[62] 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

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

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

quite obviously endogenous. First, in light of chapter 5, 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.

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

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

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 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 semi-conductors 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 $\times$ year fixed effects – τ_{et}. As each event $\times$ 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 informa-

tive 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 13 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[63] – 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

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

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 13 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, 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.19 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.19 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 ßKorea. 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 14 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.20, our estimate of the main effect becomes very noisy in this specification.

In addition to exploring this mechanism, the results reported in figure 14 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 closely link individual bureaucrats to exports during the South Korean growth miracle. The findings have important implications for debates on the role of state capacity in development success stories and in industrial policy.

First, this paper provides the first quantification of 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.

Second, the findings imply that implementation matters substantially in determining whether an industrial policy is successful – at least for industrial 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. As we compare bureaucrats implementing the same policy, the results highlight one important determinant of industrial policy success: bureaucratic capacity.[64] This focus on the "how" of industrial policy implementation is especially pertinent as export promotion is a policy many governments choose to pursue, on its own or as part of a broader industrial policy.

Third, the setting under study is distinct from other settings in the literature on bureaucrats and economic development: South Korean export promotion uses bureaucratic capacity to support a country's firms in navigating global markets. This study thus 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. This may be representative for many settings where successful policy requires entrepreneurial bureaucrats, e.g., to identify and overcome frictions constraining firm growth (Mazzucato, 2013). While the findings can only indirectly speak to concrete measures to increase bureaucratic capacity, they are informative regarding the importance of bureaucratic capacity broadly.

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

[64] Juhász, Lane, and Rodrik (2023) call for research into the "how" because governments have been pursuing industrial policies unencumbered by academic economists' views regarding "whether governments should carry out industrial policy".

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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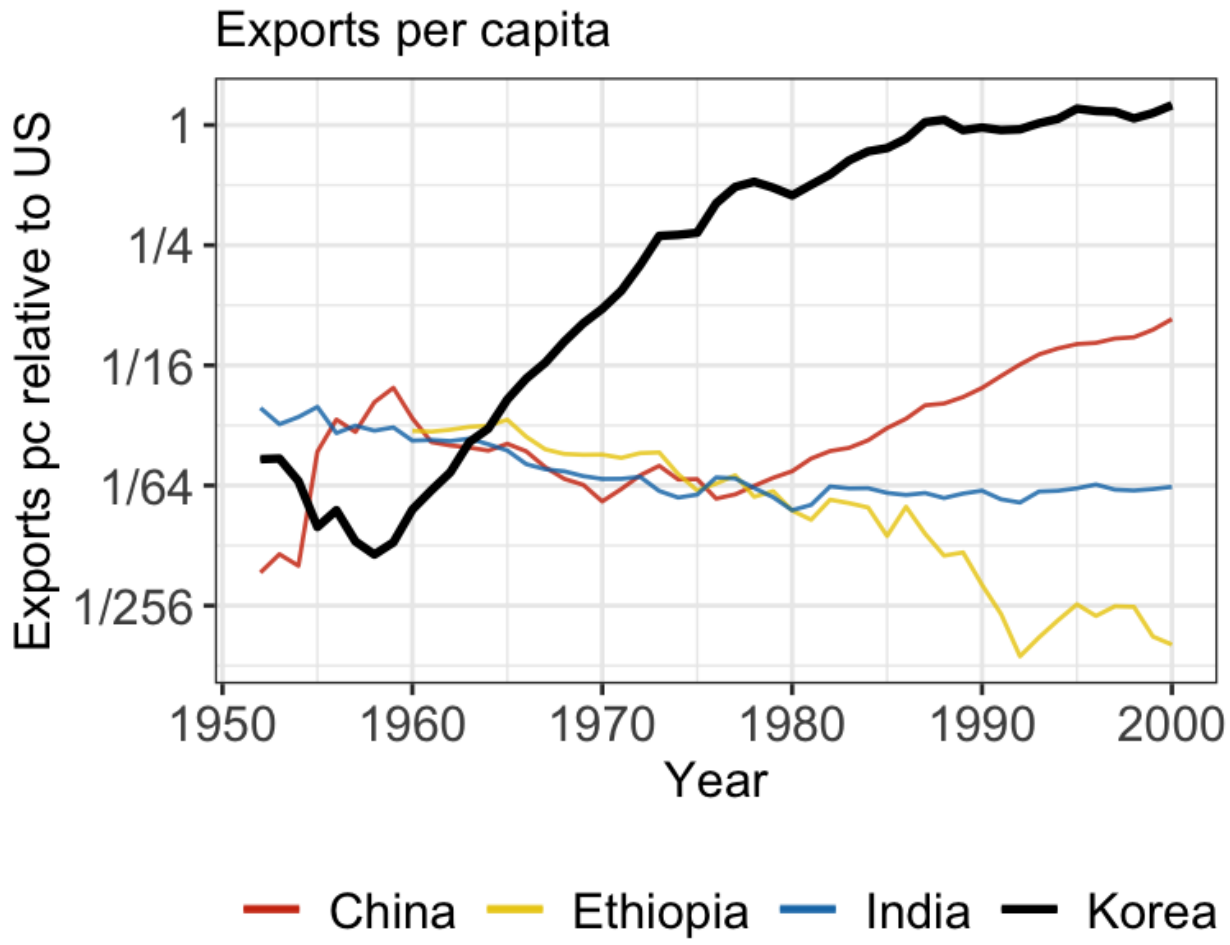
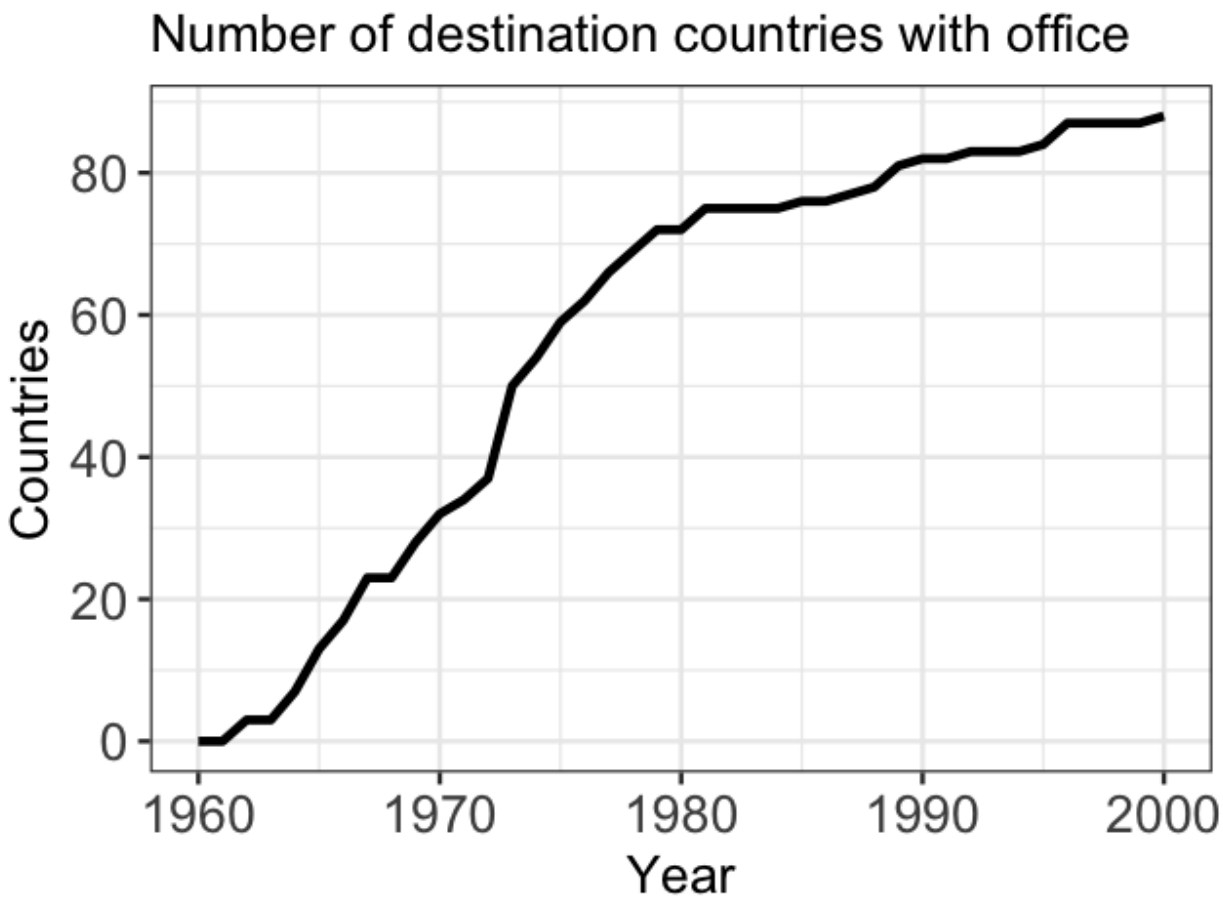
Figures and Tables

Figure 1: Growth in Korean Exports



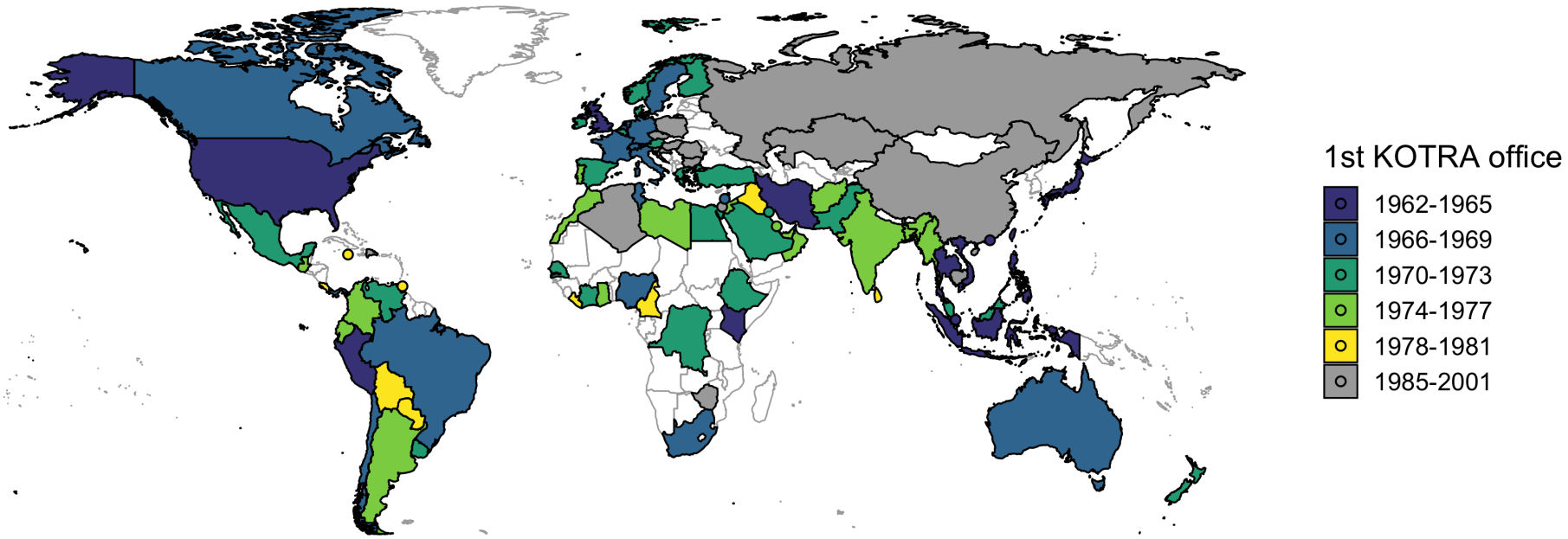
Notes: The figure displays Exports per capita relative to the U.S. the years 1952 to 2000 for Korea and a selected group of other countries. Data on exports and population obtained from International Monetary Fund (2023): Direction of Trade Statistics.

Figure 2: Growth in number of countries with export promotion (EP) offices



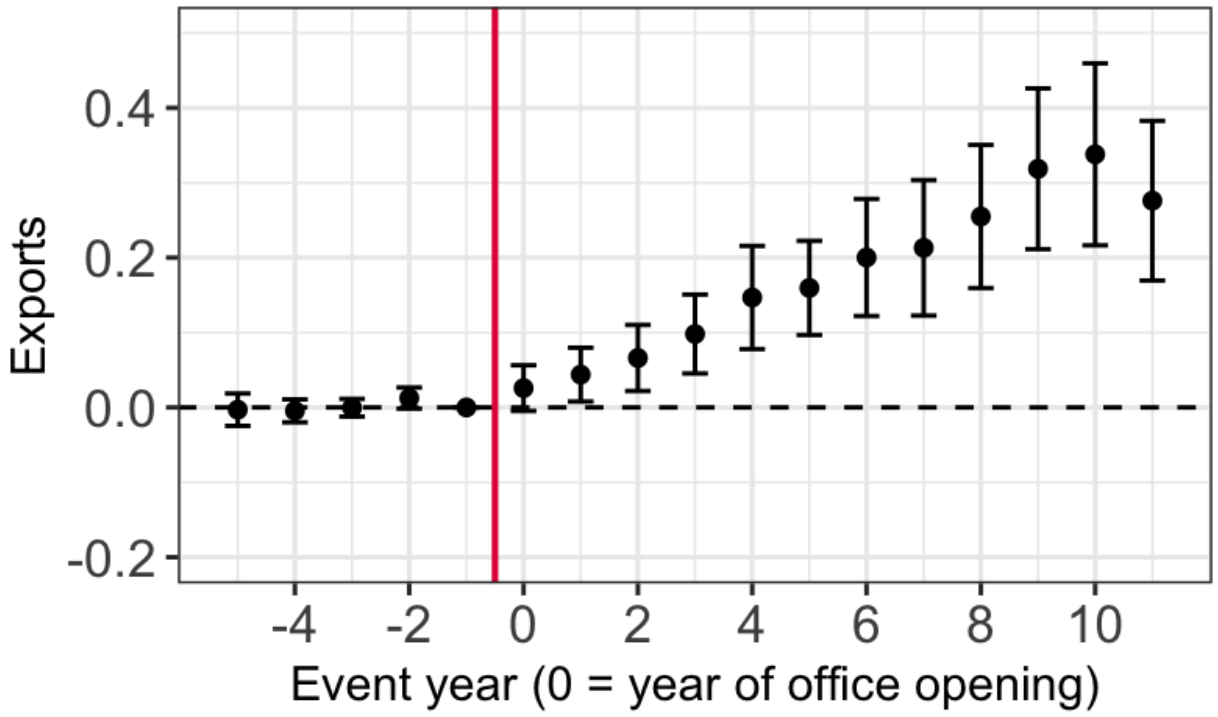
Notes: This figure presents the number of countries with an overseas export promotion office opening up until each year.

Figure 3: The roll-out of KOTRA offices to countries.



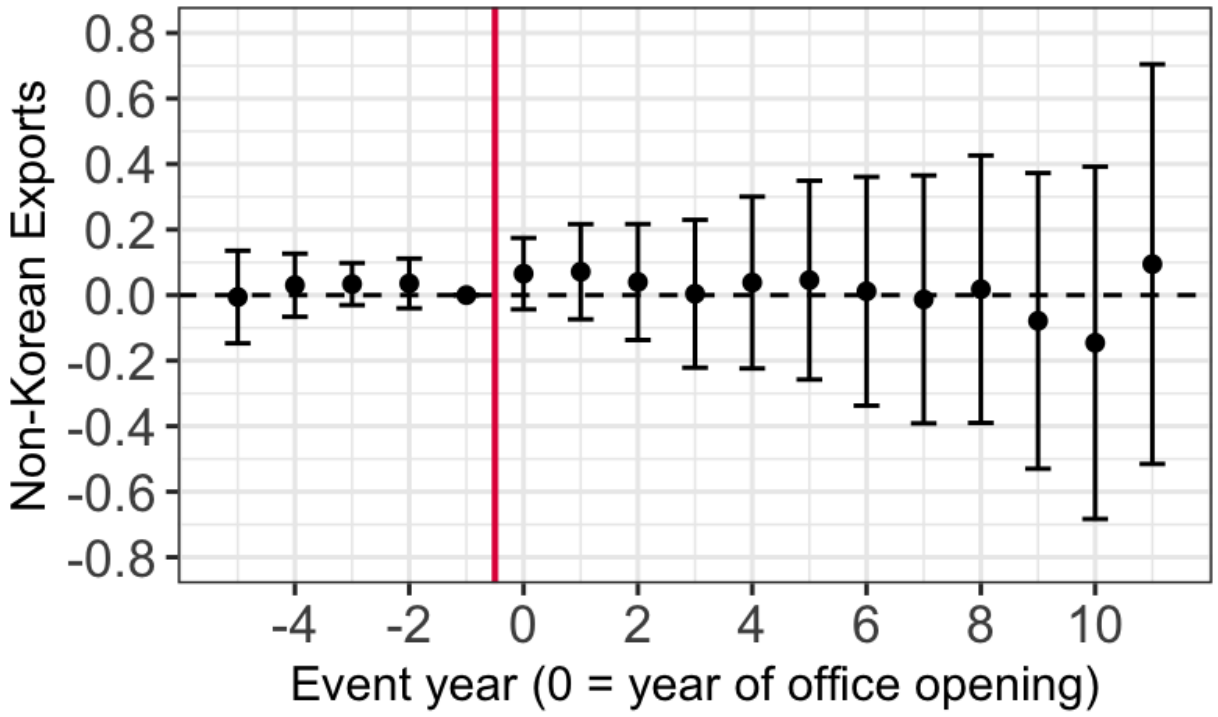
Notes: Colored countries have an office opening between 1962 and 2001. Different colors indicate the year in which the first office opened in a given country. There was a rapid roll-out until 1981 and a plateauing afterwards.

Figure 4: Event-study estimates of the effect of office opening on Korean exports.



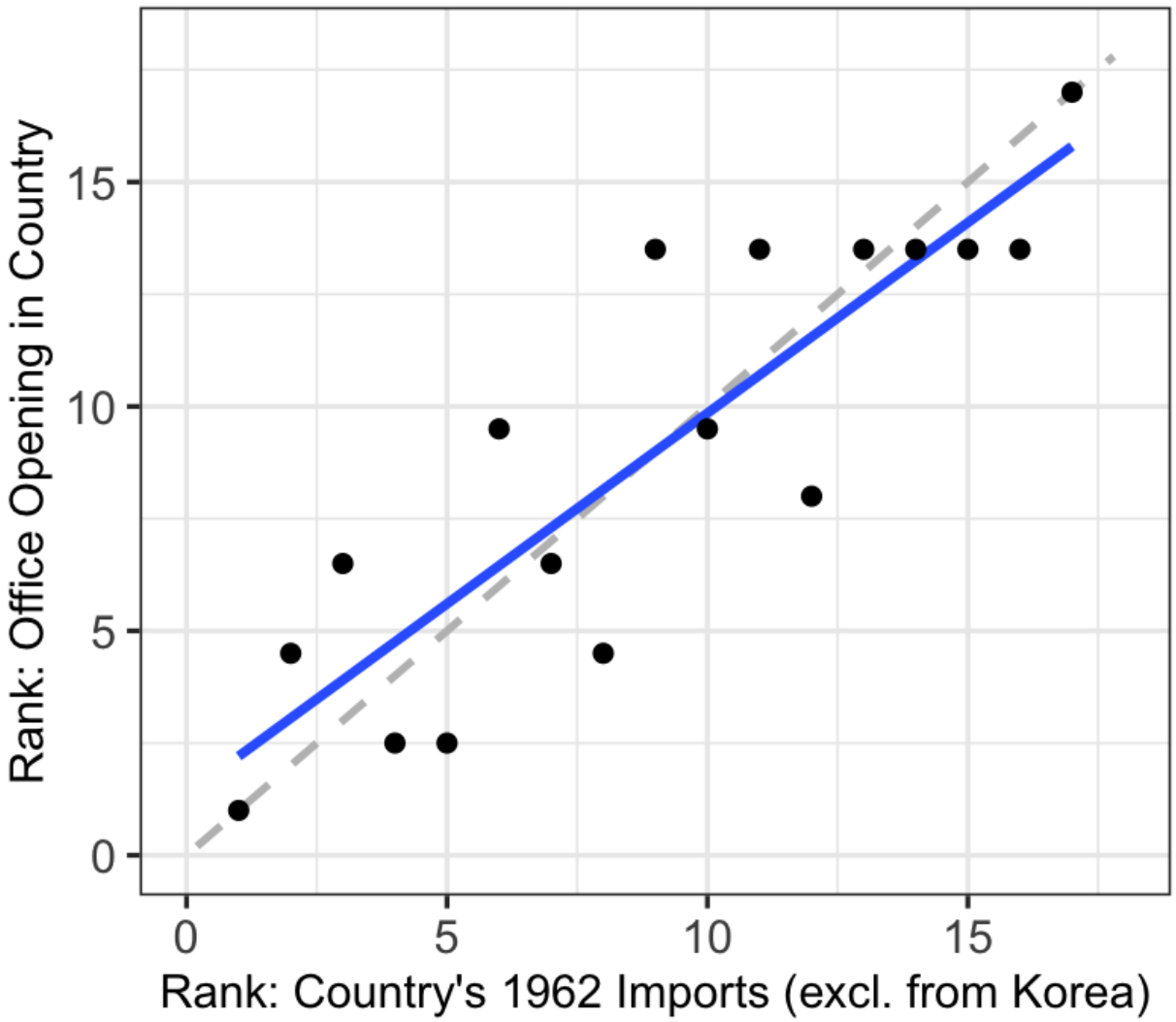
Notes: The outcome variable is the inverse hyperbolic sine of Korean exports to the country-year in question. An observation is at the product-country-year. Point estimates and standard errors are obtained from estimating equation (1). This relies on a never-treated control group. Standard errors clustered at the country-level are reported around each point estimate.

Figure 5: Event-study estimates of the (placebo) effect of office opening on import demand.



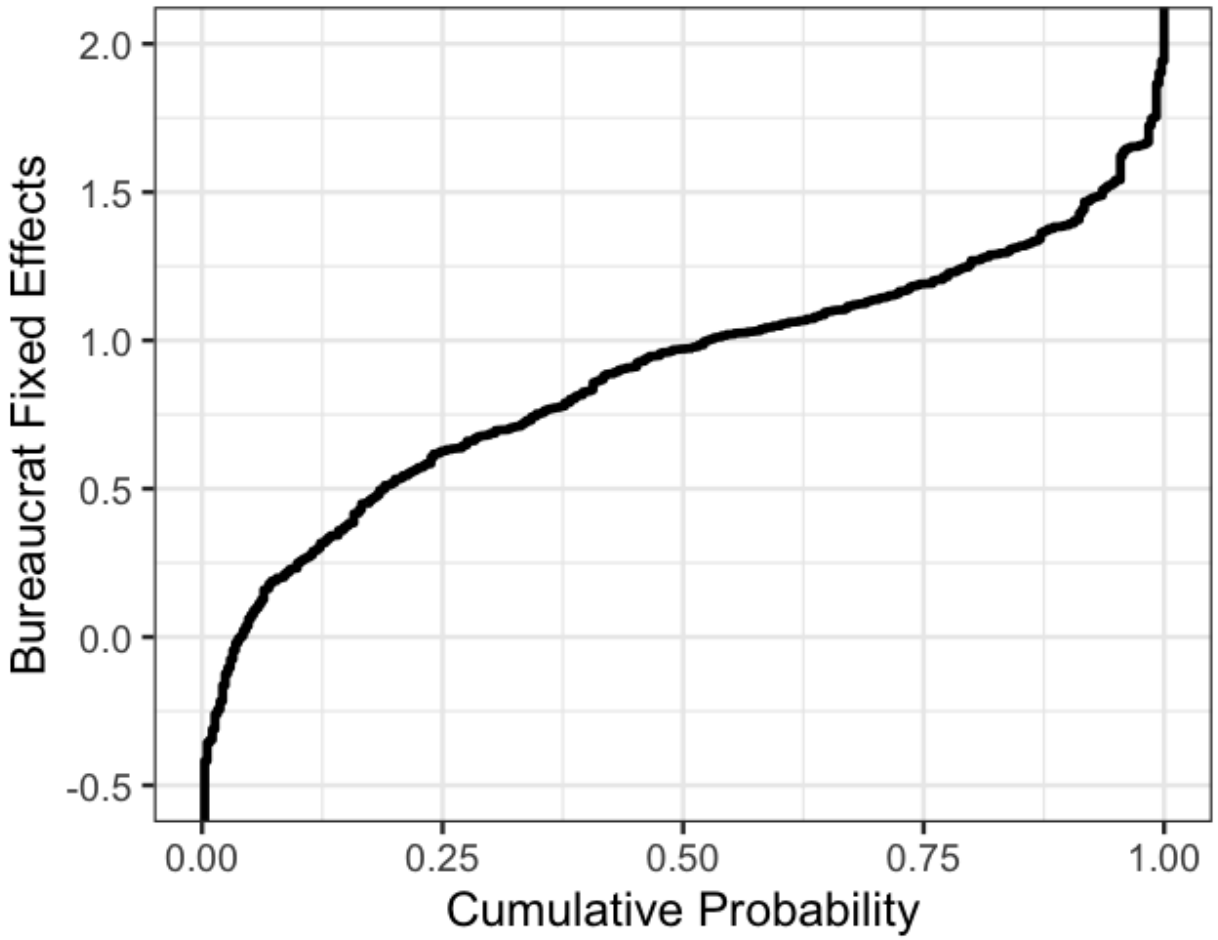
Notes: The outcome variable is the inverse hyperbolic sine of Korean exports to the country-year in question. An observation is at the product-country-year. Point estimates and standard errors are obtained from estimating equation (1). This relies on a never-treated control group. Standard errors clustered at the country-level are reported around each point estimate.

Figure 6: Europe: Openings follow pre-determined market size



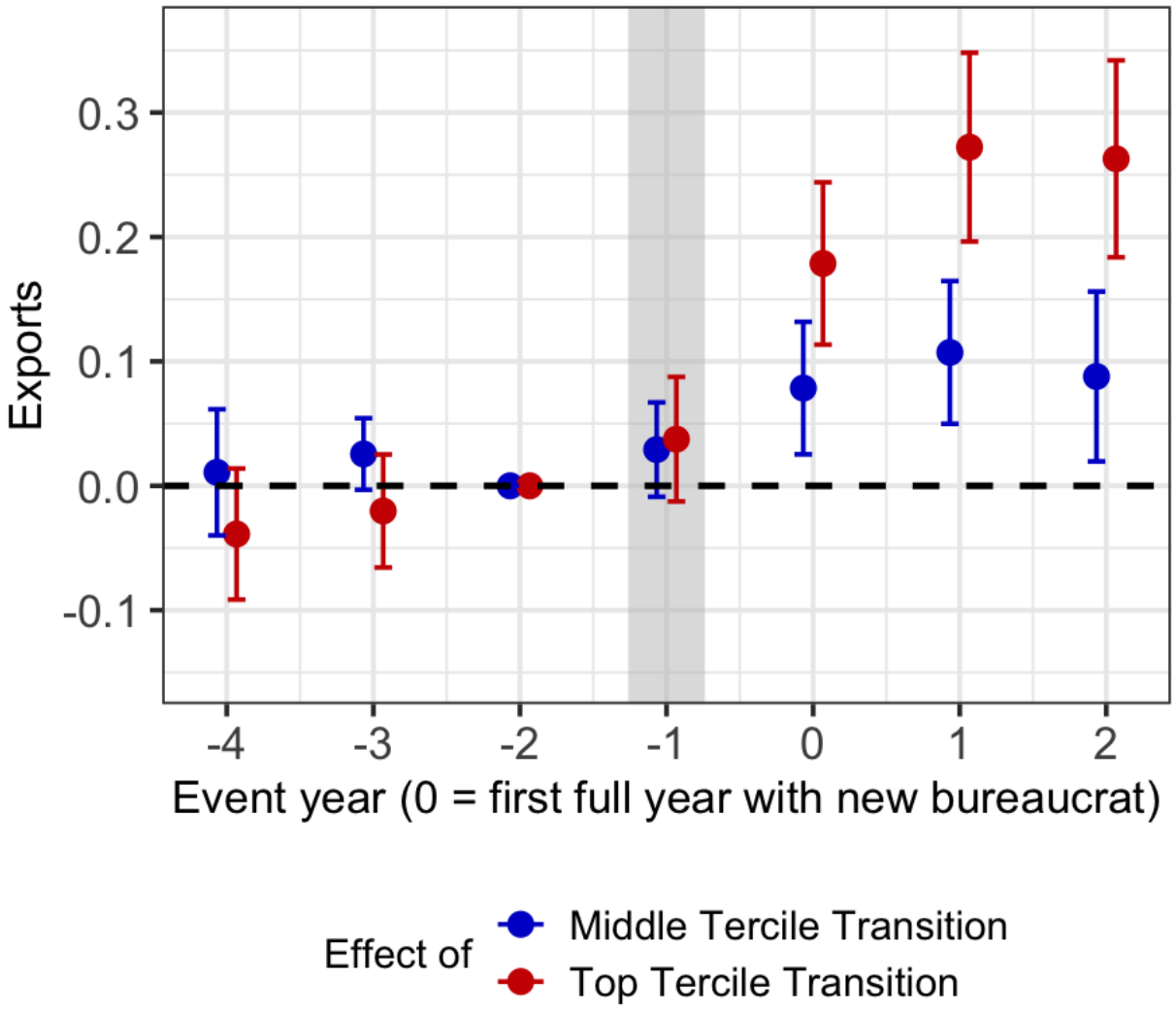
Notes: Each dot corresponds to a European country that received a KOTRA office during the main roll-out of offices (1962-1981). The x-axis gives each country's rank in terms of 1962 imports, excluding imports from Korea. The y-axis gives each country's rank in terms of the order of their office openings. 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 7: CDF of raw bureaucrat fixed effects



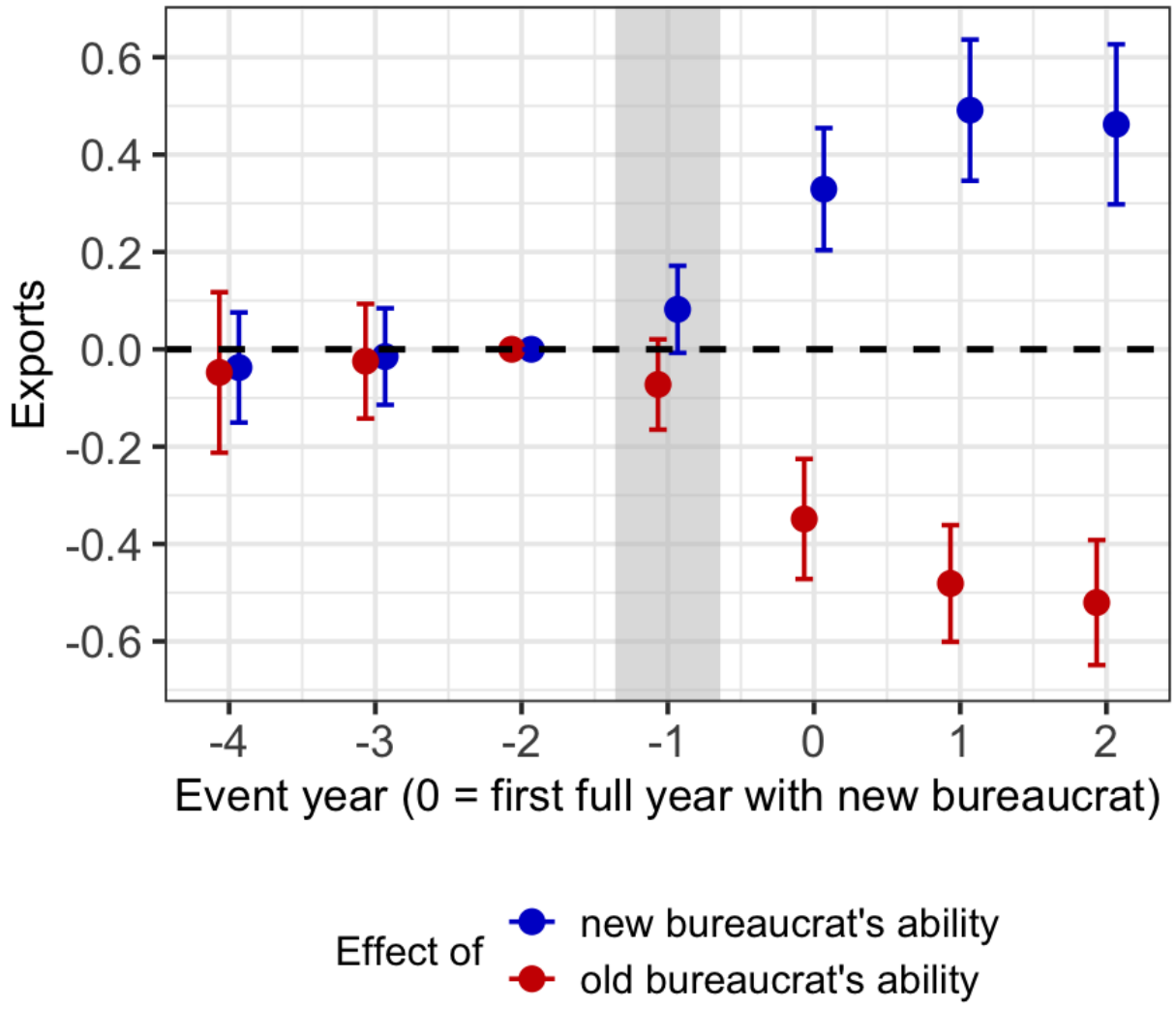
Notes: The figure shows the cumulative density function of bureaucrat fixed effects estimated based on equation (4). Even if each bureaucrat fixed effect was estimated without bias, the variance as well as the the difference between the xth and yth percentile would be overstated.

Figure 8: Switches to better bureaucrats not preceded by differential trends.



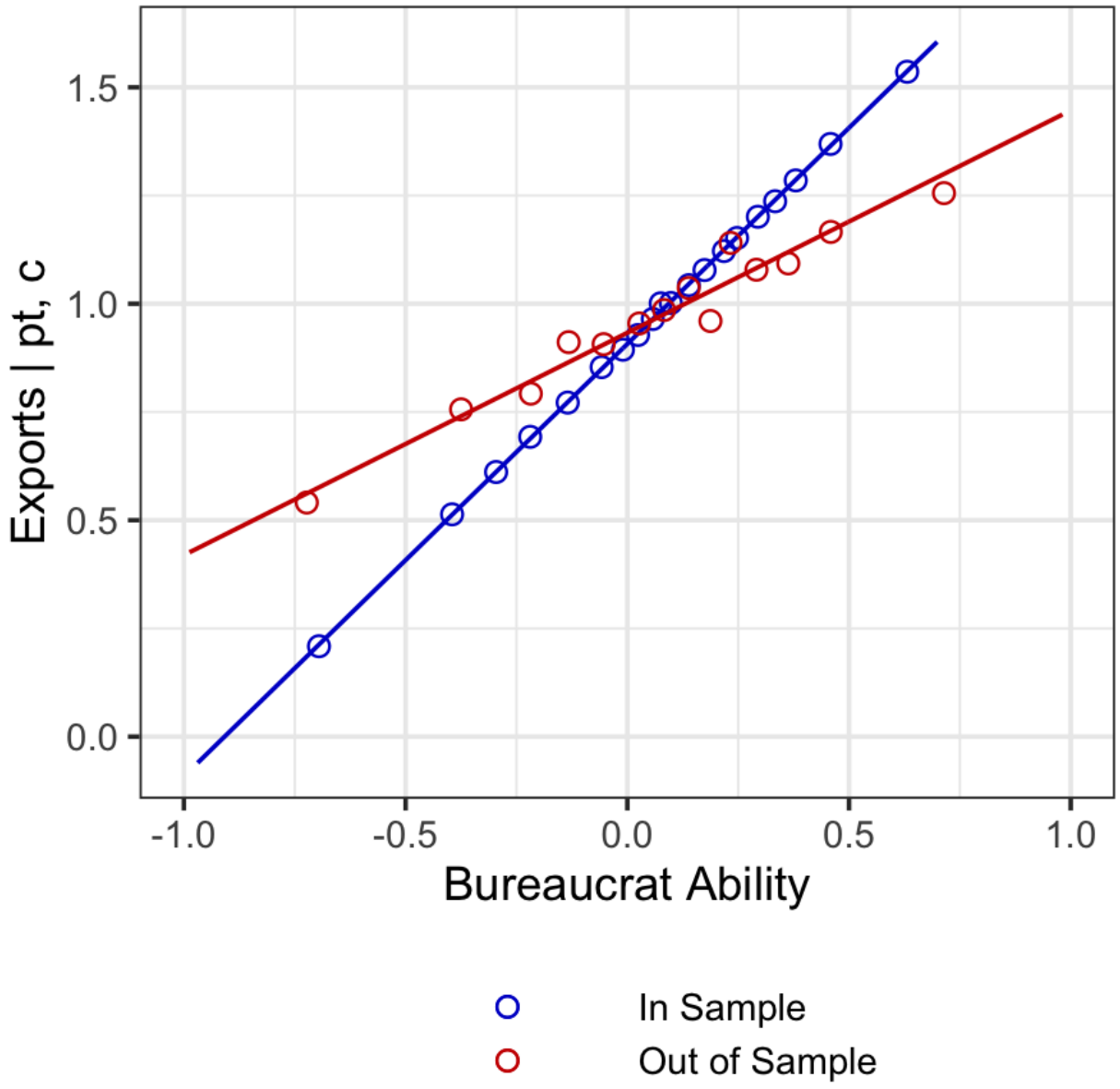
Notes: The figure shows the estimated effect of the change in bureaucrat fixed effects on exports around the time that the bureaucrat managing a country office changes. The estimates are $\hat{\beta}_k$ and $\hat{\delta}_k$ obtained from estimating equation (5). The dependent variable is the inverse 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 9: Symmetric effects from gaining and losing a bureaucrat.
No differential pre-trends.



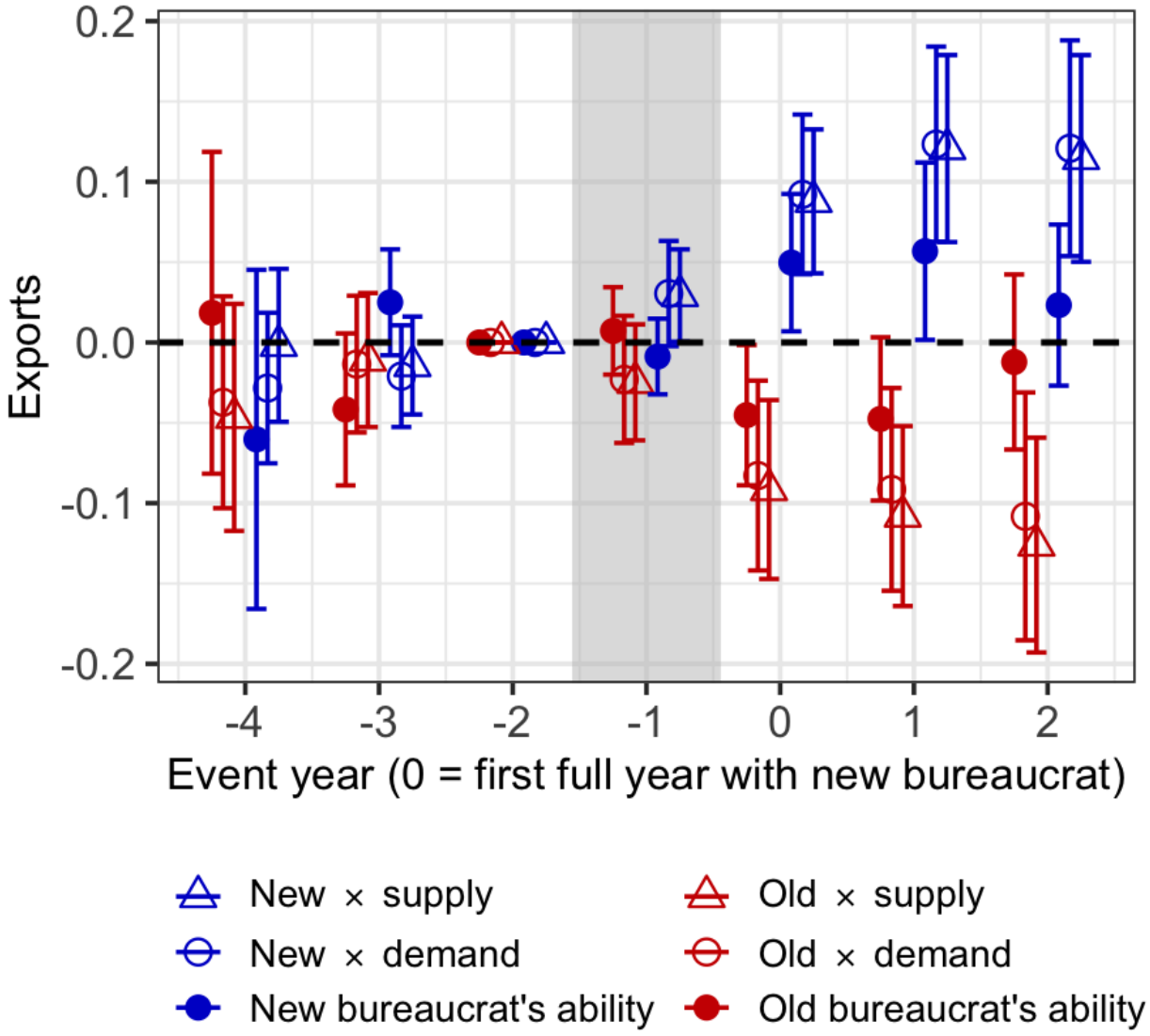
Notes: The figure shows the estimated effect of the change in bureaucrat fixed effects on exports around the time that the bureaucrat managing a country office changes. These estimates are $\hat{\beta}_k$ and $\hat{\delta}_k$ obtained from estimating equation (6). The horizontal axis indexes years in 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 10: Bureaucrat fixed effects and exports: In and Out of Sample
Out of sample effects remain predictive of exports.



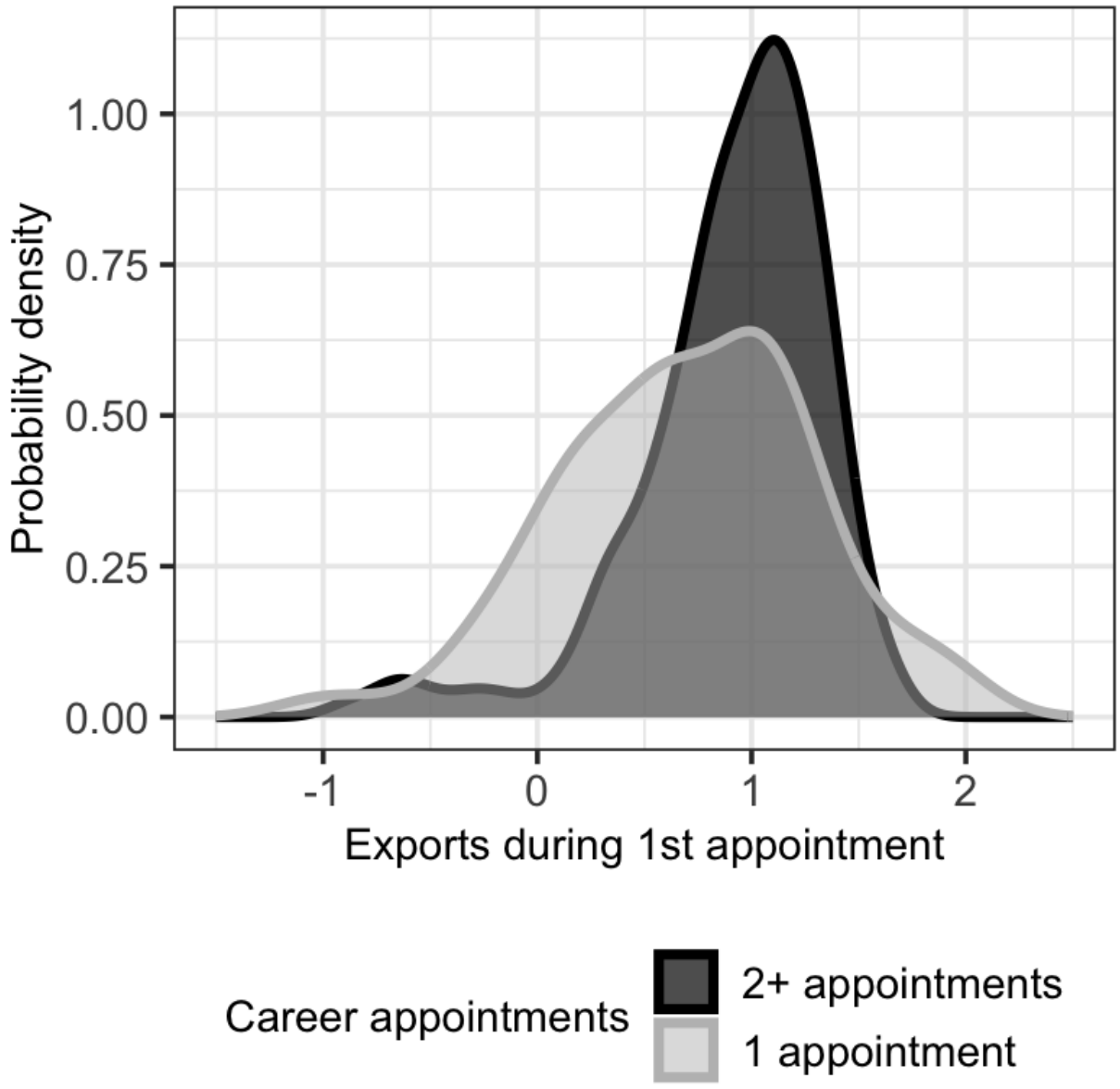
Notes: The figure displays a binned scatterplot. The y-axis shows exports after subtracting product-year fixed effects (pt) and country-year fixed effects. The two above fixed effects, as well as in-sample bureaucrat ability (fixed effects) are estimated using equation (4) and all country-years. Hence, by construction, each in-sample dot lies on a 45-degree line. This also means that in-sample fixed effects translate one-to-one into higher exports. Out-of-sample fixed effects are estimated only using other countries in estimating the fixed effects. This means to predict exports to the UK, we obtain the fixed effects on a data set using all country-years, except the UK. The slope of a regression of residualized exports on these out-of-sample, i.e. *other country*, fixed effects is 0.52.

Figure 11: Event study estimates: Decomposition
Good bureaucrats increase exports where demand (supply) are growing.



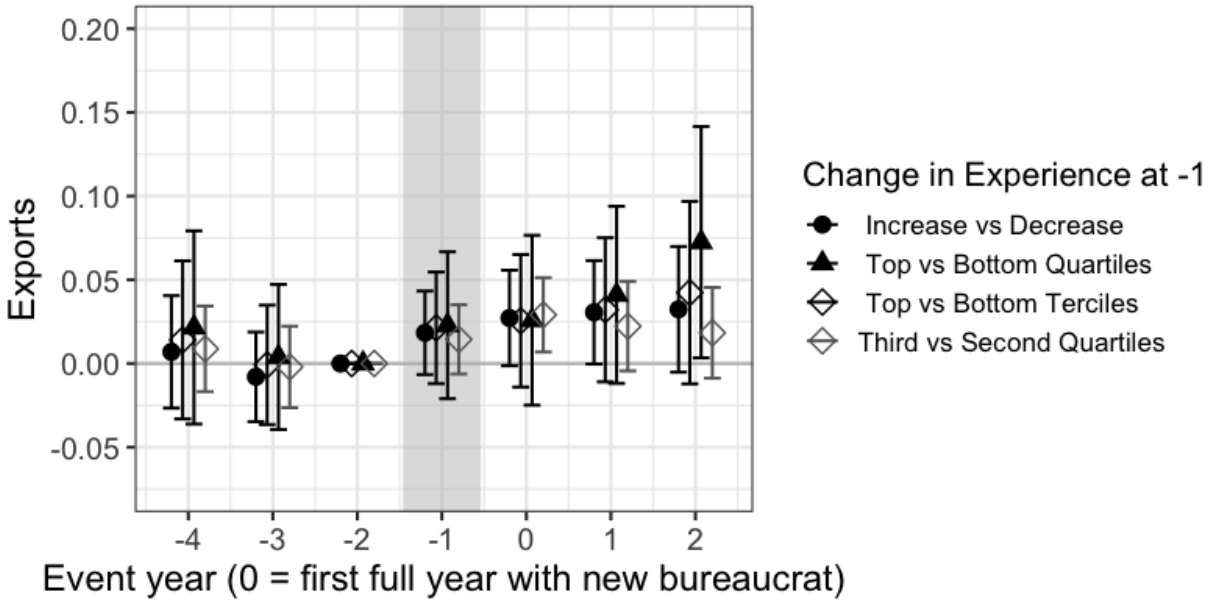
Notes: The figure shows the estimated effect of the change in bureaucrat fixed effects when interacted with two kinds of shocks. The plotted coefficients are estimates of β_k, β_k^{demand}, and β_k^{supply} as well as δ_k, δ_k^{demand}, and δ_k^{supply} obtained from regressions of equation (7). The solid circles give the main effects. The hollow circles give the interaction with exports of the same product to the same destination by other countries (β_k^{demand}, δ_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}, δ_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. Bureaucrats effectiveness are fixed effects obtained after residualizing exports by product-country and product-year fixed effects.

Figure 12: Bureaucrat effect by number of appointments in career.
2+ appointments: Less bureaucrats with negative effects



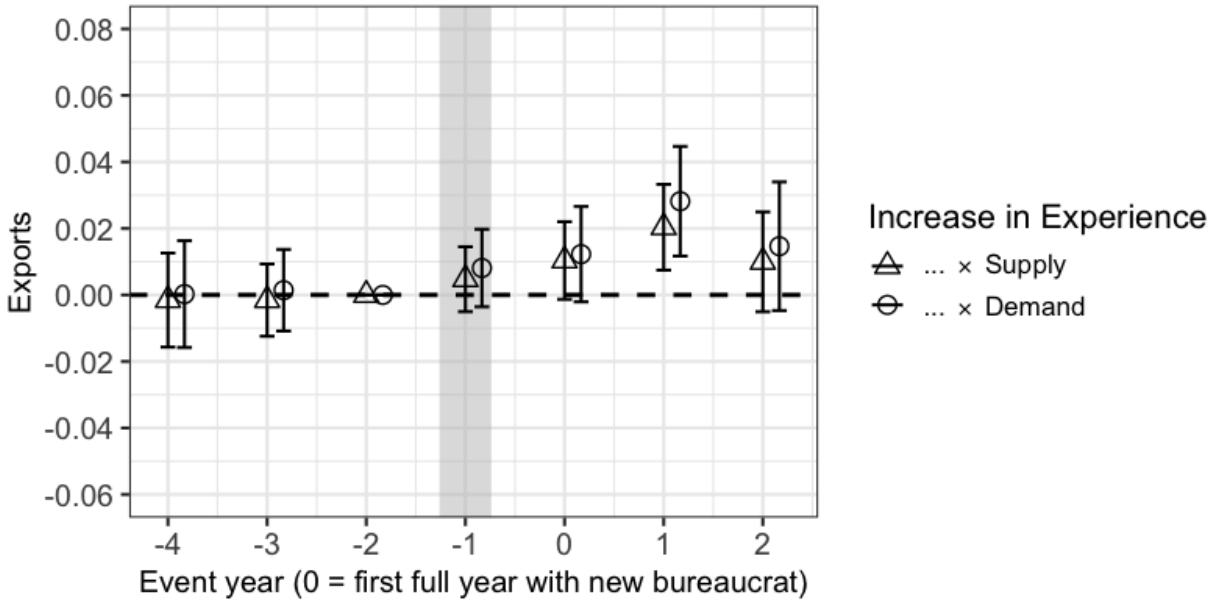
Notes: The figure shows the probability density function of residualized exports during bureaucrats' first appointments. It does so separately for bureaucrats who have 2+ appointments over the course of their career and for bureaucrats who have one career appointment. The distribution of exports under the latter group has a much fatter left tail.

Figure 13: 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 14: 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	Country Offices	Connected Set	Leave-One-Out Connected Set
	(1)	(2)	(3)	(4)
# Managers	475	398	397	380
# Countries/Offices	138	87	86	75
# Events/Appointments	974	729	728	676
# Managers $>$ 1 Office	252	194	194	180
# Offices $>$ 1 Managers	121	82	82	75
# Offices $>$ 3 Managers				72
# Offices $>$ 5 Managers				61
# Offices $>$ 7 Managers				49

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 overseas office. Column (2) restricts this to each country's main office in order to create a one-to-one mapping from KOTRA offices to export flows. Column (3) further restricts this to those countries and managers which form the largest connected set, while column (4) 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. In columns (2)-(4), 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.

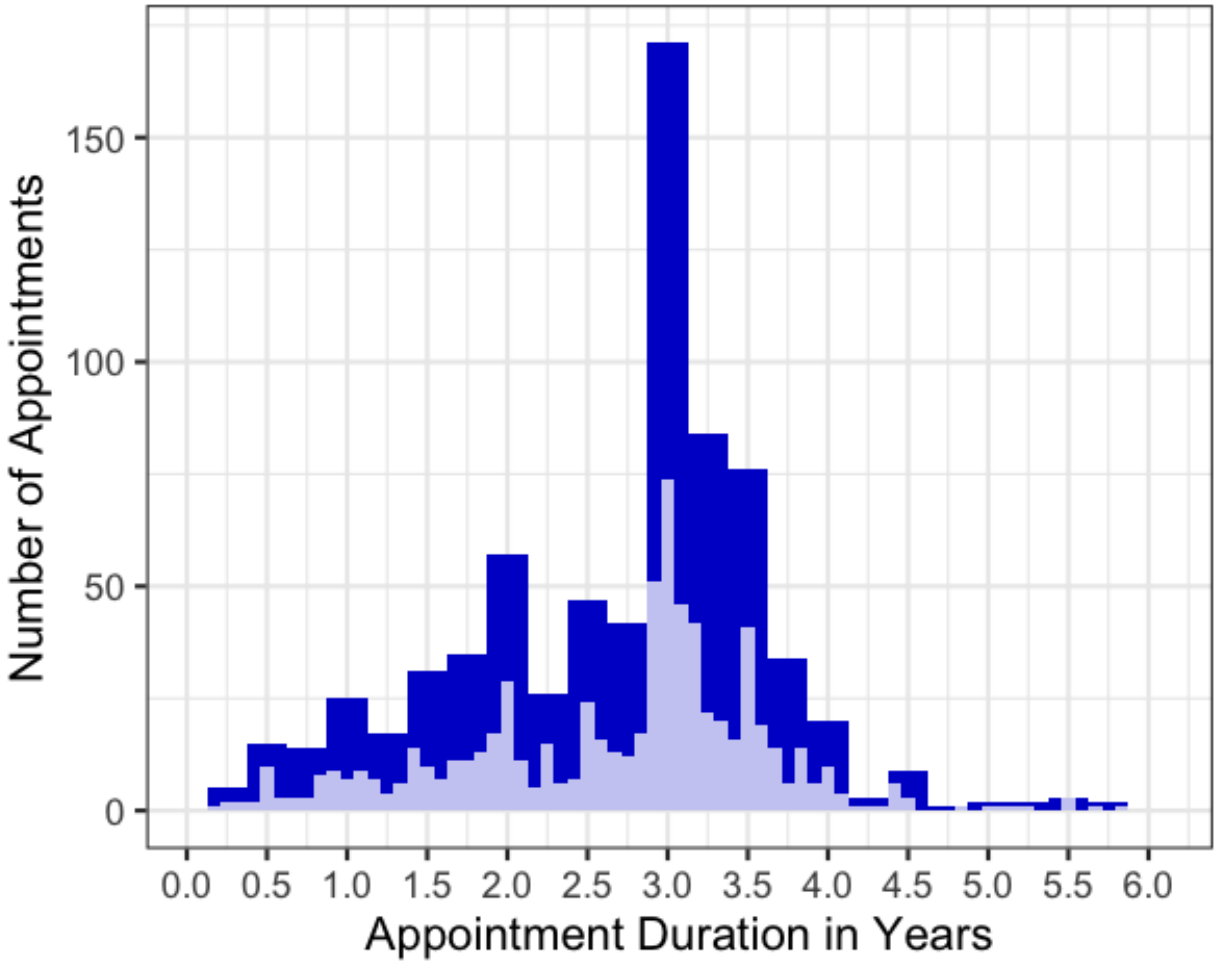
Table 2: Variance decomposition of exports

		Actual data				Placebo check: Bureaucrats randomly shuffled to countries			
		All bureaucrats		Bureaucrats with ≥ 2 appointments		All bureaucrats		Bureaucrats with ≥ 2 appointments	
		Component	% Share	Component	% Share	Component	% Share	Component	% Share
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Var(exports|*pt*), spell-level		0.732	100	0.737	100	0.737	100	0.736	100
Var(bureaucrat)		0.100	13.71	0.056	7.60	0.006	0.77	0.006	0.81
Var(country)		0.722	98.60	0.695	94.29	0.591	80.19	0.589	80.07
Cov(bureaucrat, country)		-0.088	-12.04	-0.045	-6.15	-0.005	-0.67	-0.003	-0.44
Var(bureaucrat+country)		0.646	88.24	0.659	89.45	0.586	79.59	0.588	79.94
Var(exports|*pt*), raw		4.404		4.645		4.360		4.343	
Number of observations		1703465		1222986		1757034.0		1228255.6	
Number of bureaucrats		380		184		389.2		182.7	
by no. of spells in sample:	1	200		4		209.0		2.8	
	2	96		96		99.1		98.3	
	3	56		56		53.8		54.9	
	4	24		24		21.5		21.1	
	5	4		4		5.8		5.7	
Number of countries		75		75		78.7		78.4	

The results of variance decomposition exercise according to equation (3). Columns (1)-(4) use actual data while columns (5)-(8) use data where bureaucrats are randomly shuffled to countries, preserving the number of appointment spells in the data for each bureaucrat. For columns (3), (4), (7), and (8), an initial sample restriction of bureaucrats with at least two appointments is applied. The *limited mobility bias* correction method follows Kline, Saggio, and Sølvsten (2020) and is implemented via the algorithm of Bonhomme, Holzheu, Lamadon, Manresa, Mogstad, and Setzler (2023) It is possible that there are bureaucrats with only one spell in the sample even when the sample is pre-restricted to bureaucrats with at least two appointments, because some spells drop out when constructing the leave-one-spell-out connected set for the Kline, Saggio, and Sølvsten (2020) method. 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)-(8) arising from the random shuffling of bureaucrats. Thus, we report the averages of 100 iterations for all columns.

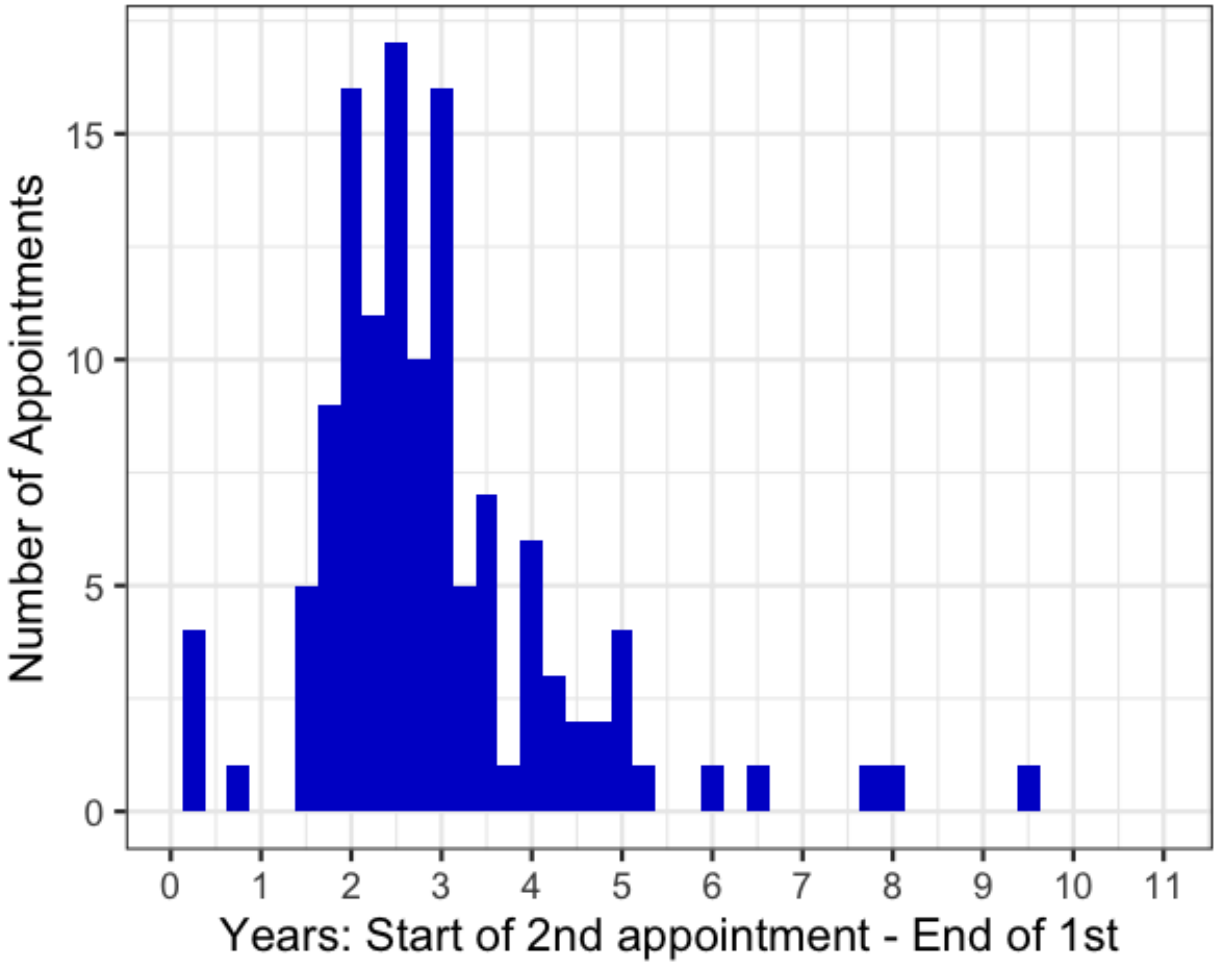
A Appendix Figures

Figure A.1: Distribution of appointment durations.
Median and modal duration: 36 months.



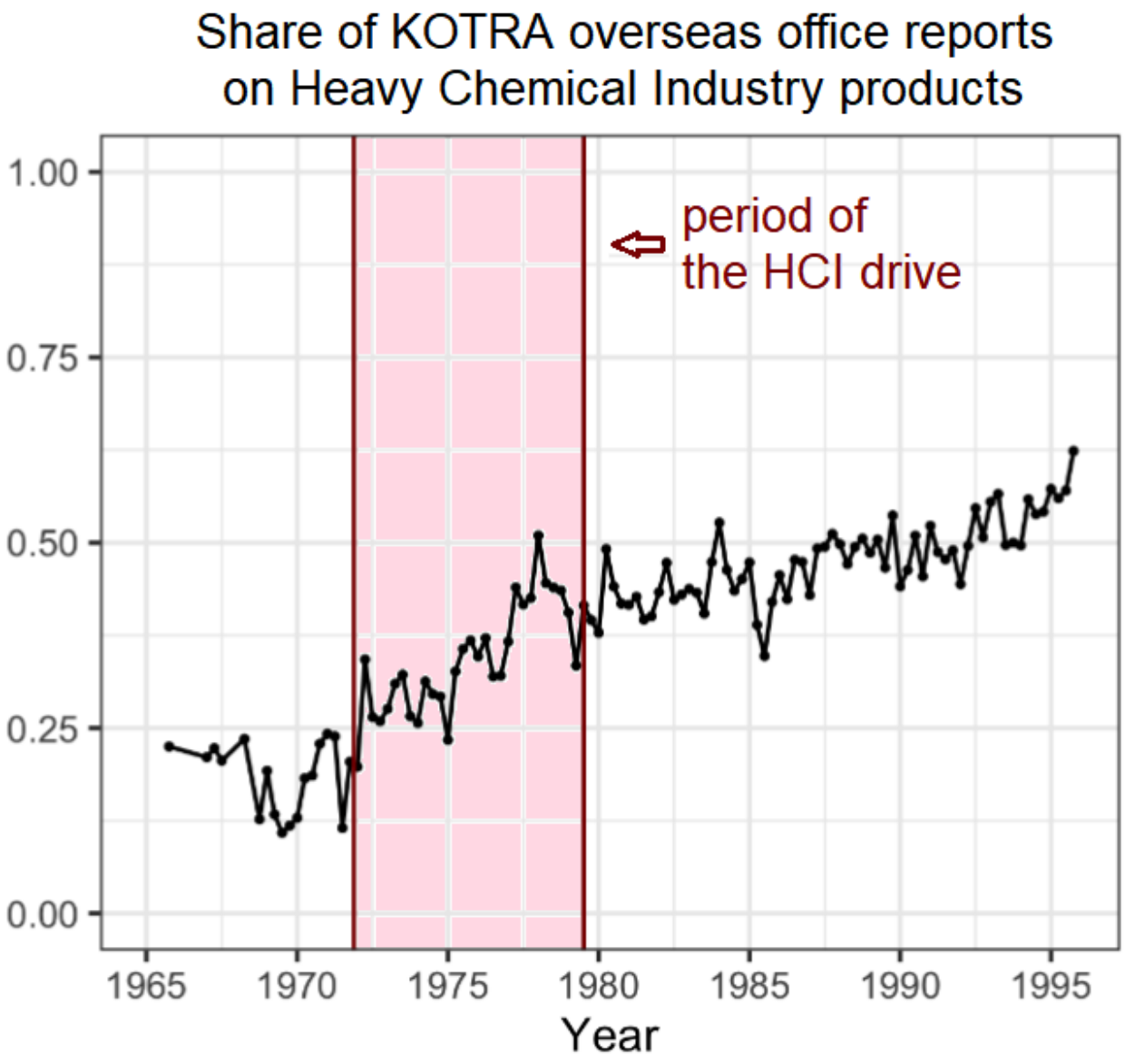
Notes: This figure represents the distribution of appointment durations. The blue bars indicate the number of appointments by quarterly duration whereas the white bars do so for the number of appointments by monthly duration. Hence, as each quarter contains multiple months, the blue bars always (weakly) 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.2: Distribution of gap lengths.
Median: 29 months. Mode: 30 months.



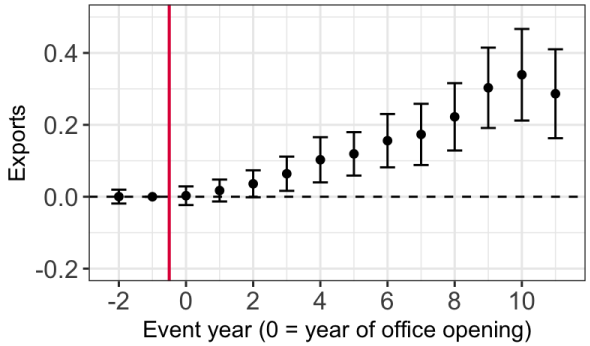
Notes: This figure represents the distribution of the duration of gaps between appointments. The blue bars indicate the number of gaps by quarterly duration.

Figure A.3: Targeting of export promotion activity by product.
Export promotion activity moves in parallel with national industrial policy

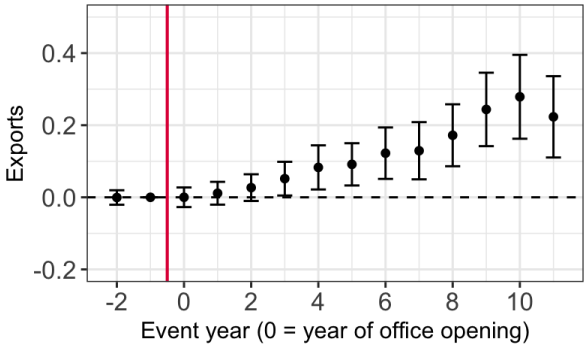


Notes: Targeting of export promotion activity by product. For each quarter, the y-axis presents the share of overseas office reports that could be linked to an HCI product relative to the number of reports that could be linked to any product.

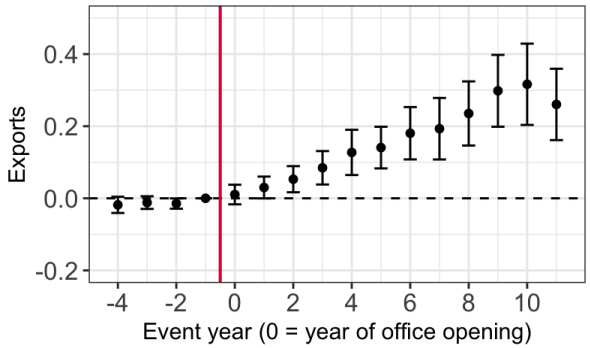
Figure A.4: Robustness: controls, sample, placebo



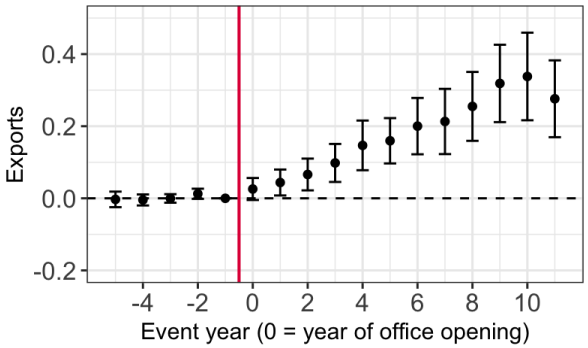
(a) Controlling for non-Korean exports.



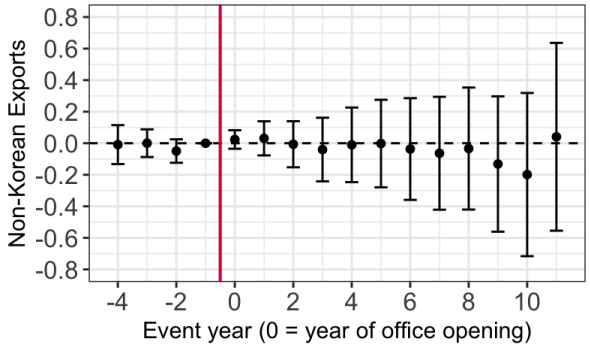
(b) Controlling for year × non-Korean exports



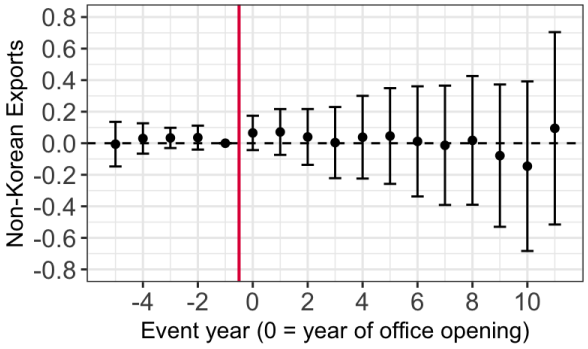
(c) Korean exports as outcome. Openings from 1966. Never-treated as control group.



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



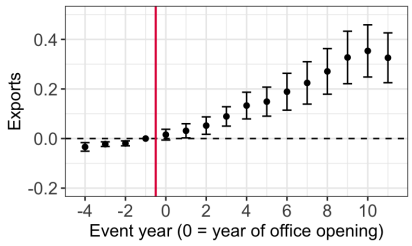
(e) Non-Korean exports as outcome. Openings from 1966. Never-treated as control group.



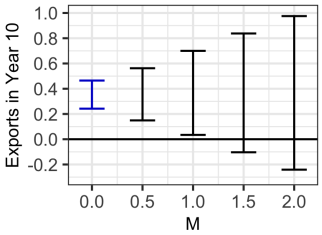
(f) Non-Korean exports as outcome. Openings from 1967. Never-treated as control group.

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 (1), 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.

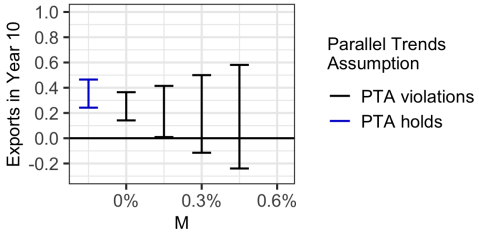
Figure A.5: Robustness: opening with not-yet-treated control



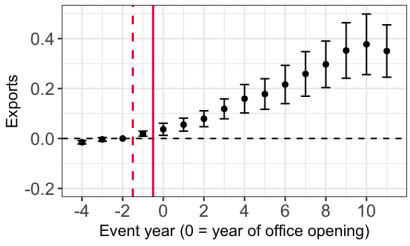
(a.i) CSA estimate, uncon tional PTA.
0 periods of anticipation.



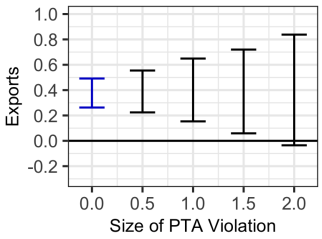
(a.ii) Sensitivity to PTA violation relative to largest pre-treatment violation of PT.



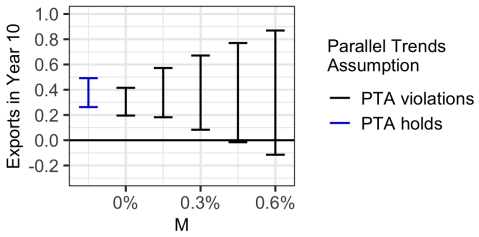
(a.iii) Sensitivity to PTA violations only bounding the extent to which the slope may change across consecutive periods.



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



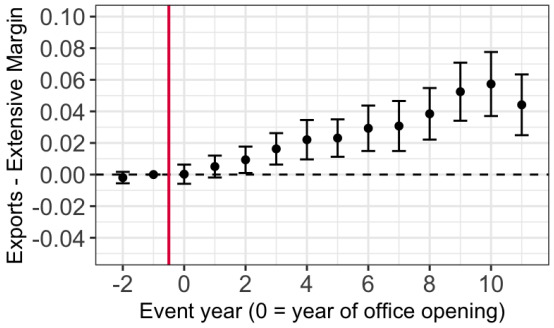
(b.ii) Sensitivity to PTA violation relative to largest pre-treatment violation of PT.



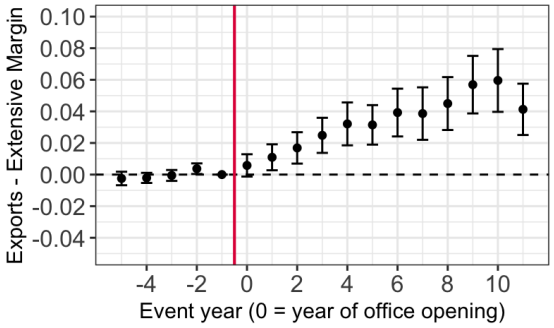
(b.iii) Sensitivity to PTA violations only bounding the extent to which the slope may change across consecutive periods.

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.

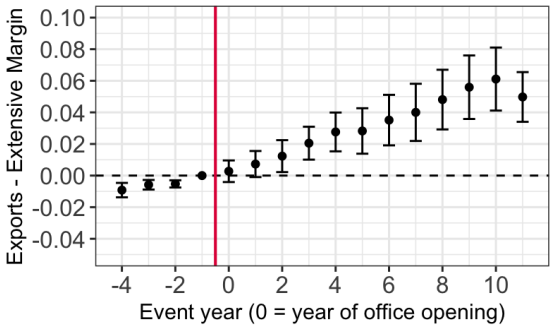
Figure A.6: Extensive margin effect of office opening



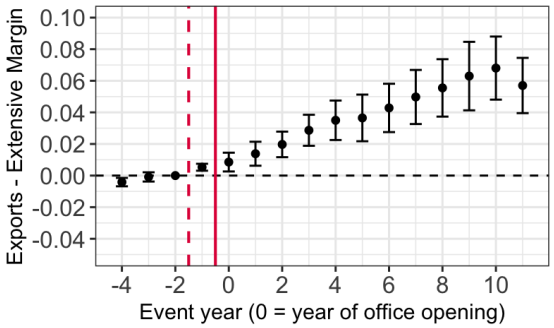
(a) Never-treated as control group. Openings from 1964.



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



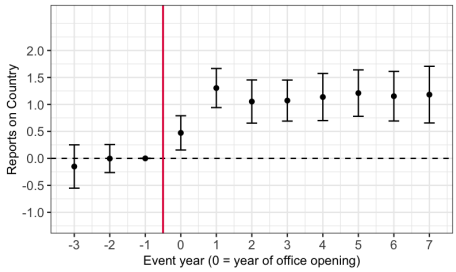
(c) "Not-yet" control. 0 period anticipation.



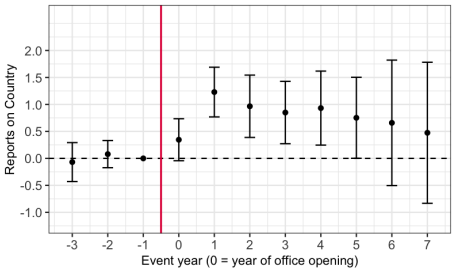
(d) "Not-yet" control. 1 period anticipation.

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 (1, 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.

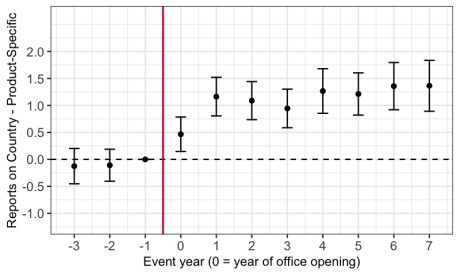
Figure A.7: Event-study estimates of office opening on KOTRA activity



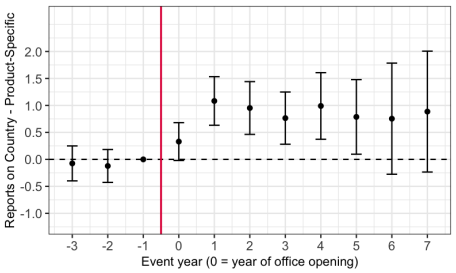
(a) Reports. Control: never-treated.



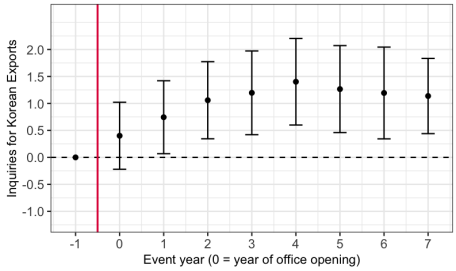
(b) Reports. Control: "not-yet".



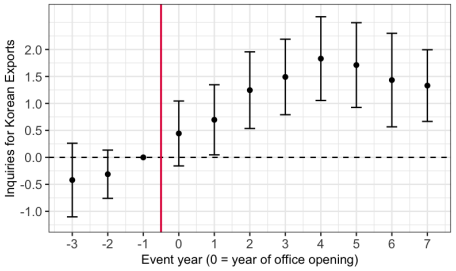
(c) Reports (product). C: never-treated.



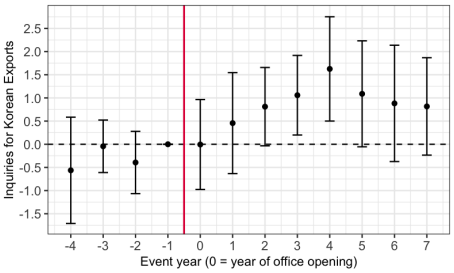
(d) Reports (product). C: "not-yet".



(e) Inquiries. Control: never-treated.



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

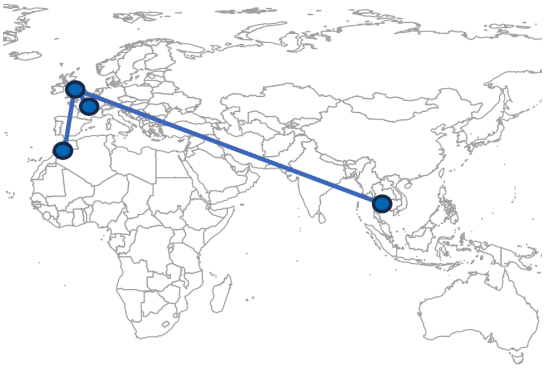


(g) Inquiries. Control: never-treated. Openings from 1978.

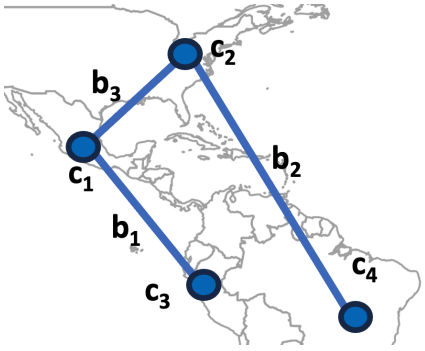
Notes: The left panels reports coefficients θ_k from estimating equation (1) when explaining three different measures of KOTRA activity regarding a specific country: number of reports written, number of product-specific reports, number of inquiries obtained - each transformed by the inverse hyperbolic 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.8: KOTRA Bureaucrats' Rotation Results in a Single Connected Set

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

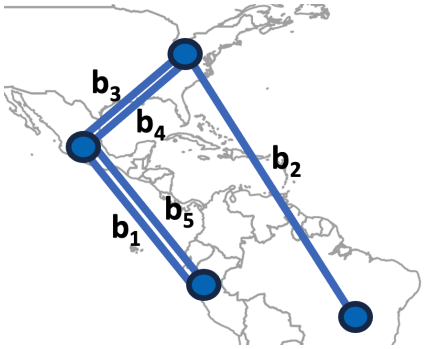


(b) Connections between the UK and other countries due to the bureaucrat appointed to manage the London office in 1981.



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

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

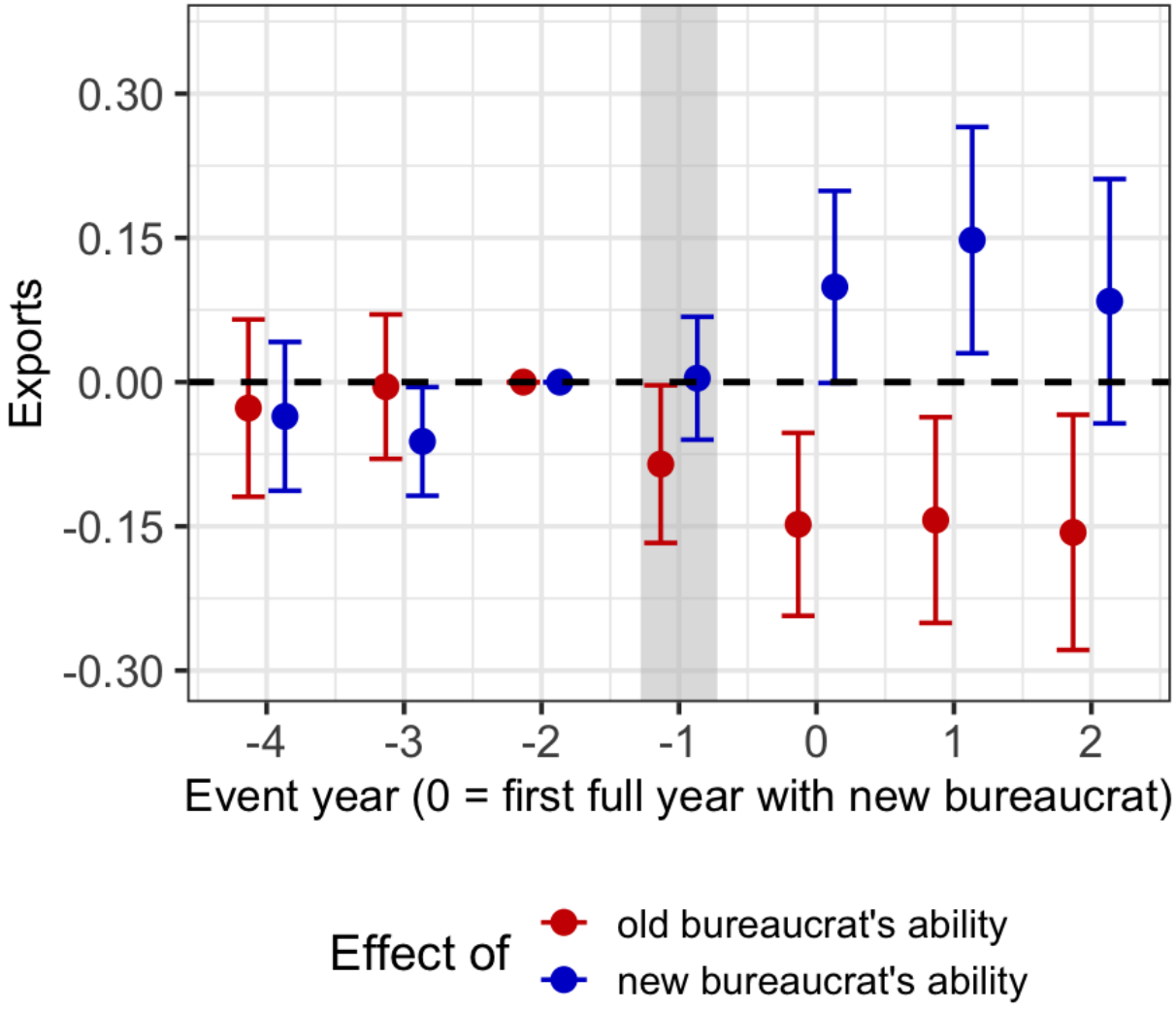


(e) Largest leave-one-out connected set includes c_1, c_2, & c_3, but not c_4.

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

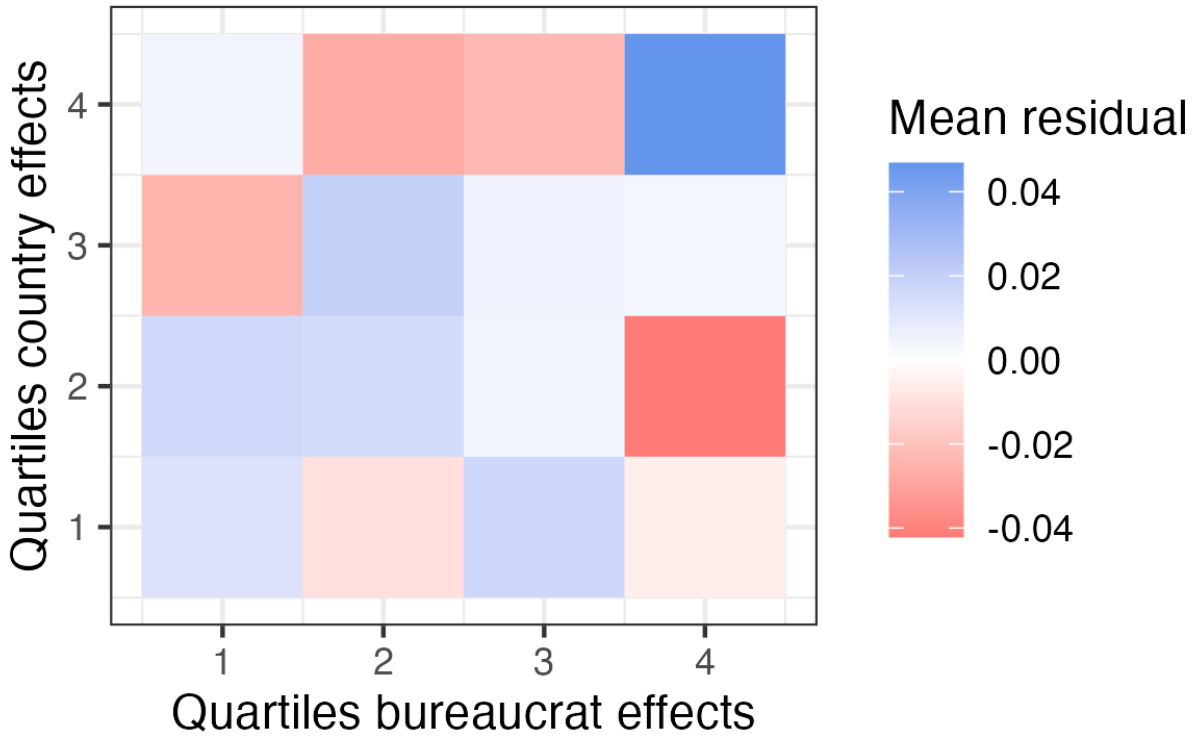
Notes: This figure highlights how this paper's data fulfills the requirement for the country–bureaucrat graph to form a single connected set. Panels (a), (c), and (e) display a hypothetical country–bureaucrat graphs. The nodes indicate the countries, the edges indicate bureaucrats who are (subsequently) 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.9: Event study estimates: Out-of-sample bureaucrat fixed effects



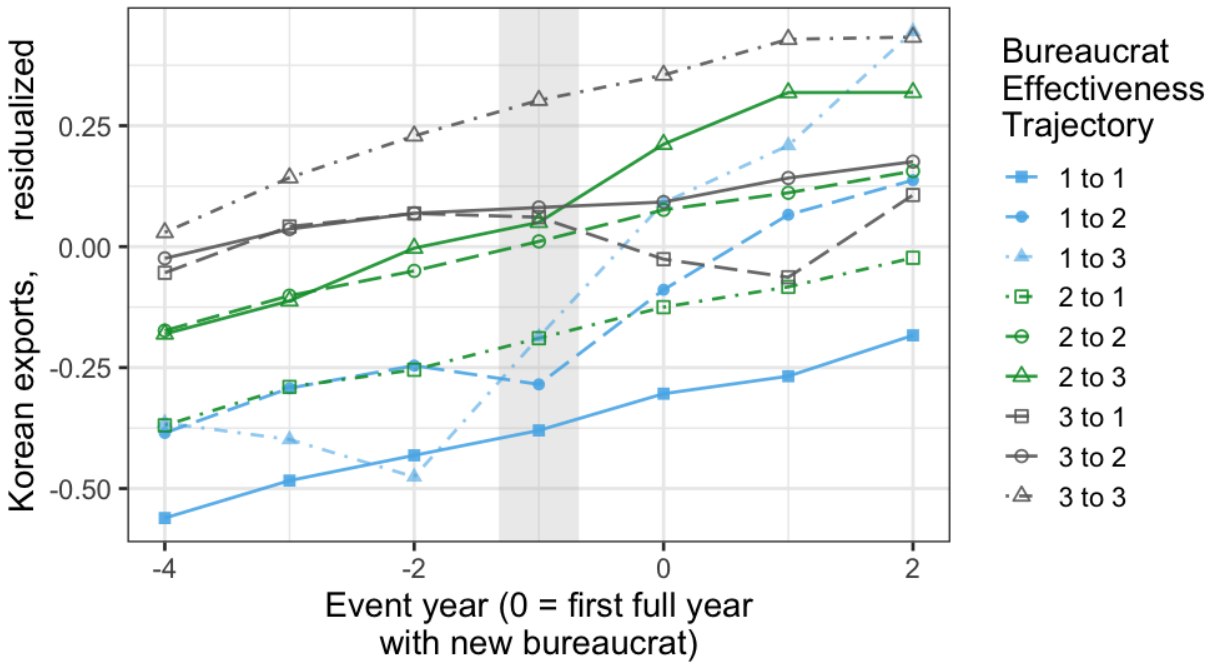
Notes: The figure shows the estimated effect of the change in bureaucrat fixed effects, estimated out of sample, on exports around the time that the manager of a country office changes. These estimates are $\hat{\beta}_k$ and $\hat{\delta}_k$ obtained from estimating equation (6). As out-of-sample fixed effects are not available for every bureaucrat, to maximize power, we report coefficients from two different models. First, we estimate equation (6) using *out-of-sample* estimates for the outgoing bureaucrat and *in-sample* estimates for the incoming bureaucrat. Second, we estimate equation (6) 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.10: Residuals by estimated bureaucrat and organization effects. Absence of clear pattern which would point to misspecification.



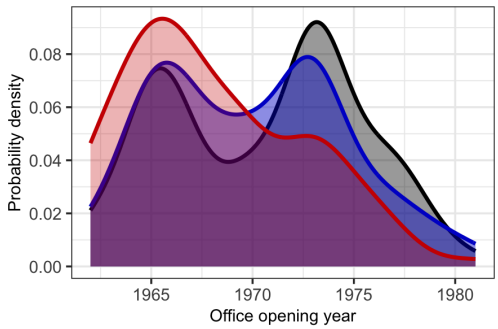
Notes: This figure shows mean residuals from equation 4 with cells defined by quartiles of estimated bureaucrat effect, interacted with quartiles of estimated country effect.

Figure A.11: Mean residualized exports around switches between bureaucrats. Effects consistent across terciles of new and old bureaucrats.

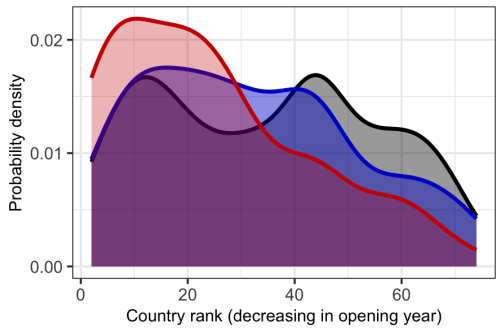


Notes: The figure shows time trends in exports around the time that the manager of a country office changes. The horizontal axis indexes years in which bureaucrats work in a particular country. Year 0 is the first full year that the new bureaucrat manages the country office. Year -2 is the last full 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.

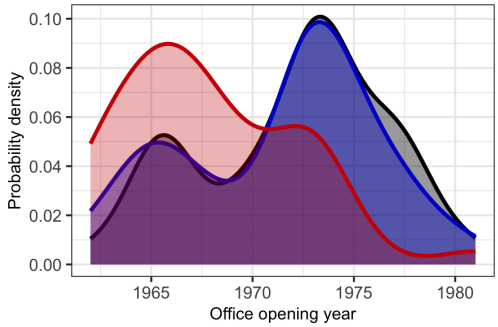
Figure A.12: As bureaucrats' careers progress they are appointed to more offices that opened earlier (proxying for importance).



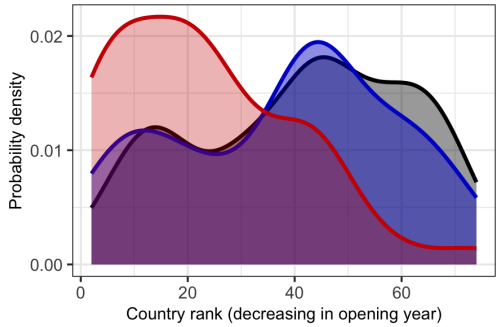
(a) PDF of bureaucrats' 1st, 2nd, 3rd appointment by opening year of the office.



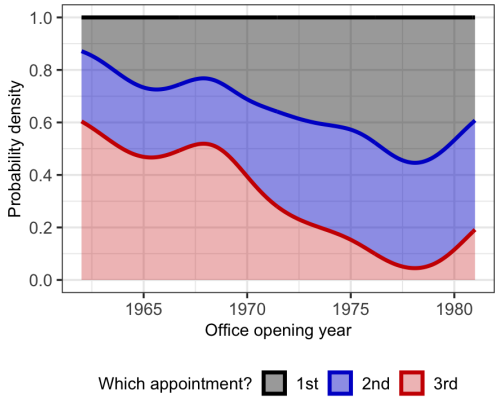
(b) PDF of bureaucrats' 1st, 2nd, 3rd appointment by opening rank of the office.



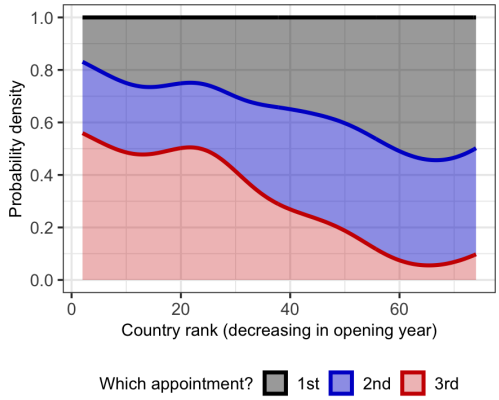
(c) Same as (a), but restricted to bureaucrats whose first appointment started no earlier than 1981.



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

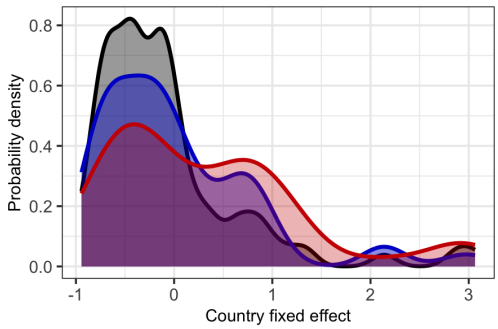


(e) Same as (c), but showing shares of appointments.

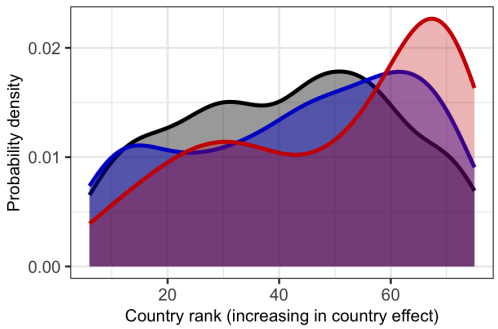


(f) Same as (d), but showing shares of appointments.

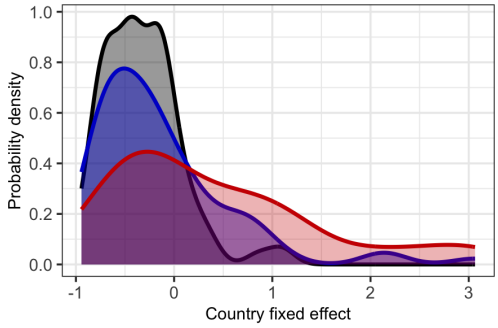
Figure A.13: As bureaucrats' careers progress they are appointed to to countries with higher fixed effects.



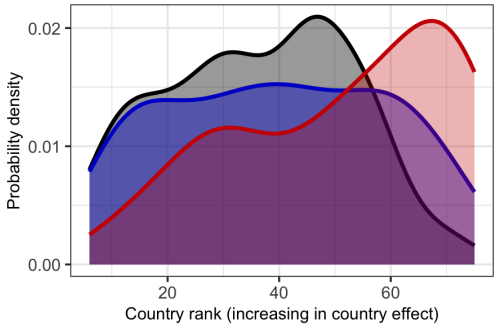
(a) PDF of bureaucrats' 1st, 2nd, 3rd appointment by country effect.



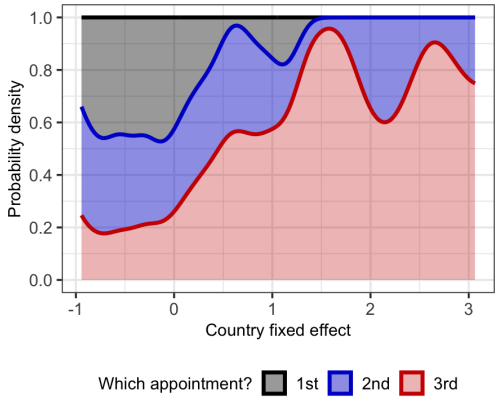
(b) PDF of bureaucrats' 1st, 2nd, 3rd appointment by *rank* of country effect.



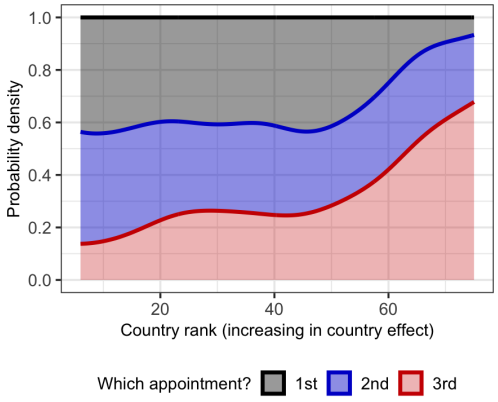
(c) Same as (a), but restricted to bureaucrats whose first appointment started no earlier than 1981.



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

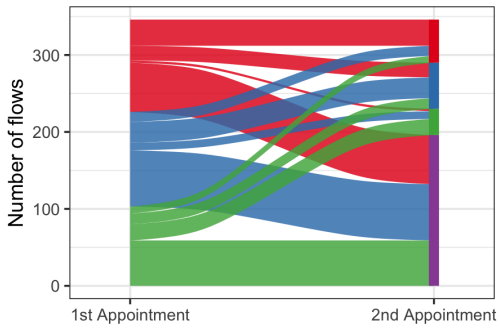


(e) Same as (c), but showing shares of appointments.

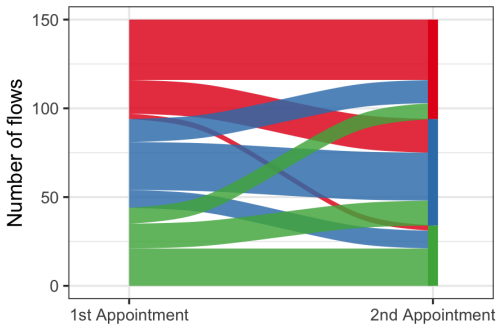


(f) Same as (d), but showing shares of appointments.

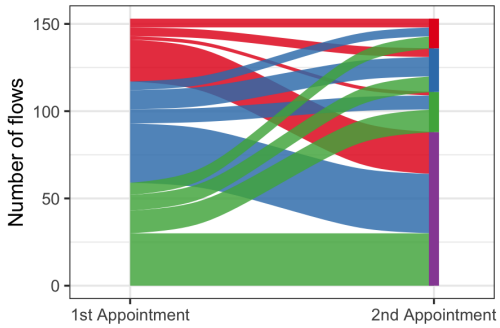
Figure A.14: Bureaucrat flows (by appointment & opening year)



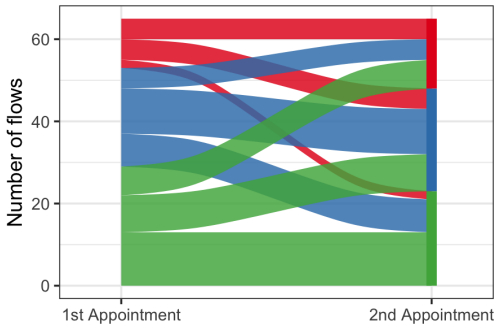
(a) All flows from 1st appointment.



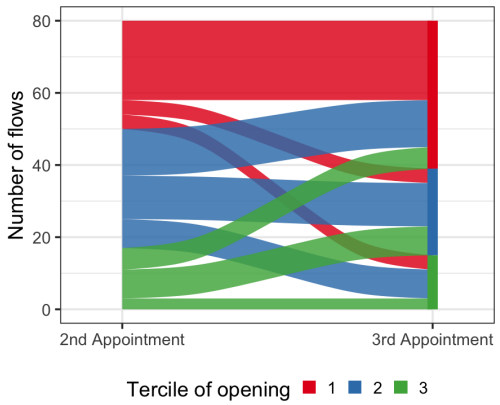
(b) Flows from 1st app. (no exit).



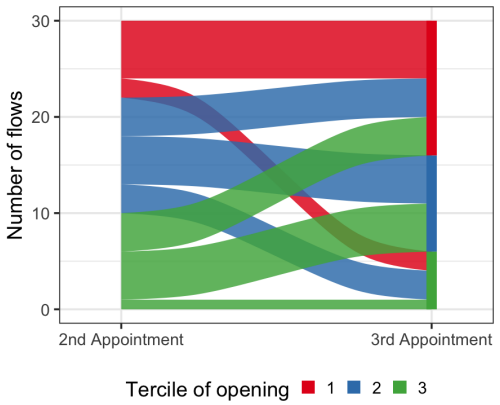
(c) Flows from 1st app. & bureaucrats without appointment before 1981.



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



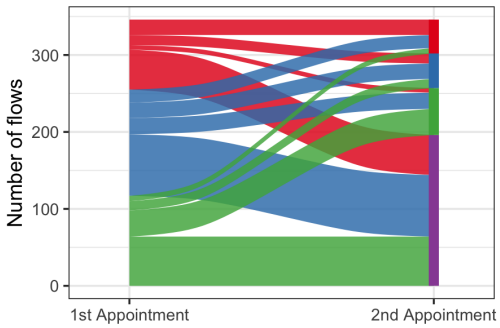
(e) Flows (except exits) from 2nd app.



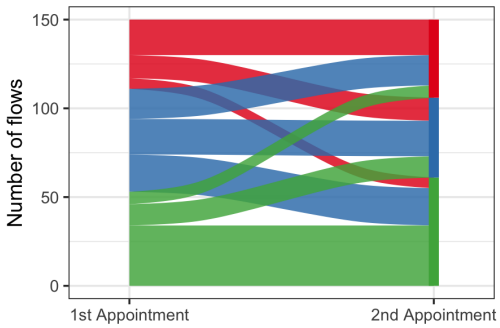
(f) Flows (except exits) from 2nd app. & bureaucrats without app. until 1981.

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.

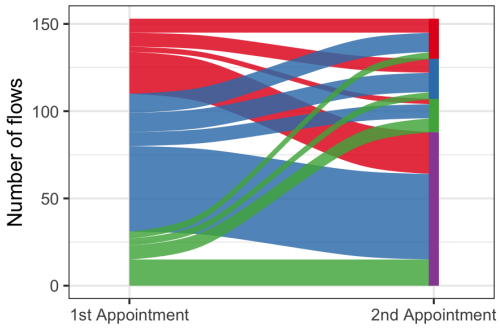
Figure A.15: Bureaucrat flows (by appointment & country effect)



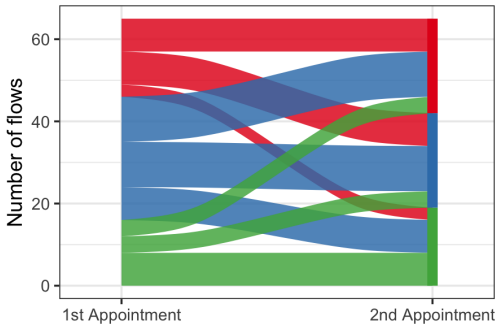
(a) All flows from 1st appointment.



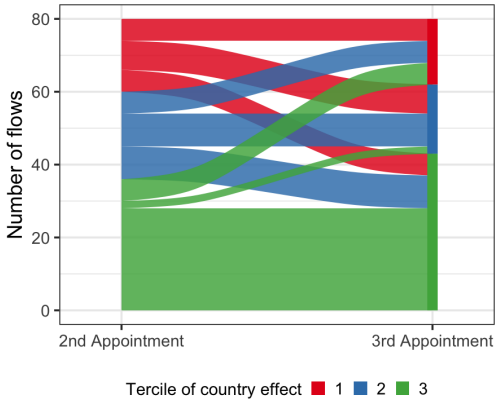
(b) Flows from 1st app. (no exit).



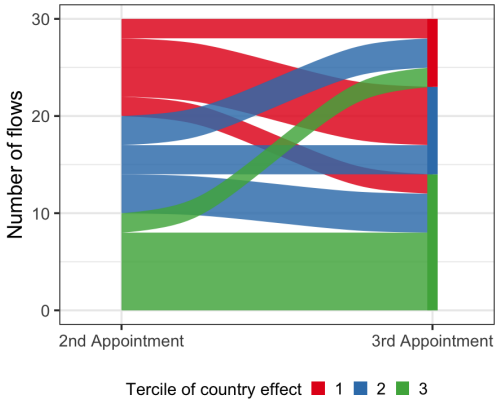
(c) Flows from 1st app. & bureaucrats without appointment before 1981.



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



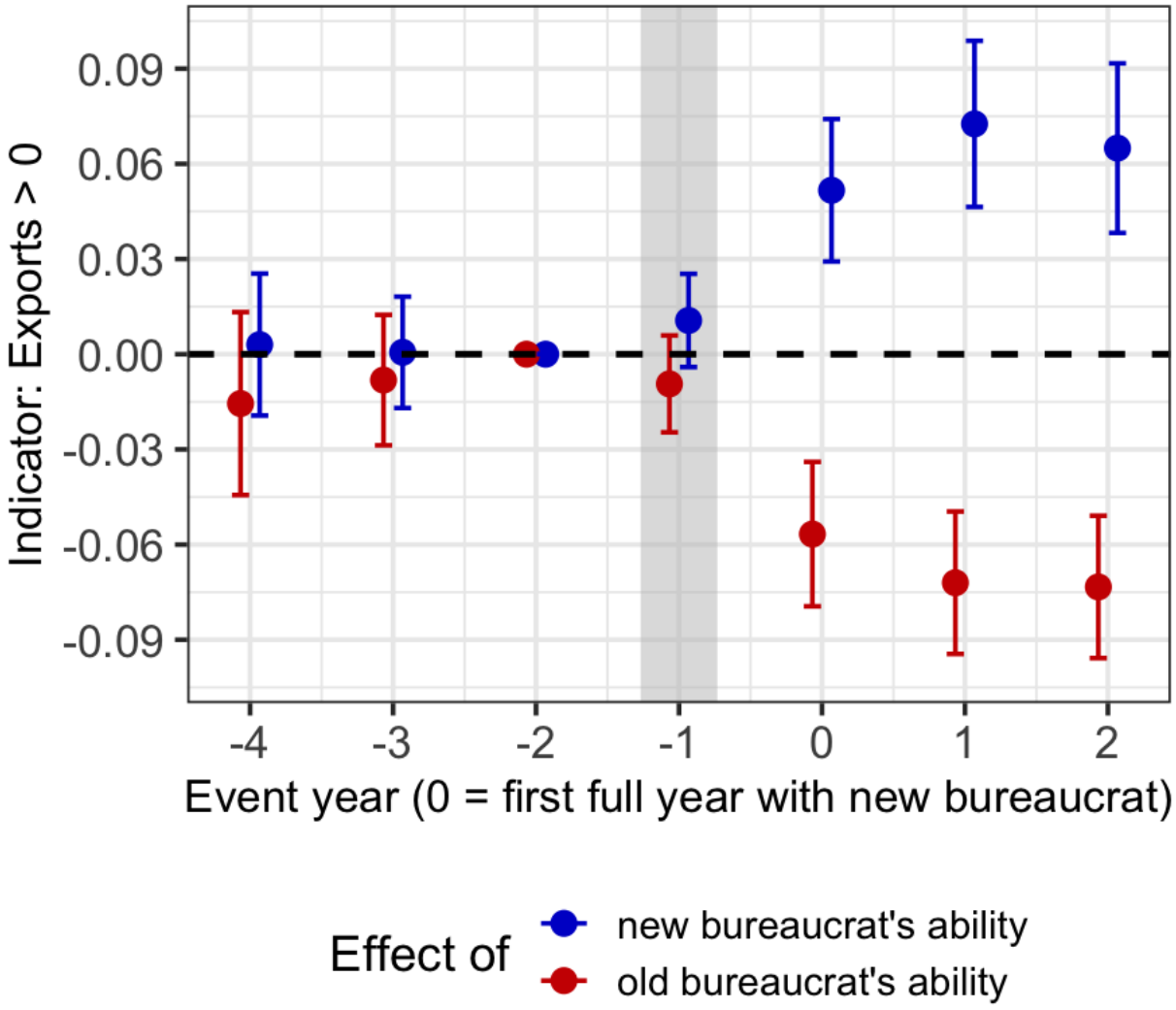
(e) Flows (except exits) from 2nd app.



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

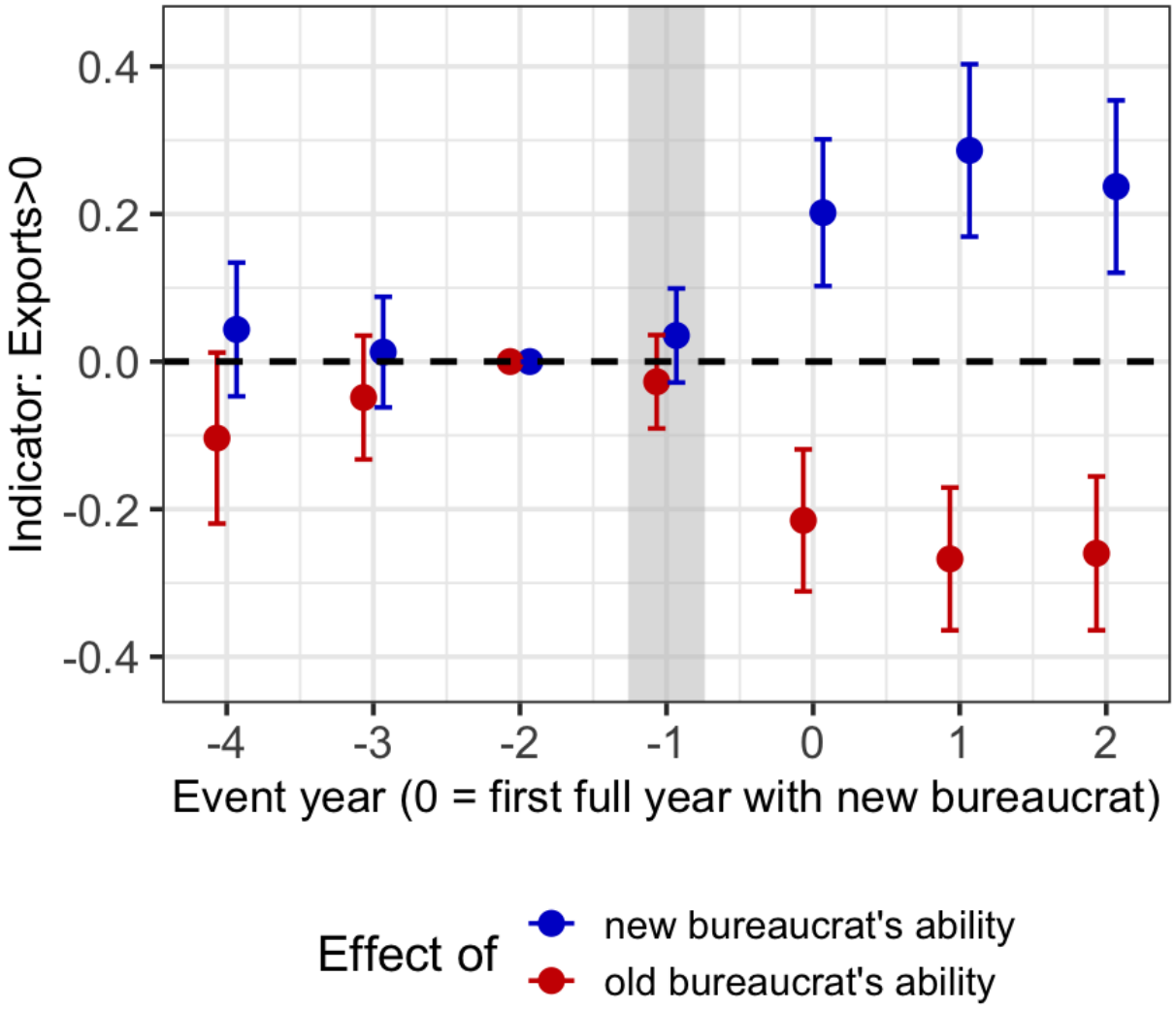
Notes: This figure shows the flow of bureaucrats from their 1st to 2nd appointment (2nd to 3rd in panels (e) and (f)). We split the offices into terciles based on the country fixed effects. The 3rd tercile consists of the most important countries according to this metric.

Figure A.16: Event study – the extensive margin response to switches between bureaucrats



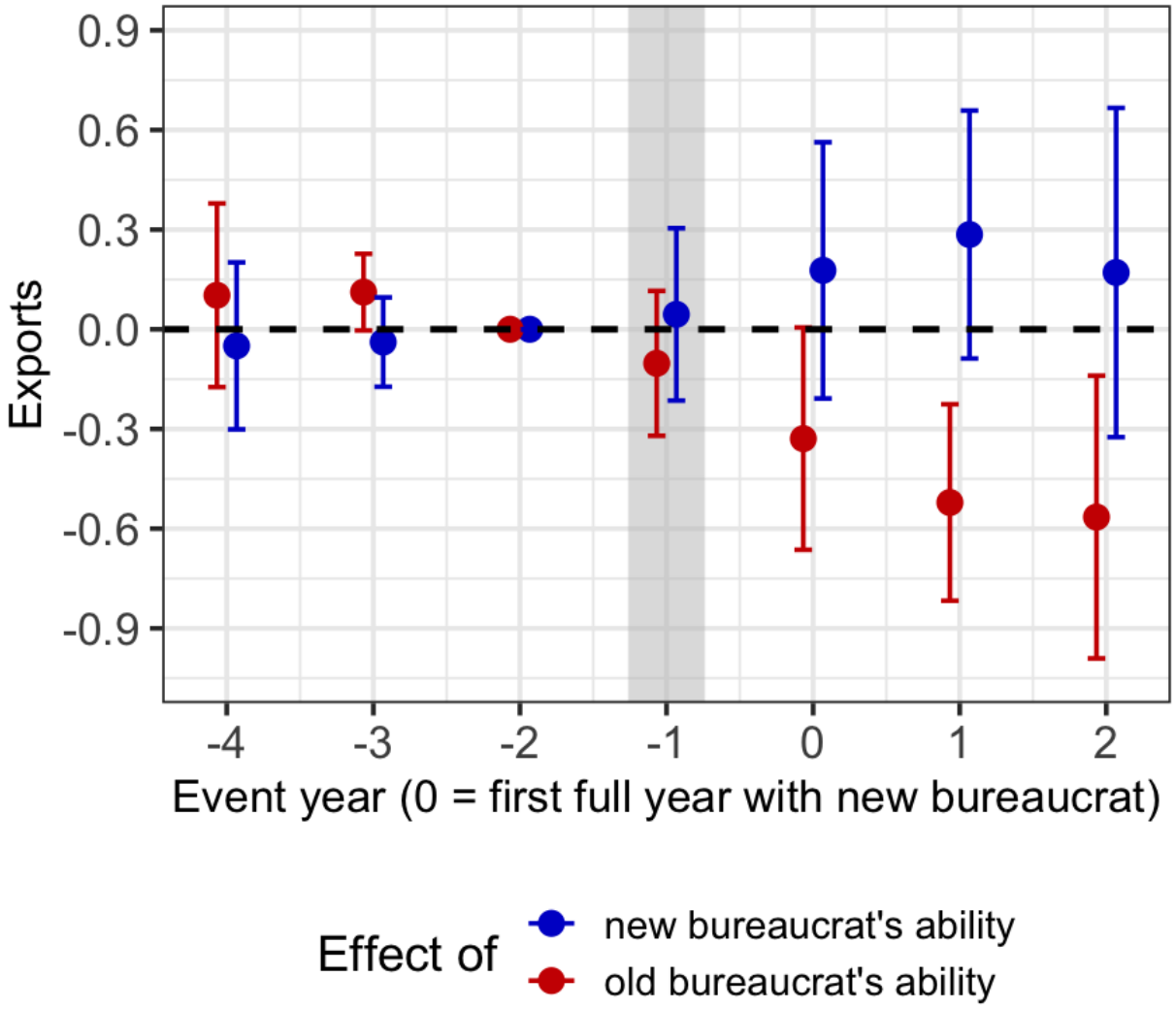
Notes: The figure shows the estimated effect of the change in bureaucrat fixed effects on the likelihood of positive exports in a given product around the time that the manager of a country office changes. These estimates are $\hat{\beta}_k$ and $\hat{\delta}_k$ obtained from estimating equation (6). 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.17: Large extensive margin response to bureaucrat effects for products with any change in extensive margin during event horizon



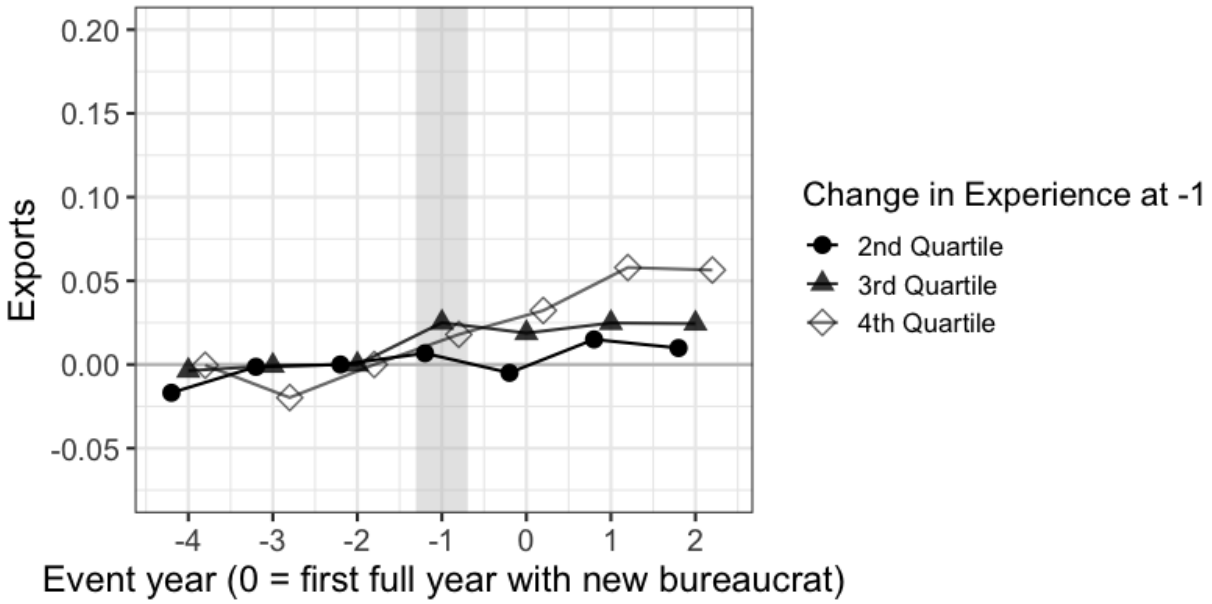
Notes: The figure shows the estimated effect of the change in bureaucrat fixed effects on the likelihood of positive exports in a given product around the time that the manager of a country office changes. These estimates are $\hat{\beta}_k$ and $\hat{\delta}_k$ obtained from estimating equation (6). 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.18: Event study – the intensive margin response to switches between bureaucrats



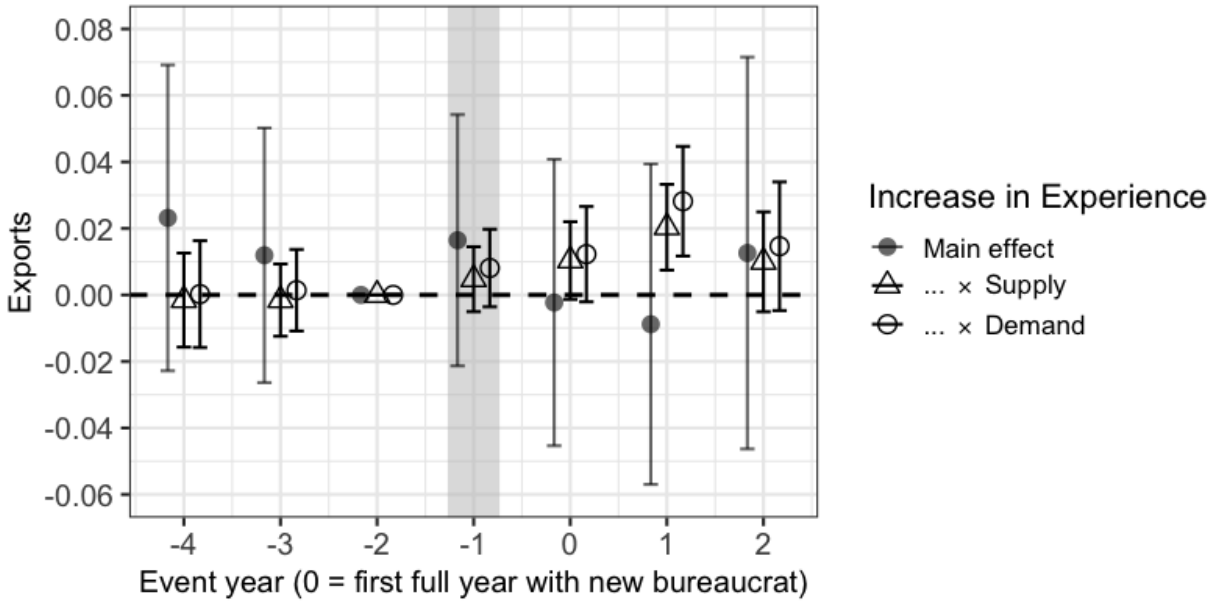
Notes: The figure shows the estimated effect of the change in bureaucrat fixed effects on exports around the time that the manager of a country office changes. This only includes the intensive margin effect as observations are included for a given event-horizon if South Korea exports this product to this country in all years during the event horizon. These estimates are $\hat{\beta}_k$ and $\hat{\delta}_k$ obtained from estimating equation (6). 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.19: 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.

Figure A.20: 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.

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	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 (2) 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 Robustness of Office Opening Results

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

This section investigates concerns that the results in 4 may be influenced by the choice of excluding events occurring before 1967. Appendix figure A.4 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 4. 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).

C.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.5 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.5 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.7 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.

C.3 Extensive Margin

This section investigates whether the results in figure 4 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 (1) 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.6 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 4. Again, the pre-treatment coefficient is very close to zero, corroborating that the parallel pre-trends in figure 4 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 C.2 applies here.

D Further Diagnostics of Variation Explained by Bureaucrats

D.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.10 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.[65] Table B.2 reports the variation explained by the different levels of fixed-effects when estimating equation (2), which identifies the causal effect of the

[65] Both of these points, especially (2), also constitute a reason to test whether bureaucrat fixed effects are predictive out of sample (see section 5.4.2).

two sets of fixed effects under the assumptions discussed above.[66] Adding bureaucrat fixed effects increases R^2 by 0.018, about 18.6% of the increase in explanatory power from adding country fixed effects.[67] To understand whether bureaucrat effects differ between appointments, we 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.11 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.11 finds that the main takeaways from figure 9 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

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

[67] 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 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).

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).

D.2 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.16 reports the event study estimates of bureaucrat effects estimated from equation (6) 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.17 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.[68] Appendix figure A.18 replicates figure 9 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 9. 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.

E Connected Set and Leave-One-Out Connected Set

Appendix figure A.8 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 (2) 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

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

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.8, 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).

F Robustness of Appointments by Country Importance

This section conducts robustness checks for the finding that bureaucrats are appointed to the least important countries during their first appointment, and to the most important countries during their third appointment, 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.13 displays similar results when measuring a country's importance as a market for South Korean exports by its fixed effect based on equation (4). 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.14 and A.15 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.[69]

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 alternative 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.

[69]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.