Bureaucrats as Drivers of Korean Exports

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Job Market Paper

August 17, 2023

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Abstract

Arguments that East Asia's growth miracle was enabled by industrial policy often highlight two key reasons why its industrial policies were successful: state capacity and export orientation. We study South Korean bureaucrats directly tasked with promoting exports while appointed to one of 87 countries between 1965 and 2001. We show that individual bureaucrats matter greatly in boosting exports, providing the first evidence that the bureaucrats who implement an industrial policy are crucial for its success. First, we use the staggered roll-out of offices to show that opening an office is associated with a 52% increase in exports. Second, exploiting the rotation of bureaucrats between these offices, we find that moving from a bureaucrat at the 20th percentile to the median, or from the median to the 90th percentile, is associated with a 50-60% increase in exports, respectively. Third, pursuing an IV approach, we find that exports in a product increase by 7.3% when the respective bureaucrat's product-specific experience increases by one standard deviation, suggesting that learning-by-doing can only partly offset the differences in fixed effects between bureaucrats. We identify a key mechanism to explain how bureaucrat capacity affects exports: better bureaucrats oversee a greater increase in exports when there is higher import demand for a product. This suggests one source of their effectiveness may be better transmission of information about market conditions.

1 Introduction

State and bureaucratic capacity are strongly associated with economic development (Besley, Burgess, Khan, and Xu (2022) - BBKX). Arguments that East Asia's growth miracle was enabled by industrial policy often highlight two key reasons why its industrial policies were successful: State capacity and export orientation (Juhász, Lane, and Rodrik, 2023).[1] Exporting is important for economic growth and development more broadly[2] and remains central to many sectoral industrial policies.[3]

In this paper, we show that individual bureaucrats matter greatly in boosting exports, providing first evidence that the bureaucrats who implement an industrial policy are crucial for its success. We study Korean bureaucrats directly responsible for exports to one of 87 countries during three-year overseas appointments between 1965 and 2001. As exports are an important development outcome, our setting allows us to push the frontier in research on bureaucrats and development by more closely linking the two (BBKX). Our main analysis uses a movers design in a two-way fixed effects framework, exploiting the regular rotation of bureaucrats to offices. Further, pursuing an IV approach, we find that exports in a product increase when the bureaucrat has more experience with this product. We also unpack the bureaucrat fixed effects to see how bureaucrat capacity affects exports. We find that better bureaucrats oversee a greater increase in exports when there is higher import demand for a product, suggesting one source of their effectivesness may be better transmission of information about market conditions.

We proceed first by estimating the effect of opening an overseas office, conducting an event-study estimation which uses the offices' staggered roll-out. We estimate an increase in exports of 51.8% compared to a never-treated control group after five years – similar to counterfactually moving London as close to Seoul as Delhi actually is. We are able to

[1] Narrative accounts of the rapid economic growth in East Asia emphasize the positive role of industrial policy and the development of capacity for carrying out complex policies, especially South Korea (Wade, 1990; Evans, 1995; Woo-Cumings, 1999).

[2] 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).

[3] Lederman, Olarreaga, and Payton (2010) report that more than 100 countries have an export promotion agency comparable to the Korean one. For evidence on the centrality of export promotion to sectoral industrial policy, see Juhász, Lane, Oehlsen, and Pérez (2022).

rule out the two most plausible alternative interpretation for the event-study findings: (1) Countries do not experience an increase in demand for non-Korean exports after an office opens. (2) The scope for strategic timing of office openings due to other reasons is limited as pre-determined variables explain the roll-out of offices across countries.

Second, we show that the effect of an office differs depending on the assigned bureaucrat by using a movers design in a two-way fixed effects framework that exploits the regular rotation of bureaucrats to offices. (1) Moving from the 20th percentile bureaucrat to the median, and from the median bureaucrat to the 90th percentile, increases exports by 50-60%, respectively. This is of the same magnitude as the effect of opening an office for the first time, i.e. as moving London as close to Seoul as Delhi is. (2) The negative fixed effect estimated for the 20th percentile bureaucrat indicates that an office opening has no effect under a 20th percentile bureaucrat. We provide event-study estimates that assuage concerns that our estimates of bureaucrat ability are not causal and conduct a variance decomposition that corrects for a limited mobility bias under heteroskedasticity (Kline, Saggio, and Sølvsten, 2020).

Third, we provide evidence that bureaucrat experience increases Korean exports. We find that exports increase by 7.3% when product-specific experience increases by one standard deviation, suggesting that learning-by-doing can only partly offset the differences in fixed effects between bureaucrats. To do so, we instrument for product-specific experience, addressing three main sources of endogeneity. The increase in exports is equivalent to moving London as close to Seoul as Berlin is.

For both greater bureaucrat fixed effects and bureaucrat experience, we show that the increase in exports is partly explained by an increased elasticity of Korean exports to market conditions – e.g., exports of a product to a country increase more strongly when there is increased import demand. This suggests that offices help relay information to potential Korean exporters, which is done more effectively by better bureaucrats.

We foremost contribute to understanding the bureaucratic determinants of state capacity (BBKX). We methodologically relate to research that finds substantial effects of managers and individual workers on the performance of organizations (Abowd, Kramarz, and Margolis, 1999; Abowd, Creecy, and Kramarz, 2002; Fenizia, 2022; Best, Hjort, and Szakonyi, 2023;

Otero and Muñoz, 2022; Metcalfe, Sollaci, and Syverson, 2023)[4]. We extend this literature to an area of great policy interest: How much do individual bureaucrats impact the success of an industrial policy? Can individual bureaucrats cause economic development, measured as changes in exports? Moreover, we shed light on a previously understudied mechanism for increasing state and organizational capacity by showing that bureaucrats gain capacity via learning-by-doing. As our bureaucrats are managers, this finding is informative about managers in other organizations.

We investigate the oft-hypothesized but under-researched link between state capacity and industrial policy (IP) (Juhász, Lane, and Rodrik, 2023). By studying an important determinant of IP success, we speak to current policy debates on the circumstances required for successful IP (Liu, 2019; Lane, 2022; Shim and Choi, 2022; Choi and Levchenko, 2021; Juhász, 2018). The importance of understanding the state capacity determinants of IP success is highlighted by the fact that IP is widespread across developing countries and even more common among developed ones, with export promotion (EP) often forming an important component (Juhász et al., 2022) . We provide novel evidence that EP moved in parallel with other components of HCI, Korea's most important industrial policy (Lane, 2022). Studying how state capacity affects export promotion provides a link to research on the determinants of firm performance in developing countries, specifically as these relate to demand-side shocks and EP more specifically (among many others: Atkin, Khandelwal, and Osman (2017); Alfaro-Ureña, Manelici, and Vasquez (2022), reviewed by Atkin and Donaldson (2022), Atkin et al. (2022); on EP, see Munch and Schaur (2018); Volpe Martincus and Carballo (2008, 2010, 2012)).

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 an office's effect depends on the bureaucrat in charge. Section 6 focuses on experience as one factor determining differential effectiveness between bureaucrats. Section 7 concludes.

[4]Further literature on bureaucracy

2 Institutional Background

Our study commences at a time when South Korea was one of the world's poorest countries. During our period of study, its GDP per capita increased from 1/19 (1961) to 1/3 (2001) of U.S. per capita income. This growth is prominently attributed to a well-functioning, activist state that conducted successful industrial policies (Wade (1990) and Cheng et al. (1998) as cited by BBKX; Amsden (1989); Juhász, Lane, and Rodrik (2023))[5]. This makes Korea an interesting case for understanding the role of state capacity in economic development broadly and the implementation of industrial policy, especially when considering that the Korean state was described as aid-dependent and corrupt until the mid-1960s (Kim and Vogel, 2011).[6]

Figure 1 highlights 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. This paper sheds light on this transformative growth in exports which appears particularly relevant given the centrality of exports to narratives of South Korea's broader economic miracle. Export promotion as a prominent area of state activism is highlighted by a representative survey of Korean manufacturers in 1976 (Jones and Il, 1980). These manufacturers who reported "foreign marketing" as the policy area where government intervention most markedly improved under President Park Chung-hee (1961-1979), compared to the Rhee administration (1948-1960).

[Figure 1 about here.]

2.1 KOTRA: Tasks and outputs produced

We study the overseas offices of Korea's Trade Promotion Corporation (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

[5] See also the well-known popular book by Studwell (2013).

[6] 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).

of public relations and advertising, [and] information service to exporters and importers" (Udell, 1965). Figure 2 displays the rise in the number of countries with an overseas KOTRA office. Offices opened in 28 countries until 1970, 68 until 1980, and 82 until 2000.

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) 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, which got published in KOTRA's Daily Market Newspaper. Business transactions were then mediated between the inquirers and respondents. Third, the overseas offices helped the "Trade fair" division with the organization of a 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 recruit, select, and brief exporters who will represent their products at the fairs. At the same time, they disseminate information about these exporters and their products to attract potential buyers to the 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 selected domestic companies often produced goods with newly trending styles and designs that matched the marketability of the venues of the fairs.

[Figure 2 about here.]

We extracted data that capture each of these three functions. This includes 80,000 market reports investigating export capability and import demand and 200,000 importer requests from 7,936 daily publications covering almost every weekday from 1965 to 2001. It further includes sales and attendance of firms at KOTRA organized trade fair pavilions ($\approx$ 13,000

firms attending 893 trade fairs).

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 main results focus on KOTRA's ultimate outcome of interest: exports. Clearly, it is hard to centrally plan whether exports to a specific 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' tacit knowledge and requires substantial improvisation. So rather than having a centrally mandated list of tasks to fulfill (as in Bandiera et al. (2021); Fenizia (2022); Best et al. (2023)), KOTRA office heads are somewhat more like the proverbial "man on the spot" of the Indian Administrative Service (Bertrand, Burgess, Chawla, and Xu, 2020) or the British colonial administration (Lugard (1926), as cited by Xu (2018)), charged with the running of an entire geographic region. However, compared to someone in charge of an entire region, KOTRA bureaucrats have a more narrowly defined target, that can largely be summarized into the measure of exports during their appointments. This makes studying KOTRA bureaucrats much less susceptible to the multi-tasking problem faced by studies evaluating the effectiveness of most bureaucrats wit regional responsibilities. Moreover, other than existing studies, EP bureaucrat's target, exports to the country of their appointment, is an outcome of direct importance for economic growth and development.

2.2 KOTRA: Assignment to overseas offices

Official rules do not dictate which bureaucrat gets assigned to which office, and the assignment system falls under the discretion of the HR Team. According to interviews we conducted with 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. Secondly, if one has worked previously at an office in a junior position, they might get assigned to the same office as a director in the future. Thirdly, given the 2.5- to 3.5-year rotation schedule, a bureaucrat is only sent to offices that become vacant at the right time. Fourthly, an officer who got 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 might matter for assignments to desirable locations.

Over the entire time preriod 1962 to 2001, KOTRA operated 138 overseas offices in 87 countries.[7] Most analysis will focus on the main country offices as outcomes are available at the country level. The regular nature of these managers' appointments is reflected by the fact that both the modal and median appointment duration is 36 months – three years. Figure 3 plots the distribution of appointment durations. Between appointments, managers spend time in KOTRA's headquarters in Seoul. 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 – 2.5 years. Figure 4 plots the distribution of gaps between appointments.

[Figure 3 about here.]

[Figure 4 about here.]

2.3 Export promotion and industrial policy

One reason for studying EP is the narrative of Korea's development as being export-driven, as well as EP's prominent role in Korean IP. Korea's largest scale IP, the HCI drive, commenced in early 1973 and ended in October 1979. When discussing whether a product was treated by HCI, we follow Lane (2022), 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.

To show the connection between EP and HCI, we linked about 45000 of the reports written by KOTRA's overseas offices between 1965 and 2001 to the products or sectors discussed by each report. Figure 5 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

[7] For example, by 1977, KOTRA had 79 overseas offices, of which 64 were the respective country's head office.

constant. This supports the view that export promotion was used as part of Korea's overall industrial policy.

[Figure 5 about here.]

3 Data

The explanatory variables largely derive from public announcement of KOTRA's personnel appointments. The main dependent variable, exports, comes from the widely-used World Trade Flows data base compiled by the UN and harmonized by Feenstra and Romalis (2014). This is complemented with firm-level export data we digitized and additional data regarding the three main functions of KOTRA's overseas activities.

3.1 Bureaucrat appointments

The most relevant source regarding bureaucrat appointments comes from contemporaneous newspaper reports on appointments to KOTRA's overseas offices. These have the advantage of denoting the exact date of the announcement. In most years, there were two main dates at which appointments were announced, usually in January and July. It should be noted that these announcement dates do not exactly correspond to the date at which the new director started their position. The actual start most frequently occurred in April and October. Further, this information is usually reported in three different newspapers (*Dong Ah Ilbo*, *Choson Ilbo*, and *Gyeonghyang Sinmul*). This means there are almost no rounds of announcements that we fully missed. For almost all rounds of announcements we were able to corroborate the information using at least two sources.

The data on appointments is further complemented and corroborated using a variety of data on the director in charge of an office at a given point in time. For the years 1966 to 1971, KOTRA updated the names of bureaucrats in a monthly publication aimed at non-Korean firms. For the years 1977, 1991-1994, and 1998-2000, we obtained and digitized a directory of KOTRA's network including all of its overseas bureaucrats. For the years 1987-1997, we obtained and digitized a full directory of all office directors using the Korean

Business Directory. For the years 1969 and 1971-1997, we extracted substantial information on all office directors that organized the Korean representation in a trade fair from KOTRA's reports on trade fairs.

Overall, we are able to identify 138 offices that existed between 1962 and 2001. These were located in 87 distinct countries. The number of offices increased greatly between 1962 and 1981 – see figure 2. From 1981, the number of countries with an office remained relatively stable. We identify 475 unique directors and 974 unique appointments of directors to offices. Table 2 provides further descriptive statistics regarding directors and appointments.

Directors are identified using their names, which requires us to avoid two types of errors. First, it may be that bureaucrats share names. For this or other reasons, we may code two bureaucrats as the same one. After a plethora of checks, it appears very unlikely that any bureaucrats in our data share the exact same name. More challenging in practice, we had to determine whether slightly different names truly corresponded to distinct bureaucrats. This task was 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. *yul* and *ryul*. We resolved this task in three steps: (1) Finding close matches of more common names to very unusual ones. (2) Standardize the rendering of certain syllables, e.g. *yul* and *ryul*. (3) Finding 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 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 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", "Fur clothing".

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

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 8000 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 there is a change in the identity of the head organizer from one bureaucrat to another. On average, the Korean representation was composed of 2-3 KOTRA bureaucrats, usually headed by the local office director, and 15 Korean exporters, making for approximately 34,000 bureaucrat-firm encounters. 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 office to identify the causal effect of the opening of a country's first office on the range of products Korea exported to this country. The staggered roll-out from seven countries with offices in 1965 to 67 by 1980 is displayed in figure 2, while figure 6 indicates the countries which had one or multiple offices in 1981, at the end of the main roll-out. This section estimates a 51.8% increase in exports 5-7 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 Delhi and Seoul.

[Figure 6 about here.]

This is of interest for two reasons. (1) While an increase of 51.8% is a sizable effect for any policy, this result shows that offices explain a modest share of the 50-fold rise in per capita exports Korea experienced relative to the U.S. (see figure 1). (2) The effect of an office provides a natural benchmark against which to compare the effect of an individual bureaucrat. Later sections find that the difference in fixed effects between a bureaucrat at the 20th percentile and the median, or between the median bureaucrat and the 90th percentile, are of a similar magnitude as the effect of opening an office. Moreover, an office with a bureaucrat at the 20th percentile has no effect on exports. Our setting is exceptional in allowing us to estimate such a benchmark from event studies of office openings. This is because (1) We observe a sufficient number of office openings and (2) Korean exports to a country are a well-defined variable in absence of an export promotion office.

4.1 Identification: Effect of Office Opening on Exports

To estimate the effect of an EP office, the ideal experiment would randomly allocate a fully-developed office to some countries and not to others. As this is not feasible for most organizations with an economically meaningful goal, the analysis here will use the staggered roll-out of offices to countries.

As a first step, we estimate the two-way fixed effects specification given by equation (1). π_{pt} indicates product-year fixed effects, γ_c indicates country fixed effects. D_{ct}^k indicates that the first office in country c opened k periods before t (or $-k$ periods after). Office opening is an absorbing treatment. θ_k is the variable of interest. It corresponds to the effect of the office after k periods. We compare countries with an opening event in the years 1966 to 1981 (ever-treated) to the never-treated: countries which never receive an office, i.e. no office until 1990. Equation (1) corresponds to a Diff-in-Diff specification because we normalize by the differences that exist between a treated country and all never-treated countries in period -1, the year before the office opens. For countries that do receive an office between 1966 and 1981, we include four years prior to the office opening and seven years after the office opening. The earliest start year for a treated country's event horizon is 1962. The latest end

year is 1988. Hence, for countries that do not receive an office, we include all years between 1962 and 1988.

$$y_{cpt} = \pi_{pt} + \gamma_c + X_{cpt}^T + \sum_{k \neq -1} \theta_k D_{ct}^k + \epsilon_{cpt} \tag{1}$$

We rely on two main assumptions to interpret $\hat{\theta}_k$ as estimating the causal effect of the office opening after k periods. (1) Parallel trends: 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 never-treated. Persistent level differences between the treatment and control group do not constitute a violation of this assumption. (2) No spillovers, i.e. an office affects exports only to the country in which it is located. More technically this is the stable unit treatment value assumption (SUTVA), as one unit's treatment value does not depend on other units' treatment. This would be violated if firms simply redirect exports from country c_1 to c_2 because c_2 has a KOTRA office. This would mean that EP offices do have an effect. However, we would overestimate the causal effect of treatment as the untreated suffer from a negative spillover effect. Alfaro-Ureña, Castro-Vincenzi, Fanelli, and Morales (2023) – ACFM – provide reason to believe a SUTVA violation of this type is not a first-order concern. ACFM assume that exporting to one country never decreases a firm's exports to another country. Under this assumption, they find that exports to different countries are complements. Hence, violations of SUTVA most plausibly lead to underestimates. We explore this further in the robustness section. A third assumption – no anticipation – is required for identification. This would be violated if office openings have a causal effect, at $k < 0$.

Our main specification uses the inverse hyperbolic sine of Korean exports as the outcome variable and does not include a control variable (X_{cpt}). Later specifications will control for non-Korean exports or, alternatively, use the inverse hyperbolic sine of non-Korean exports as the outcome.

4.2 Results: Effect of office opening on Korean exports

Figure (7) reports the estimated effects of the first overseas EP office in a destination country around the year of the office opening. The point estimates in the pre-period are informative about the validity of the parallel trends assumption. These are economically small and not statistically distinct from 0. This assuages concerns that the parallel trends assumption is violated. Figure (7) 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 does not materialize until the point estimates stabilize starting around year 5. The average point estimate in years 5-7 is 0.417, suggesting exports are 51.8%[8] higher relative to the control group.

[Figure 7 about here.]

Assuming an elasticity of -1 of trade to distance, an office opening has an effect similar to reducing the distance between London and Seoul to the existing distance between Delhi and Seoul. Alternatively, an office opening makes a country with a fixed effect at the 25th percentile (Ecuador) as attractive as a country at the 50th percentile (Greece).[9] At the same time a country at the 50th percentile (Greece) becomes as attractive as a country at the 75th percentile (Spain) due to an office opening.

4.3 Robustness checks: Effect of Office Opening on Exports

First, we consider that export promotion offices may be opened strategically in years when a destination country experiences increases in import demand. We address this concern in two ways. First, we re-estimate equation (1) while controlling for non-Korean exports to a country (also transformed as the inverse hyperbolic sine). Figure 8a shows that the estimates from this specification are largely unchanged compared to the baseline. Second, instead of Korean exports we use non-Korean exports as the dependent variable. The coefficients from

[8] $lim_{x\to\infty} sinh(x + 0.417)/sinh(x) = 1.518$

[9] Percentiles are calculated for those countries that ever have an office.

this regression are reported in figure 8b. It shows that opening export promotion offices does not coincide with statistically significant effects regarding this placebo outcome. If anything, there appears to be a slight negative trend. While this could theoretically be caused by increased competition from Korean exporters, this is unlikely, given the minuscule share of Korean exports to any country for most of the period under study.

To further assuage concerns about violations of our identifying assumptions, we report results using different control groups. Figures 8c and 8e use a "not-yet-treated" control group instead of the "never treated" used by our main estimation strategy. These figures report estimates following the estimator proposed by Callaway and Sant'Anna (2021), which allows for consistent estimates in cases where our main TWFE approach fails.[10] Figure 8c is estimated using the same sample of countries as figure 7. Like our main specification, we obtain positive and significant point estimates in every post-period. These are smaller than the estimates following the TWFE approach, although the confidence intervals from both approaches almost always overlap. Using the not-yet-treated control group raises our confidence in the parallel-trends assumption (PTA) as the treated and not-yet-treated are likely more similar, and experience more similar shocks than the treated and never-treated. On the other hand, for the same reason, SUTVA may more plausibly be satisfied with a never-treated control group. We indeed find smaller estimates when using the not-yet-treated control. This could be true if exporting to country c_1 is a complement to exporting to country c_2 (ACFM), and this is more strongly the case between treated and not-yet-treated countries than between treated and never-treated. We explore this further by estimating treatment effects using only European countries. These are reported in figure 8e. Point estimates remain positive for this sample but substantially smaller. The average point estimate in years 5-7 is 0.19. To interpret the difference, one should consider that this sample restriction focuses on a set of countries which are more likely to follow parallel-trends in absence of being treated as they are subject to similar shocks. On the other hand, ACFM find especially strong complementarities in exporting to European countries. So we may simply have confirmed that there are substantial spillovers from starting to export to one European country.

[10]Further approaches based on de Chaisemartin and D'Haultfœuille (2020); Borusyak, Jaravel, and Spiess (2023) will be included in future versions of the draft.

Figure 8d replicates figure 8b using Callaway and Sant'Anna (2021) difference-in-differences estimator. Similarly, there is no indication of a positive correlation between export promotion offices and non-Korean exports.

[Figure 8 about here.]

Third, we show 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 is also time-invariant, they are absorbed in 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. 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. Within Europe, distance from Korea is relatively stable, so the main predictor for office openings from a gravity equation would be the size of each destination's market. To rule out that openings were timed based on counterfactual increases in Korean exports, we use 1962 non-Korean exports to a country to predict the year when a country would get its first office. We do so for all European countries where an initial office opened between 1966 and 1981. Table 1 shows that true and predicted opening dates often coincide exactly. For both predictions, only the inclusion of Ireland causes any prediction to differ by more than three years from the actual opening year.[11] When simply assigning the nth opening year to the n-ranked country, the predicted year falls within two years of the actual opening for all countries except the Netherlands and Belgium (three years). The predicted year remains within three years of each actual opening (except Ireland), even when omitting from the prediction the year of a country's own first office opening, which mechanically introduces some error into the prediction.

[Table 1 about here.]

[11] Ireland's first office opened six years after the last previous first opening in a European country.

4.4 Extensions: Openings, activity, and market conditions

First, we analyze how country-specific activities change around the opening of an office. We re-estimate equation 1. 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 related to the country. Figure 9 reports results for these three outcomes. For each outcome, the coefficients stabilize after a couple of years at around 1. This translates into roughly multiplying by 2.7[12] the annual number of reports and inquiries – from 8 to 21 and from 26 to 70.

[Figure 9 about here.]

We next explore one potential channel through which KOTRA offices could impact Korean exports. Do offices cause Korean exports to be more reactive to non-Korean exports? We test this by estimating equation 2.

$$y_{cpt} = \pi_{pt} + \gamma_c + X^T_{cpt} + \sum_{k \neq -1} \theta_k D^k_{ct} + \sum_{k \neq -1} \theta^{\text{demand}}_k D^k_{ct} \; exports^{non-Korean}_{cpt} + \epsilon_{cpt} \tag{2}$$

Figure (10) reports the interactions and main effects obtained from estimating equation 2. It shows that there is an upwards sloping trend in the reactivity of Korean exports to non-Korean exports (demand). However, the upwards sloping trends is present before the office opening and it is unclear whether there is a causal effect due to the office opening. Moreover, the main effects of office openings are not very different from figure 7. Overall, this suggests that relaying information about market conditions is not central to the effect of office opening reported in figure 7.

[Figure 10 about here.]

[12] $lim_{x \to \infty} sinh(x+1)/sinh(x) = 2.718$

5 Office effectiveness depends on bureaucrat

This section shows that the effect of an office differs substantially depending on the assigned bureaucrat. It finds the following two results: (1) The negative fixed effect estimated for the 20th percentile bureaucrat indicates that an office opening has no effect under a 20th percentile bureaucrat. (2) Moving from the 20th percentile bureaucrat to the median, and from the median bureaucrat to the 90th percentile, increases exports by 50-60%, respectively – as much as opening an office for the first time, i.e. as moving London as close to Seoul as Delhi is. We conduct a number of robustness checks that assuage concerns that our estimates of bureaucrat ability are not causal. We also correct for a limited mobility bias under heteroskedasticity (Kline, Saggio, and Sølvsten, 2020).

To obtain these results, we adapt the AKM framework to study how much bureaucrats matter in explaining Korean exports (Abowd, Kramarz, and Margolis, 1999; Abowd, Creecy, and Kramarz, 2002; Fenizia, 2022; Best, Hjort, and Szakonyi, 2023). We exploit the rotation of office directors across countries to estimate bureaucrat and country (office) fixed effects. After correcting for limited mobility bias, we find that the SD of bureaucrat fixed effects is about 3/4 of the effect of an office opening, or moving London 3/4 of the way towards Delhi. Comparing bureaucrats and countries, bureaucrats explain about 1/7 as much variation as countries. As our bureaucrats are managers within the same organization, our paper closely relates to Metcalfe, Sollaci, and Syverson (2023). Of course, there are many omitted factors influencing Korean exports, plausibly much more so than regarding procurement prices, but not necessarily more than regarding wages or firm performance.

[Table 2 about here.]

5.1 Identification: Office effectiveness by bureaucrat

Table 2 describes the structure of our sample. Column 1 reports the statistics for the full sample of offices. Column 2 restricts attention to the main country offices which will be used to explain country-level outcomes, mainly exports to a destination country. The sample for the country-level analysis contains 397 directors and 86 countries. 194 directors move across

countries, and 82 countries experience multiple directors[13] The remaining 4 countries do not contribute to the estimation of the bureaucrat effects. All 86 countries and 397 countries in sample (2) are part of the same connected set. This is because our data covers 40 years of an organization with the average office experiencing nine different office heads.

We model the inverse hyperbolic sine of exports, 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 (π_{pt}), a bureaucrat component ($\theta_{b(c,t)}$), a country component (γ_c), and an error term (ϵ_{cpt}).

$$exports_{cpt} = \pi_{pt} + \gamma_c + \theta_{b(c,t)} + \epsilon_{cpt} \tag{3}$$

Note that equation 3 corresponds to an ihs-linear regression specification. This approximates a log-linear regression equation, but allows for the data containing zeroes. As in other parts of the paper, we aim to explain exports at the product-level. This avoids that results for a country-year are driven by a couple of dominant export products, and increases statistical power.

We aim to estimate fixed effects for the country and the individual. The variation captured by bureaucrats is interpreted as explained by the individual. The country captures the time-invariant effect of the individual EP office - similar to the existing literature. But, it additionally captures the time-invariant component of demand by this country for Korean products.

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

Identification requires that director mobility is as-good-as-random, conditional on product-year and country fixed effects. These orthogonality conditions allow for director assignment to offices on the basis of the permanent component of country exports α_c or the permanent

[13]This 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.

component of director ability $\theta_{b(c,t)}$. That is, sorting of better bureaucrats to destinations with greater time-invariant Korean exports would not violate the identifying assumptions. Neither would managers that are on average better across products sorting to destinations with greater average demand across products.

For the variance decomposition, we simplify equation (3) by removing the effect of product-year dummies from the value of exports to obtain $(exports|pt)_{cpt}$ according to equation (4), where $\hat{\pi}_{pt}$ is estimated from equation(3).[14] $\hat{\pi}_{pt}$ is likely important, as underlined by table 4, but unrelated to individual offices or bureaucrats. $\hat{\pi}_{pt}$ captures macroeconomic shocks, but also long-run changes in Korea's industrial structure. E.g. compare $\hat{\pi}_{cars,1965}$ (tiny) and $\hat{\pi}_{cars,1995}$ (large).

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

We correct for limited mobility bias when reporting the variance decomposition according to equation (5). Limited mobility bias describes a spurious negative correlation between the two dimensions of fixed effects in a two-way fixed-effects specification, arising when the switches across groups occur too infrequently. The bias correction is necessary when bureaucrats (offices) each are observed with only a very small set of offices (bureaucrats). In our setting, this is true for bureaucrats, but less so for offices which are observed with nine different bureaucrats on average.

$$\text{Var}[(exports|pt)_{cpt}] = \text{Var}(\theta_{b(c,t)}) + \text{Var}(\gamma_c) + 2\text{Cov}(\theta_{b(c,t)}, \gamma_c) + \text{Var}(\epsilon_{cpt}) \tag{5}$$

We follow the bias correction method by Kline et al. (2020), which allows for unrestricted forms of heteroskedasticity via leave-out estimation of observation-specific error variances. As such, the sample is the largest leave-one-out connected set of countries, or the largest set of countries that remain connected after any given appointment spell of a bureaucrat is removed from the sample. Moreover, 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

[14] This follows Chetty, Friedman, and Rockoff (2014) who explain that to remove the effect of pt without biasing the bureaucrat effects θ and country effects γ, $\hat{\pi}_{pt}$ needs to be estimated using only within-bureaucrat and within-country variation.

of the residualized exports as the total variation.[15,16] 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.[17]

5.2 Results: Office effectiveness by bureaucrat

Figure 11 reports the cumulative distribution function of bureaucrat fixed effects obtained from estimating equation (4). The fixed effect for the 20th percentile bureaucrat, -0.449, roughly offsets the positive effect estimated for an office opening. Therefore, an office only creates positive exports to the extent that the bureaucrat in charge is better than the 20th percentile.

Among bureaucrats with more than one appointment, negative fixed effects are less common. Among these, the fixed effect for the 10th percentile bureaucrat, -0.415, roughly offsets the positive effect estimated for an office opening. This difference is further explored in section 5.5.

Moreover, the difference between the 90th and the 50th percentile (0.425) and the 50th relative to the 20th percentile (0.456) are associated with increases in exports between 50 and 60 %, similar in magnitude to the effect of an office opening. Hence, assuming an elasticity of -1 of trade to distance, replacing a bureaucrat at the 50th percentile with one at the 90th percentile increases exports as much as reducing the distance to the export destination from London to Delhi. The same goes for moving from the 20th to the 50th percentile.

[Figure 11 about here.]

Next, we report the results from the variance decomposition of equation (5). Columns

[15] 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 to those that are estimated on the uncollapsed, raw data.

[16] The variance of the raw (i.e. country×product×year-level) residualized exports is also reported in Table 3 for reference.

[17] 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.

(1)-(2) of Table 3 report our preferred variance-decomposition results. Bureaucrats explain around 14% of the variation in residualized exports after collapsing at the spell-level. The variation explained by bureaucrat fixed effects implies a standard deviation of the bureaucrat fixed effects of 0.32, roughly 3/4 of the 25-75 spread. Increasing bureaucrat ability by one standard deviation amounts to 3/4 of the effect of moving London as close to Seoul as Delhi actually.

Column (1)-(2) also highlight that bureaucrats are about 1/7 as important as countries. The negative correlation between bureaucrat and country fixed effects suggests that better bureaucrats work in smaller countries. Overall, after taking out time-trends, bureaucrat and country fixed effects explain 88% of the spell-level variation in exports.

Moreover, we perform a "placebo check" on the validity of the variance decomposition exercise when bureaucrat fixed effects 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, as they should.

To allay concerns that the fixed effects of single-appointment bureaucrats may suffer from aggravated overfitting[18] and therefore magnify the variation in bureaucrat fixed effects, we also report in columns (3)-(4) the variance decomposition results excluding them. The share of total variation in residualized exports explained by bureaucrats does drop to around 8%. 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. On the other hand, if it is correct that the lowest ability bureaucrats are endogenously not re-appointed, the somewhat smaller variation in ability among re-appointed bureaucrats is an interesting results. The fact that the variation in bureaucrat fixed effects is not any larger when including the single-appointment bureaucrats (columns (5)-(6)) than when excluding them (columns (7)-(8)) in the randomly shuffled data also supports the reliability of the preferred decomposition results of columns (1)-(2) that includes

[18]For a single-appointment bureaucrat, their fixed effect value equals the residualized export value to the country they were appointed to during their appointment, less the country fixed effect value of that country.

single-appointment bureaucrats.

[Table 3 about here.]

We report further results in table 4. While informative, these are subject to some of the criticisms addressed by the Kline, Saggio, and Sølvsten (2020) and Andrews, Gill, Schank, and Upward (2008) bias correction reported in table 3.

First, we show that bureaucrat fixed effects substantial explanatory power over and above country-product fixed effects. We do so by estimating equation (3), which identifies the causal effect of the two sets of fixed effects under the assumptions discussed above. Adding bureaucrat fixed effects increases R^2 by 0.097. This is on a similar order of magnitude as other recent papers, studying managers of organizations that process insurance claims (increase in R^2 of 0.11), or hospital CEOs (0.09). Next, we assess how much the effects of a bureaucrat differ across their appointments. For this we compare the explanatory power when including appointment fixed effects (columns (4)) compared to column (3) which assumes that bureaucrat and country effects are time-invariant. The increase in explanatory power from this is negligible, suggesting that bureaucrats' effects are relatively stable across appointments, which provides some support that the productivity of a bureaucrat-country match is well approximated by the linear combination of the bureaucrat fixed effect and the country fixed effect.

[Table 4 about here.]

5.3 Robustness: Bureaucrat FE are not spurious

We next obtain event study estimates which show that switches in bureaucrat effectiveness correspond to sharp changes in exports without corresponding pre-trends. Hence, the bureaucrat fixed effects are likely to correspond to each bureaucrat's causal effect on exports. Given the lack of pre-trends and the finding of sharp jumps around new appointments, it seems implausible that the fixed effects are driven by spurious correlations between bureaucrat appointments and time trends.

First, we statistically test the pre-trends and the effects of the change in bureaucrat-product fixed effects relative to the last year the old bureaucrat headed the office. To that

end, we estimate equation (6), which explains exports as a time-varying function of the fixed effects of the new bureaucrat ($\hat{\theta}_e^{new}$) and the old bureaucrat ($\hat{\theta}_e^{old}$). e indicates the event. It is uniquely defined by the country and the year of the event. Equation (6) 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}).

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

Figure 12 plots the event-study estimates (β_k and δ_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. Pre-trends are not statistically distinct from 0 and economically very small.

[Figure 12 about here.]

Next, figure 13 closely follows Fenizia (2022) and Best, Hjort, and Szakonyi (2023). It shows time trends in de-trended exports around the years when an office experiences a change in the director. It classifies switches into quartiles of effectiveness 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.[19]

Figure 13 corroborates the main takeaways from figure 12. First, exports change sharply, and in the expected direction, precisely when a destination switches to a less or more effective bureaucrat. The estimates suggest that when the effectiveness of a bureaucrat in a location switches from the highest to the highest quartile, exports increase by approximately 25% from period -2 to period 1. Upon a transition to the lowest quartile, exports decrease by a similar magnitude. When the incoming bureaucrat is in the middle two quartiles, the change in exports is roughly zero, although slightly more positive for the third compared to the second quartile. If the old bureaucrat was in the lowest quartile, exports also increase most if the incoming bureaucrat is in the highest quartile. They increase progressively less

[19]Since quartiles of effectiveness are determined from average exports during a bureaucrat's appointments, it is somewhat mechanical that the level of exports in years 0 and 1 relative to years -3 and -2 is positive (negative) if the bureaucrat effectiveness trajectory is positive (negative). However, causal effects due to the bureaucrat are the most plausible reason for the sharp pattern discussed in this section.

as the quartile of the incoming bureaucrat falls. Second, the figure shows little sign that exports are increasing in organizations that subsequently switch to a better bureaucrat, and vice versa. This suggests that drift in effectiveness and switches are uncorrelated. Third, the changes in exports associated with switching bureaucrats appear roughly symmetric.

[Figure 13 about here.]

A further set of checks assesses whether the fixed effects we estimate are also predictive out of sample. We find that this is the case, further allaying concerns about overfitting.

The most natural and most conservative way to do this, in our setting, is to only use *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. This comes at a cost. For a bureaucrat with n appointments, the out-of-sample FE are estimated on $n-1$ appointments. Nevertheless, our data is comparably well-placed to estimate such out-of-sample fixed effects. When estimating FE 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.

Figure 14 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 we get a coefficient of 0.52. This is very close to the coefficient found by Metcalfe, Sollaci, and Syverson (2023), who study managers of retail stores. However, their approach splits the sample into two periods pre-Covid and during Covid. That means they may still estimate a bureaucrats fixed effect from the same appointment in which they then try to predict performance which is never the case for us.

[Figure 14 about here.]

Further, figure 15 replicates figure 12 using out-of-sample, i.e. *other country*, fixed effects. Around a switch in bureaucrats it shows that incoming and outgoing ability still statistically significantly predict exports in the expected way even when ability is estimated only using other countries.

[Figure 15 about here.]

Overall, this section provides support to the interpretation tha bureaucrat fixed effects identify the causal impact of an individual bureaucrat on exports. Given the lack of pre-trends and the finding of sharp jumps around new appointments, it seems implausible that the fixed effects are driven by spurious correlations between bureaucrat appointments and time trends. Hence, this section confirms the findings from the variance decomposition.

5.4 Mechanism: Good bureaucrats increase responsiveness of exports to market conditions

We next investigate whether part of 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 in a destination to a more effective bureaucrat, Korean exports' elasticity to market conditions increases sharply. Our findings suggest that most of the effect of high ability bureaucrats comes from more effectively exploiting market conditions, e.g., by relaying information about destination market demand.

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". "Demand" is the short-hand for other countries' exports of the same product to the same destination. "Supply" is the short-hand for 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 ability of the incoming or outgoing 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 to market conditions relative to the last full year the outgoing bureaucrat was in the country that is due to the estimated ability of the new or old bureaucrat.

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

Figure 16 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 increase in the elasticity of Korean exports to market conditions. The elasticities to market conditions increase by 9-11 ppt in the incoming bureaucrat's ability. They decrease by a comparable amount in the outgoing bureaucrats fixed effects.

The point estimates for the effect of incoming and outgoing ability after the change in bureaucrat mostly remain statistically significant. They are, however, reduced to about 1/10 of their size in figure 12, suggesting that much of the effect of high ability bureaucrats is due to the increased elasticity of Korean exports to market conditions, e.g. by relaying information about local conditions (demand) and identifying opportunities based on market developments common to Korean exporters across destination markets (supply).

Figure 16 also is informative about pre-trends. The absolute values in the pre-period are always statistically insignificant at the five percent level and much smaller in absolute values than the estimates in the post-period.

[Figure 16 about here.]

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

5.5 Extension: Bureaucrat fixed effects and 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 17 reports the probability density function of residualized exports, splitting the sample by the 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 far from causal, this suggests KOTRA may decide not to re-appoint bureaucrats with a very low ability.

[Figure 17 about here.]

We next regress bureaucrats number of appointments on residualized exports during their first appointment, part of a bureaucrat's fixed effect used 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, most prominently: (1) The number of appointments depends highly on a bureaucrat's tenure at KOTRA. (2) Bureaucrats may differ systematically by their first year of appointment as an overseas office director. Within 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 head 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.

6 Bureaucrat capacity is shaped by experience

We saw in section 5 that a large share of the variation in Korean exports can be attributed to the directors of overseas export promotion offices. This raises the question whether the capacity of these bureaucrats can be built.

We follow an instrumental variable approach to estimate the causal effect of product-specific experience. We find that a one standard deviation increase in product-specific experience of a bureaucrat raises exports by 6-7 percent, about the same as moving London as close to Seoul as Berlin is.

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. It further contributes to the literature by showing that bureaucrat ability is not uni-dimensional, but differs across products, or dimensions of the policy space. One bureaucrat may outperform their peers in promoting exports of textiles, while being outperformed in promoting exports of cars.

6.1 Identification

This section discusses our strategy to identify the causal effect of product-specific experience β. We do so by estimating equation (8). As before, t indicates the year, c the country, p the 4-digit product, and $b(c,t)$ the bureaucrat assigned to country c in year t. $\text{Tenure}_{b(c,t),t}$ - is a dummy indicating that this is not b's first appointment as office head. Product-specific experience - $\text{experience}_{b(c,t),pt}$ - is a continuous variable that is determined by Korea's exports during a bureaucrat's first appointment, i.e. the exports to which a bureaucrat was exposed at the time of their first appointment. This measure of experience may be endogenous for several reasons.

$$\text{exports}_{cpt,b(c,t)} = \beta\, \text{experience}_{b(c,t),pt} + \delta\, \text{tenure}_{b(c,t),t} + \alpha_{cp} + \pi_{pt} + \epsilon_{cpt,b(c,t)} \tag{8}$$

We address three main sources of endogeneity to identify β. First, a bureaucrat's first appointment may be endogenous if they are strategically appointed based on existing exports to that destination. We rule out that experience is due to such strategic appointment, by subtracting lagged exports from our measure of experience. Doing so means that our measure of experience is not due to differences in exports (of product p) between countries that existed in the three years prior to a bureaucrat's first appointment. Our (not normalized) measure of experience is given by equation (9). $T_1(b)$ and $C_1(b)$ indicate the year and country of this

b's first appointment. As in the remainder of the paper, *exports* always refer to the inverse hyperbolic sine of exports. In our regression, we normalize this measure of experience so that its standard deviation equals one.

$$\text{experience}_{b(c,t),pt} = \sum_{k=0}^{2} \text{exports}_{p,b(c,t),C_1(b),T_1(b)+k} - \sum_{k=-3}^{-1} \text{exports}_{p,b(c,t),C_1(b),T_1(b)+k} \tag{9}$$

Second, to avoid that our measure of experience is endogenous to bureaucrat' actions during their first appointment, we instrument for experience as described in equation (10). This follows the same form as equation (9) but uses non-Korean exports to predict (Korean) exports according to equation (11). In our regression, we also normalize this measure so that its standard deviation equals 1.

$$\text{instrument}_{b(c,t),pt} = \sum_{k=0}^{2} \widehat{\text{exports}}_{p,b(c,t),C_1(b),T_1(b)+k} - \sum_{k=-3}^{-1} \widehat{\text{exports}}_{p,b(c,t),C_1(b),T_1(b)+k} \tag{10}$$

To calculate predicted Korean exports, we use contemporaneous non-Korean exports to the same product-country. This captures a country's overall import demand. To obtain the import demand relevant to Korean, this is normalized by the ratio of Korean to non-Korean exports of the same product to other countries in the same year to capture the country-invariant component of Korean exports.

$$\widehat{\text{exports}}_{cpt} = exports_{cpt}^{non-Korean} \frac{exports_{-c,pt}}{exports_{-c,pt}^{non-Korean}} \tag{11}$$

Third, bureaucrats' later appointments may be correlated with their experience gained during their first appointment. However, this is problematic only if bureaucrat appointments are endogenous to our instrumented experience, given by the differences across products in the change in non-Korean exports during a bureaucrat's first appointment. We follow two separate approaches to rule out such remaining correlation between experience and $\epsilon_{cpt,b(c,t)}$, e.g. due to strategic appointment. The main approach we follow is to include country-product and year-product FE. The first concern this addresses is given by a mechanical relationship between our measures of experience and exports due to secular changes in Korea's exports of certain products over time. If a bureaucrat is first appointed in 1968, they

gain more experience regarding the type of products that Korea was exporting in 1968 (e.g. textiles, not cars). This bureaucrat is more likely to be re-appointed in 1973 – when 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 FE.[20] Year-product FE further avoid spurious correlations due to the fact that Korean exports in later years are larger for any product or the fact that textiles always make up a larger share of Korean exports than do maize or crude oil.

The second concern we rule out is that bureaucrats may be re-appointed to countries with high demand for the products in which they are experienced. If product-specific experience matters, it seems intuitive this would be taken into account for re-appointments – even though our qualitative research suggests that product-specific experience is not a factor when deciding bureaucrat appointments. Further, for this to be an identification concern, re-appointment would need to take into account our instrumented measure of experience. As long as our instrumented measure of experience does not predict reappointments, a correlation between other components of a bureaucrat's experience and their re-appointments would not constitute a violation of our identifying assumptions. We include country-product FE to avoid attributing any effects to time-invariant country-product demand. They further avoid spurious correlations due to the fact that Korean exports to bigger importers are larger across all products.

To causally identify the effect of product-specific experience on exports, our identifying assumption is: Conditional on year-product FE and country-product FE, there is no correlation between our instrumented measure of product-specific experience and the error term: $\mathbf{E}[\text{instrument}_{cpt,b(c,t)}\mathcal{E} \mid \alpha_{cp}, \pi_{pt}] = 0 \,\forall\, cpt$. Time-invariant country(-product)-specific determinants of exports/appointments are ruled-out as a source of our results by the inclusion of country-product FE. Country-invariant year(-product)-specific determinants of exports and appointments are ruled-out by the inclusion of year-product FE.

To causally identify the effect of tenure on exports, we need to assume that there is no correlation between tenure and the error term: $\mathbf{E}[\text{tenure}_{ct,b(c,t)}\mathcal{E} \mid \alpha_{cp}, \pi_{pt}] = 0$. The foremost

[20] This concern is already largely addressed by our measure of experience as a first difference relative to the three years prior to a bureaucrat's appointment. We have replicated column (1) from table 5 without year-product FE and find almost no change in coefficients.

concerns is strategic appointment due to (in)experience, e.g. the appointment of experienced bureaucrats to countries or in years with greater Korean exports. An additional concern may lie in bureaucrats being posted to more desirable locations as their career progresses. As there is no exogenous variation in tenure, the assumptions to causally identify the effect of tenure remain quite strong. While the "effect" statistically attributed to tenure is of substantial interest, we consider it a correlation rather than causation.

We conduct two robustness checks. First, we add country-year FE, λ_{ct}. These rule out any effect common to all products exported to a destination in a country-year. Importantly, as there is only one bureaucrat assigned to each country-year, this also means all comparisons are within bureaucrat-year. Within bureaucrat-year comparisons preclude identifying the effect of tenure. This weakens the identifying assumption so that it only needs to hold conditional on country-year FE: $\mathbf{E}[\text{instrument}_{cpt,b(c,t)}\mathcal{E} \mid \alpha_{cp}, \pi_{pt}, \lambda_{ct}] = 0 \; \forall \; cpt$.

As a second robustness check, we use a more relevant instrument for product-specific experience. This instrument takes into account the ratio of Korean to non-Korean exports of other products to the same country in the same year. It thus accounts for the fact that Korean exports to a destination differ in importance across years. It is an even more relevant instrument, with an F-statistic about three times higher than the main instrument. However, it may be endogenous to the bureaucrat's overall ability.

6.2 Results

Table 5 reports the results from estimating equation 8. We find a strong positive correlation between exports and bureaucrat experience without including any country fixed effects. Exports are 11% higher with product-specific experience. We find a smaller, but significant, effects of both product-specific experience when estimating the full equation (8) (column 2) and when estimating the full 2SLS (column 5) which instruments product-specific experience as described in equation (11). Both approaches yield statistically significant positive effects of product-specific experience, although the point estimates differ (3.5% and 7.3%). Following both approaches, we find that exports are about 15% higher with bureaucrat tenure.

Overall, the findings supports the view that product-specific experience of bureaucrats increases Korean exports. There is also a positive association between tenure and Korean

exports. This strengthens and adds nuance to the findings from section 5: Bureaucrats matter in determining exports. More experienced bureaucrats are more productive. Taking seriously the larger effect of tenure suggests that bureaucrat learning is largely transferable across products. But there is a non-negligible effect of product-specific experience on exports.

[Table 5 about here.]

6.3 Robustness: Alternative instrument and overidentification test

This section discusses the results from estimating equation (8) under different sets of assumptions presented in table 6. Column (2) reports the results from the IV when weakening the assumption of strategic appointment by including country-year - and hence bureaucrat-year - fixed effects. We find a similarly sized effect on exports from product-specific experience as the preferred estimation (reproduced in column (1)).

Columns (3) and (4) report coefficients from the second stage of analogous IV approaches that replace our main instrument with a more relevant but somewhat less plausibly exogenous instrument that takes into account the ratio of Korean to non-Korean exports of other products to the same country in the same year. This leads to sustantially larger F-statistics, indicating the increased predictiveness of instrument 2. This gives rise to slightly larger and less noisy estimates of increases of exports of 0.097 and 0.073 per SD of product-specific experience.

Column (5) replicates columns (2) and (4) with both instruments. Including both instruments allows us to conduct an overidentification test. We are unable to reject the null-hypothesis that the instruments are valid. The point estimate is essentially unchanged between column (5) and (4).

[Table 6 about here.]

6.4 Extension: Experience increases the reaction to demand shocks

Both previous results rely on (weak) assumptions to rule out strategic appointments. This section moves beyond these assumptions by considering variation within appointment $\times$

product. We estimate equation (12) and show that bureaucrat experience increases the elasticity of Korean exports to demand shocks. This highlights 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 to Korean exporters or helping them effectively react to such shocks.

$$\begin{aligned} \text{exports}_{cpt,b(c,t)} = & \alpha \text{ demand shock}_{cpt} + \beta \text{ experience}_{b(c,t),pt} \times \text{ demand shock}_{cpt} \\ & + \delta \text{ tenure}_{b(c,t),t} \times \text{ demand shock}_{cpt} + \alpha_{cbp} + \epsilon_{cpt,b(c,t)} \end{aligned} \tag{12}$$

Identifying the causal effects of δ (and β) only requires the weakest of assumptions. A sufficient assumption is that appointing a new bureaucrat does not cause dynamic effects that are correlated with the dynamics of demand shocks between years during their appointment. This would be violated if the effect of bureaucrat experience in p is increasing over the course of their appointment, and bureaucrats with experience in p also face increasing demand shocks over their appointment. As a robustness check to rule out such a correlation we allow the experience in a product to have a differential effect by year within an appointment - i.e. in year 1 experience may have a different effect from year 2. Demand shocks are measured along the lines of predicted exports as given by equation (10).

Table 7 reports the results from estimating equation 12. The difference between the three columns is the measure of product-specific experience. Column (1) is the OLS, while columns (2) and (3) are reduced forms that directly include the instrument for product-specific experience. All three columns report the main effect of a demand shock on Korean exports. This demand shock is given by non-Korean exports to a destination scaled by the ratio of Korean to non-Korean exports of the same product to other destinations in the same year. All three estimations include appointment-product fixed effects. So the coefficient on "Demand Shock" gives the elasticity of Korean exports to variations in demand relative to the average for a given product during a given appointment. Across specifications, we find an elasticity of 0.194, i.e. when non-Korean exports to this product-destination increase by

10%, Korean exports increase by around 2%.

This effect is augmented by around 13% from a one s.d. increase in product-specific experience. The effect of product-specific experience is similar in size when regressing on the augmented instrument and slightly weaker when regressing on the main instrument.

[Table 7 about here.]

7 Conclusion

Our paper closely links individual bureaucrats to exports, a variable important to economic growth and development. We find that offices openings increase exports by 51.8%. The importance of bureaucrats is illustrated as this effect is entirely offset if the bureaucrat in charge is at the 20th percentile of effectiveness. Moreover, we show that there is some scope for bureaucrats to acquire capacity on the job. Our findings have important implications for debates on industrial policy and the role of state capacity in economic development.

First, the bureaucrats we study engage in the implementation of an industrial policy. Our finding thus imply that implementation matters substantially in determining whether an industrial policy is successful. This adds nuance to the resurgent debate on industrial policy. As we compare different bureaucrats who implement the same policy, our results highlights one important dimension of "how industrial policy should be carried out" instead of "whether governments should carry out industrial policy" (Juhász, Lane, and Rodrik, 2023). This focus on the "how" is especially pertinent as export promotion is a policy many governments choose to pursue, especially as part of a broader industrial policy (Juhász, Lane, Oehlsen, and Pérez, 2022).

Second, export promotion is quite distinct from the tasks studied by the existing literature on bureaucrats. Our paper highlights that there may be returns to developing countries who do not only build bureaucratic capacity domestically, but use it to support their firms as they navigate global markets. Further, tasks like export promotion that aim to identify and overcome frictions that constrain firm growth may not require Weber's impersonal bureaucrats following standardized processes, but something closer to an "entrepreneurial bureaucrat" (Mazzucato, 2013) making use of tacit knowledge.

Third, we find that our bureaucrats learn to promote exports of certain products when exogenously exposed to them. This suggests a potential path for building state capacity endogenously as bureaucrats acquire capacity as they are exposed to certain opportunities and problems (Hirschmann, 1958). However, it also points to potential path dependence in state 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.

Finally, our findings are informative regarding narratives about South Korea's "export-led" growth from one of the poorest countries in 1960. Our findings support this narrative in the sense that the bureaucrats we study are not alleviating supply-side constraints but strongly target the demand for Korean products abroad. In this sense, our study points towards a role for policy aiming to create growth that is led by demand for exports. It should be noted that for such a policy to be effective, bureaucrats have to be well-informed – embedded (Evans, 1995) – regarding domestic firms' export capacity.

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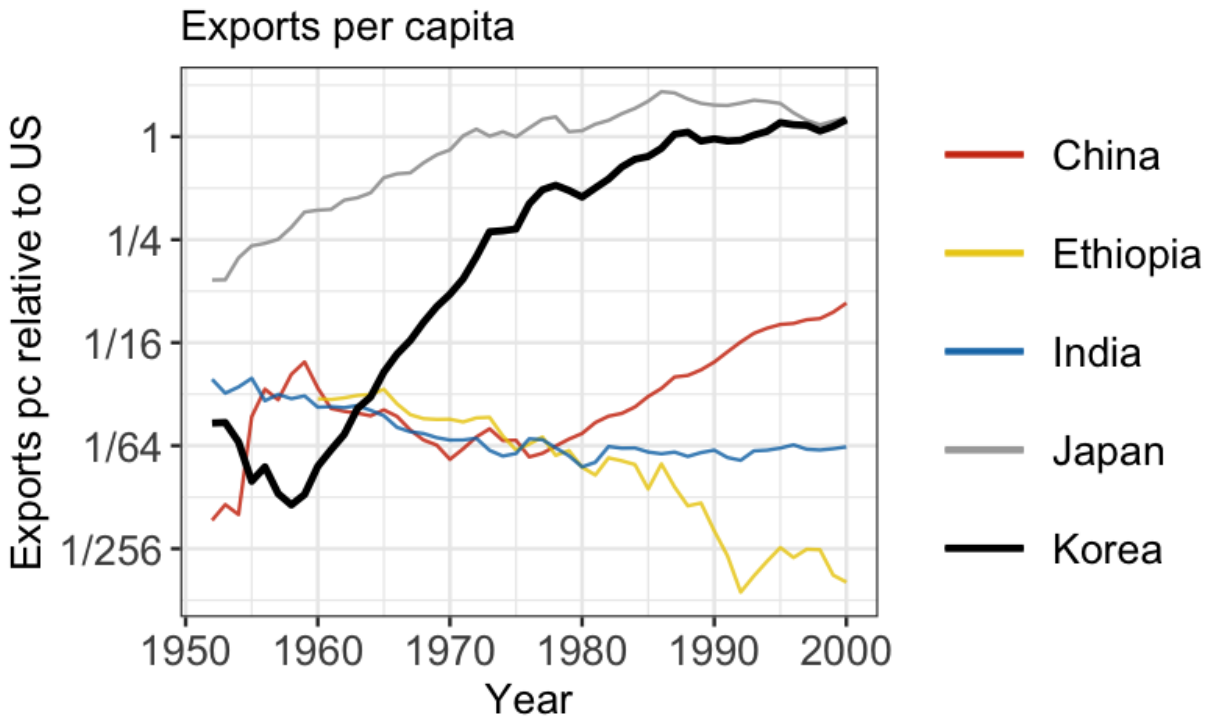
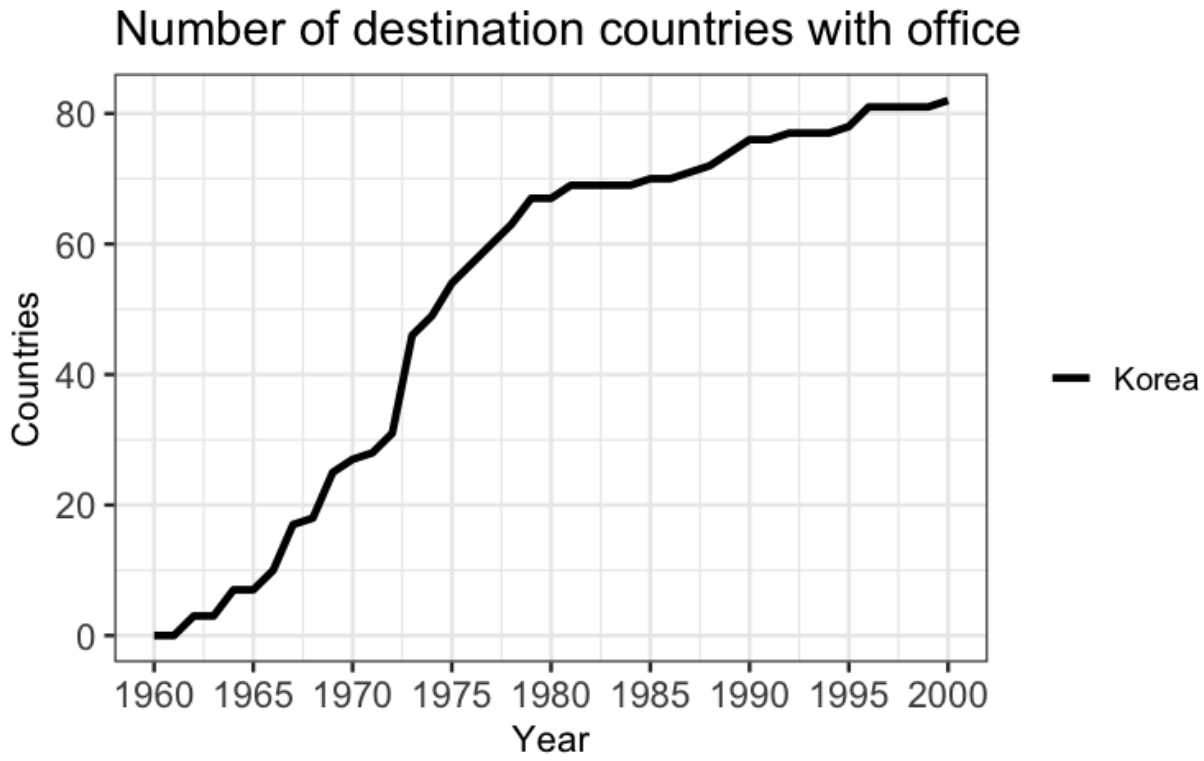
Figure 1: Growth in Korean Exports



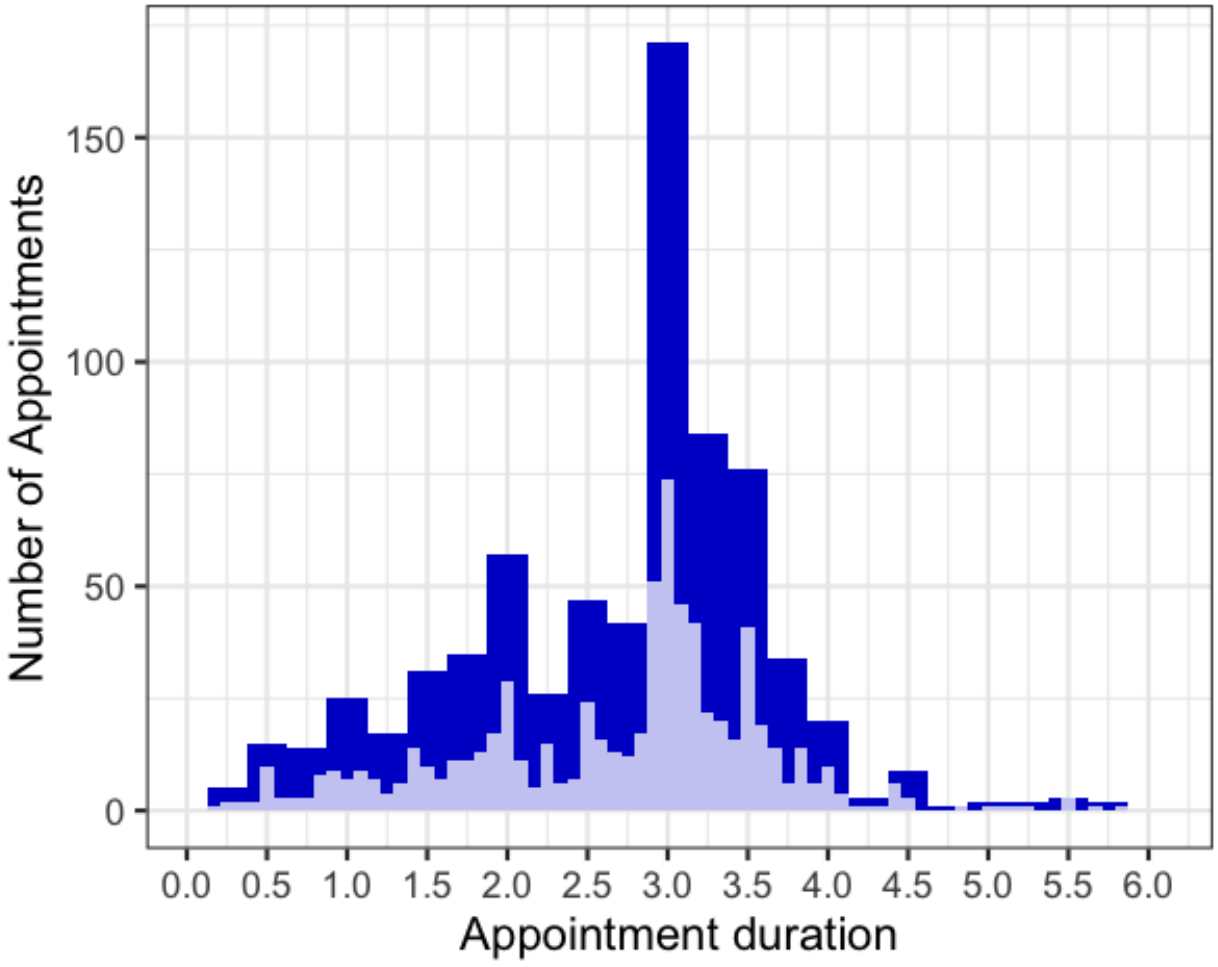
Notes: The figure displays Exports per capita relative to the U.S. the years 1952 to 2000 for Korea and a selected group of other countries. Data on exports and population obtained from International Monetary Fund (2023): Direction of Trade Statistics.

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



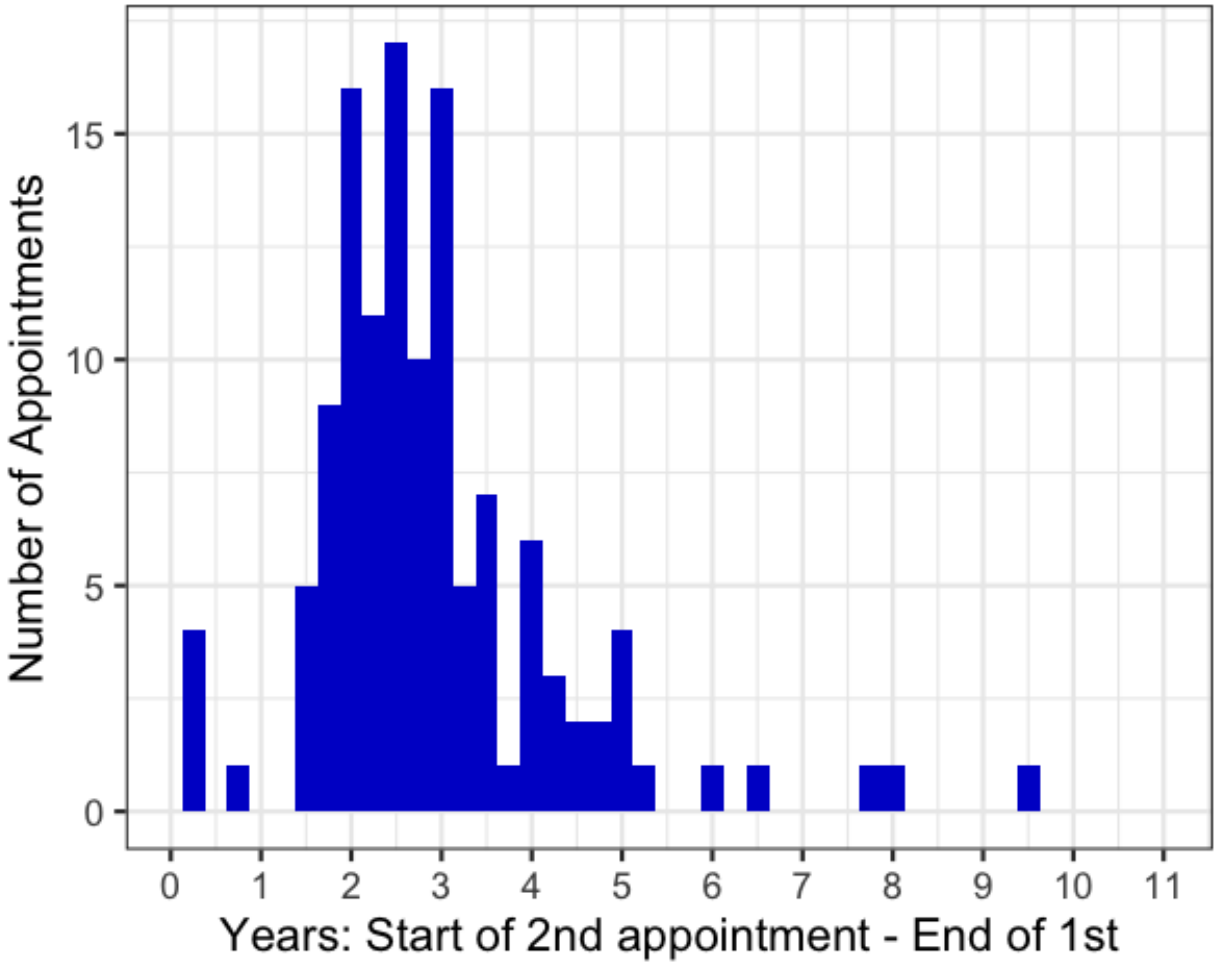
Notes: This figure presents the number of countries with an overseas export promotion office opening up until each year.

Figure 3: Distribution of appointment durations.
Median and modal duration: 36 months.



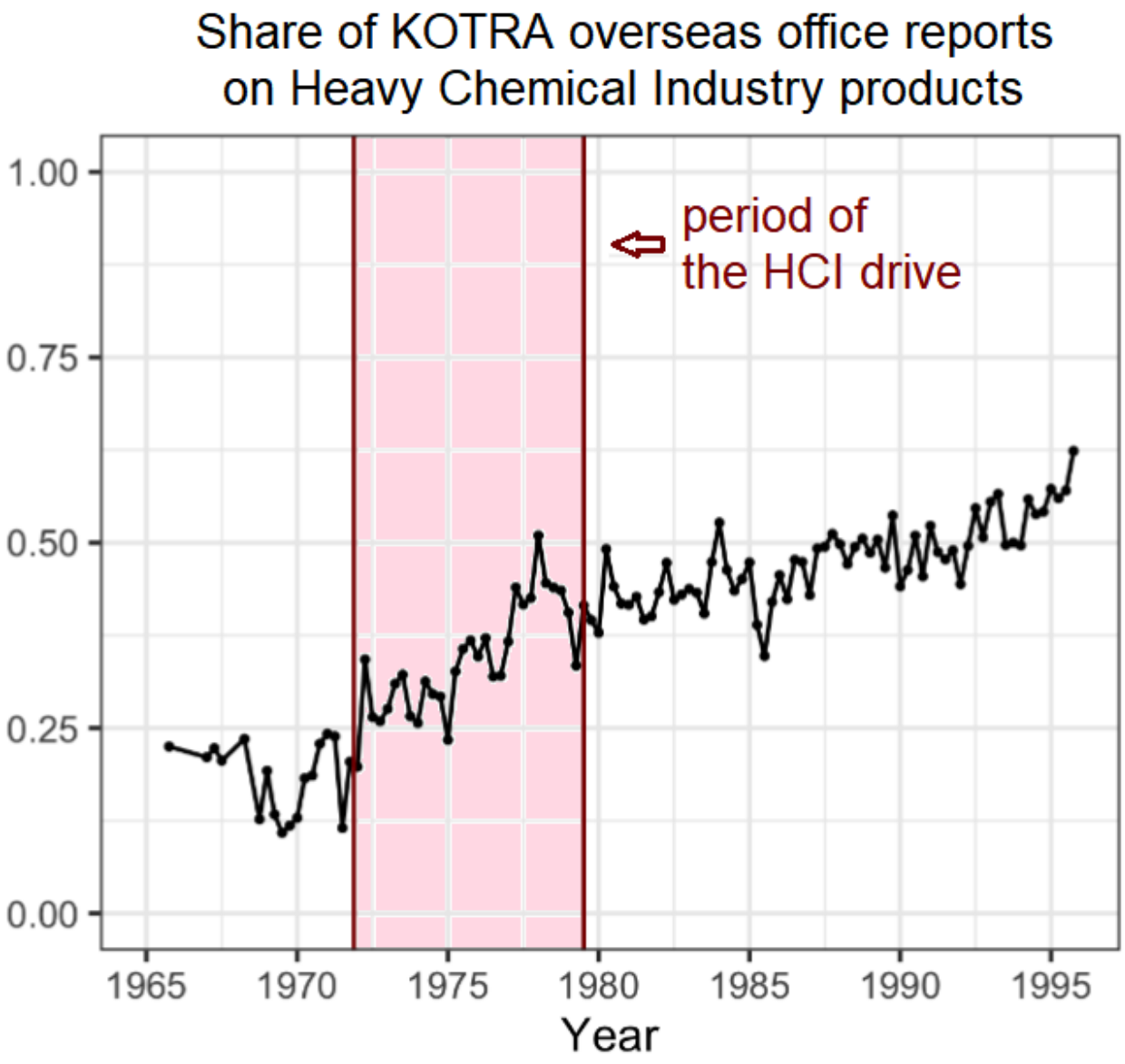
Notes: This figure represents the distribution of appointment durations. The blue bars indicate the number of appointments by quarterly duration whereas the white bars do so for the number of appointments by monthly duration. Hence, as each quarter contains multiple months, the blue bars always (weakly) 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 4: Distribution of gap lengths.
Median: 29 months. Mode: 30 months.



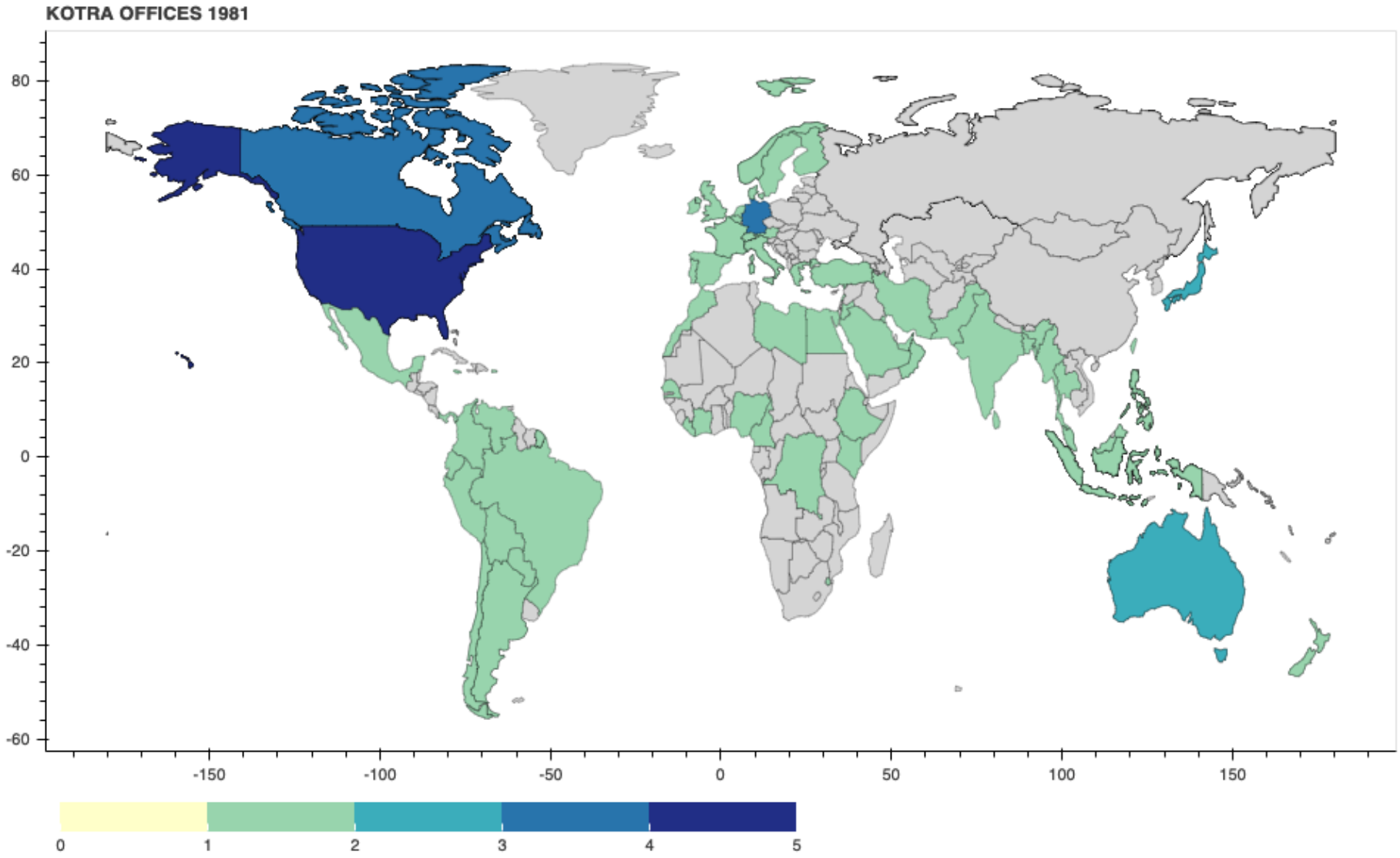
Notes: This figure represents the distribution of the duration of gaps between appointments. The blue bars indicate the number of gaps by quarterly duration.

Figure 5: Targeting of export promotion activity by product.
Export promotion activity moves in parallel with national industrial policy



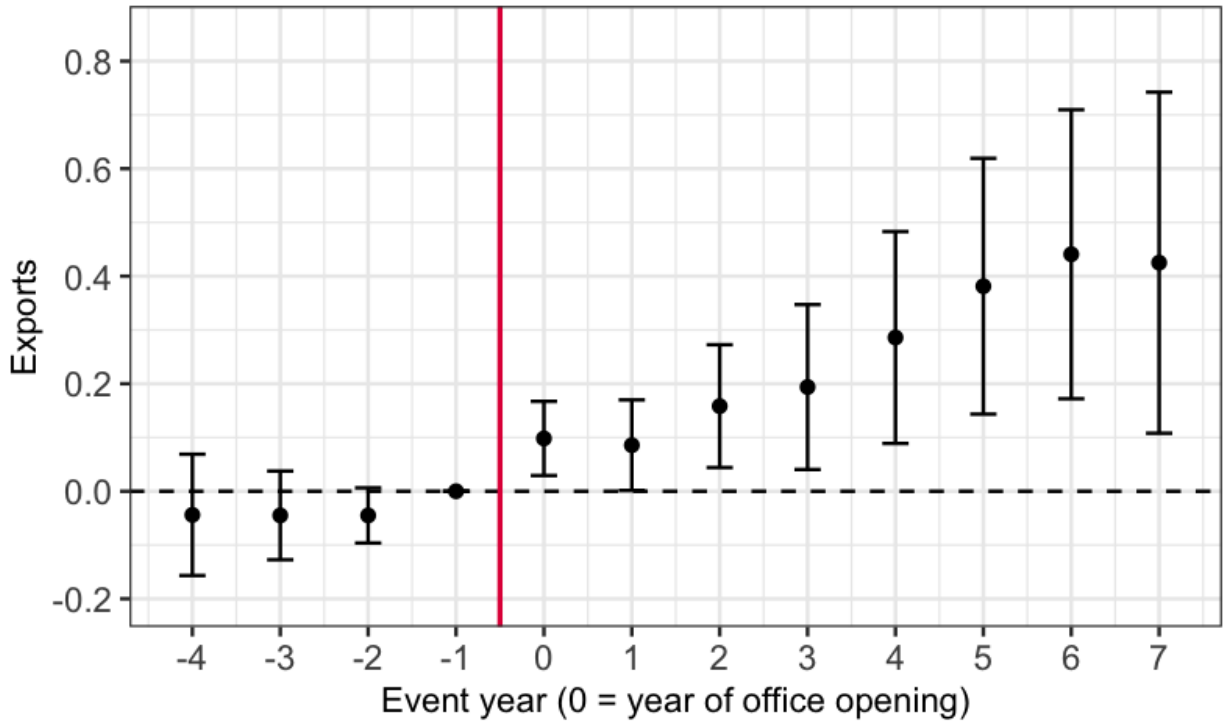
Notes: Targeting of EP activity by product. For each quarter, the y-axis presents the share of overseas office reports that could be linked to an HCI product relative to the number of reports that could be linked to any product.

Figure 6: The 71 countries with KOTRA offices in 1981.



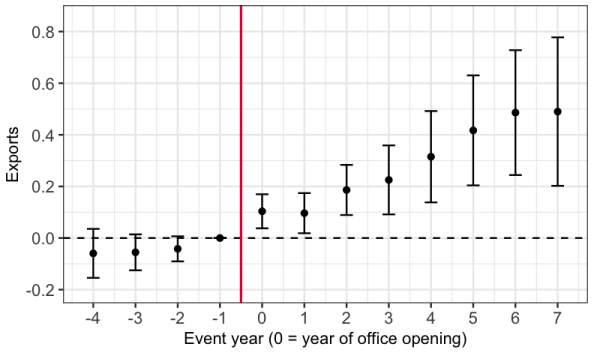
Notes: Colored countries have at least one offices. Different colors indicate the number of offices in a given country. Each office indicated was opened between 1962 and 1981. In 1981 the number of countries with offices reached a plateau, fluctuating between 67 and 71 until 2000.

Figure 7: Event-study estimates of the effect of office opening on Korean exports.

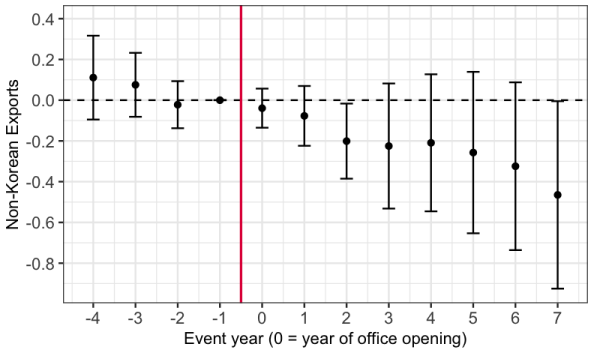


Notes: The outcome variable is the inverse hyperbolic sine of Korean exports to the country-year in question. An observation is at the product-country-year. Point estimates and standard errors are obtained from estimating equation (1). This relies on a never-treated control group. Standard errors clustered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

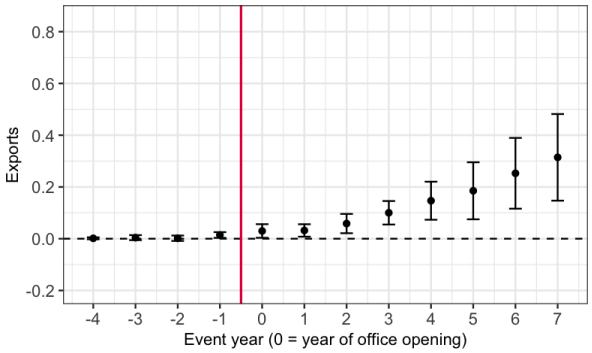
Figure 8: Robustness: Export Promotion works on average



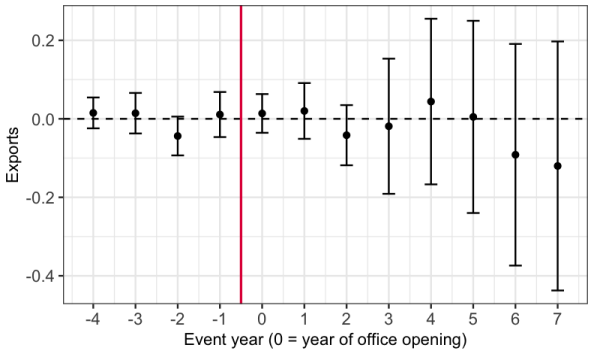
(a) Controlling for non-Korean exports.



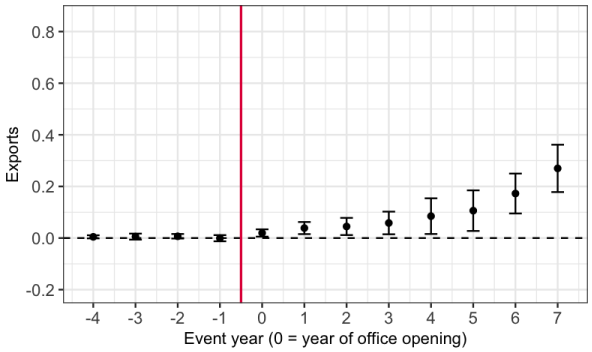
(b) Non-Korean exports as outcome



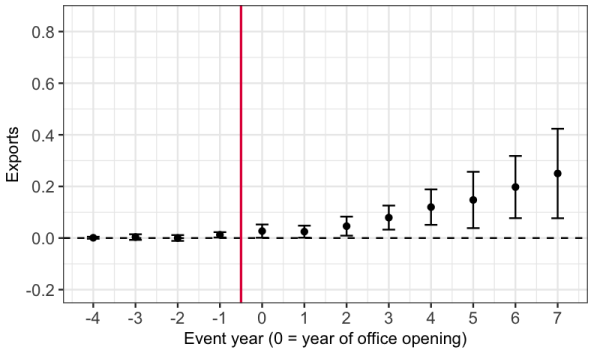
(c) CSA : "Not-yet-treated" control group



(d) CSA: Non-Korean exports as outcome



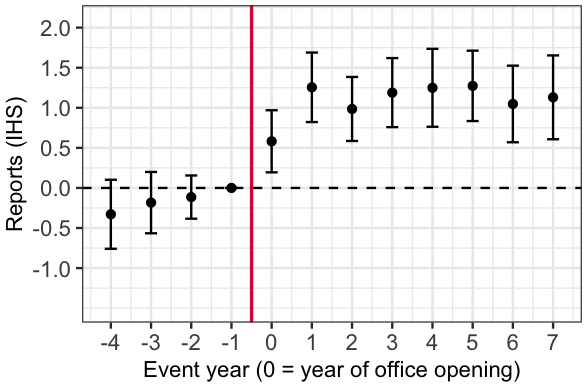
(e) CSA : Europe only



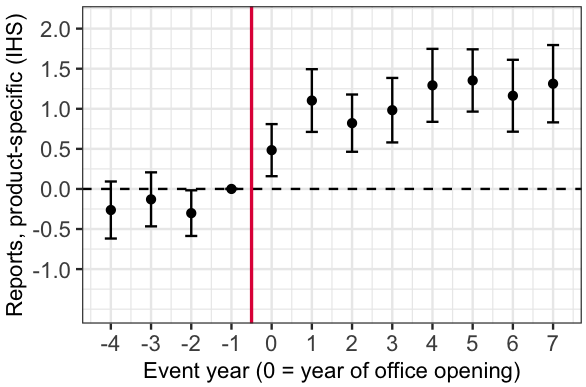
(f) CSA: Controlling for non-Korean exports

Notes: For 8a, 8c 8e, 8f, the outcome variable is the inverse hyperbolic sine of Korean exports to the country-year in question. For 8b and 8d, 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 in 8a), and 8b 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. Point estimates in 8c, 8d, 8e, and 8f, 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. Standard errors also clustered at the country-level are reported around each point estimate for this approach. 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.

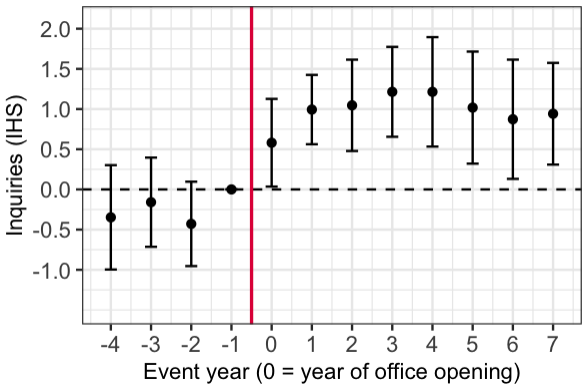
Figure 9: Event-study estimates of the effect of office opening on immediate KOTRA activity



(a) Controlling for non-Korean exports.



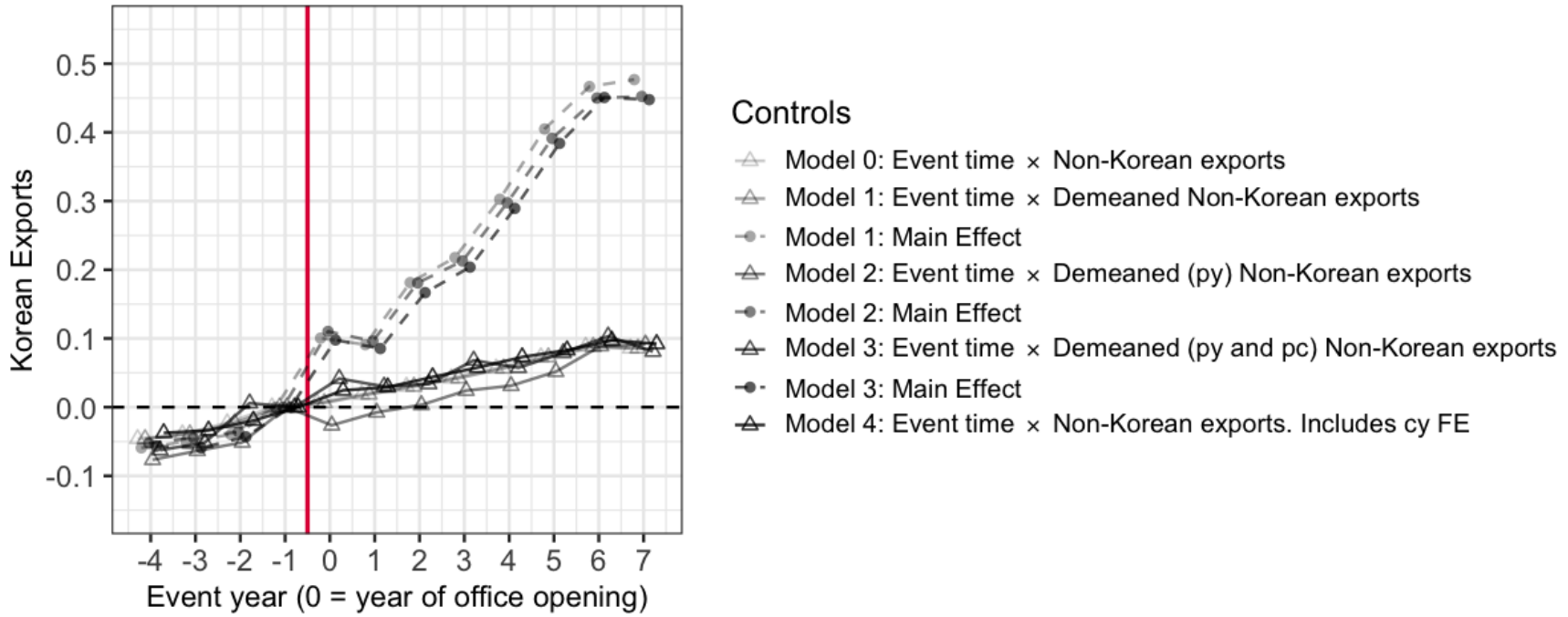
(b) CSA : "Not-yet-treated" control group



(c) CSA : Europe only

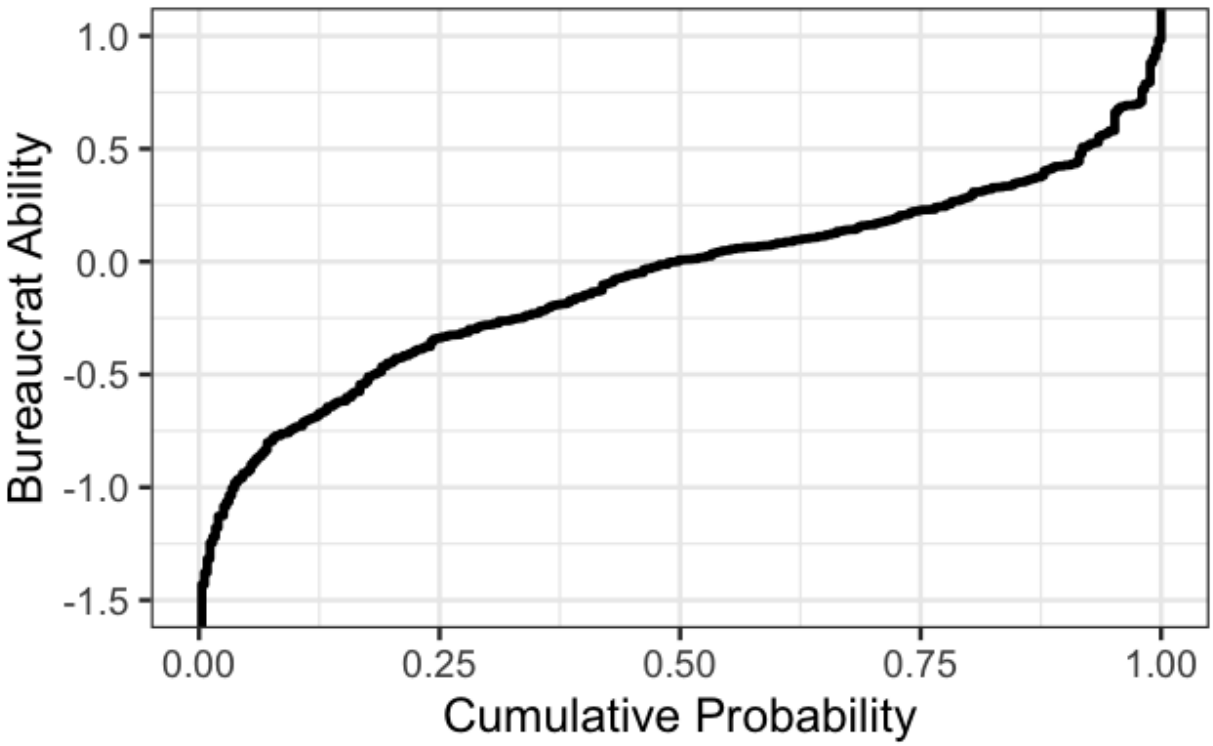
Notes: Each panel reports coefficients θ_k from estimating equation (1). Instead of exports, we aim to explain three measures of KOTRA activity, each transformed using the inverse hyperbolic sine. (1) The number of reports about a country, (2) the number of product-specific reports - which may be more specific or informative, (3) the number of inquiries for trade with the country.

Figure 10: Mediation - Event time interacted with market conditions.
Relaying information about market conditions not central to the effect of office opening reported in figure 7.



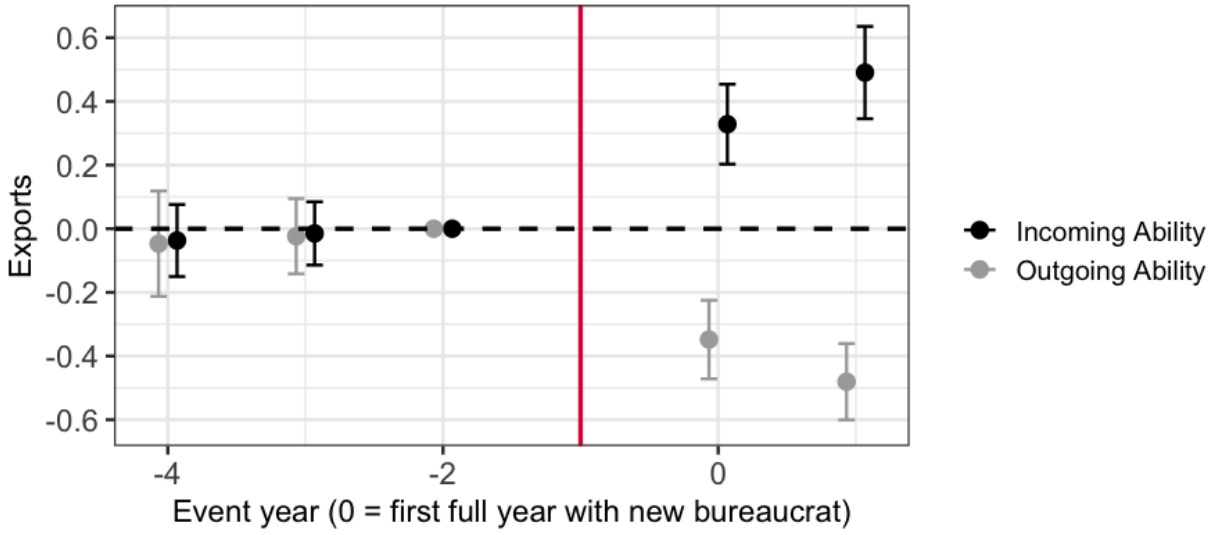
Notes: The figure displays coefficient estimates from five different models that estimate equation (2). The models differ by their measure of non-Korean exports. Each model gives roughly the same results. Model 0 does not normalize non-Korean exports at all. Given that the mean of non-Korean exports is 6.625, this means the coefficients $\hat{\theta}_k$ become difficult to interpret. For this reason, the figure only reports the estimated coefficients for the interaction. Models 1-3 use demeaned non-Korean exports. Model 1 simply subtracts the mean from non-Korean exports (6.625). Hence, the main effect is the main effect when non-Korean exports are 6.625. Model 2 subtracts mean non-Korean exports for each product-year. Model 3 does the same for each product-year and product-country. Model 4 reverts to using (non-normalized) non-Korean exports but includes country-year fixed effects in the regression. This country-year fixed effect absorbs the main effect.

Figure 11: CDF of bureaucrat fixed effects.
Moving from 20th to 50th percentile: 0.46. 50th to 90th: 0.42



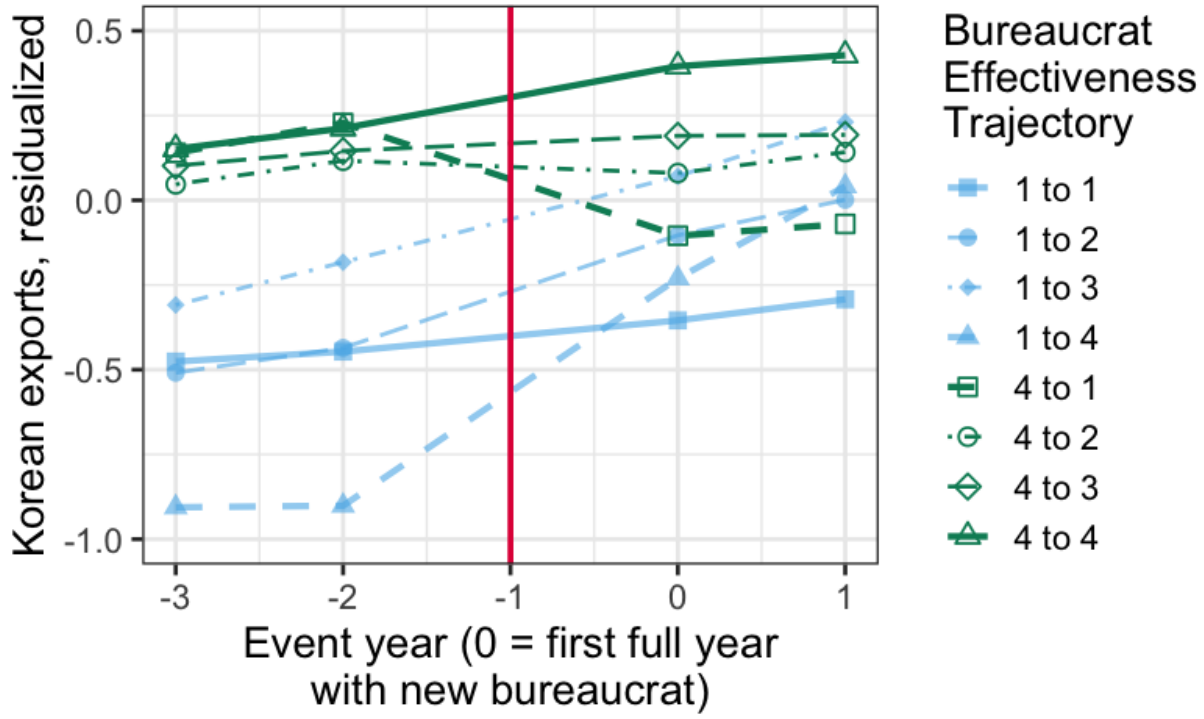
Notes: The figure shows the cumulative density function of bureaucrat fixed effects.

Figure 12: Event study estimates: Effect on exports due to switch in bureaucrat. Sharp jump in exports explained by change in bureaucrat effectiveness.



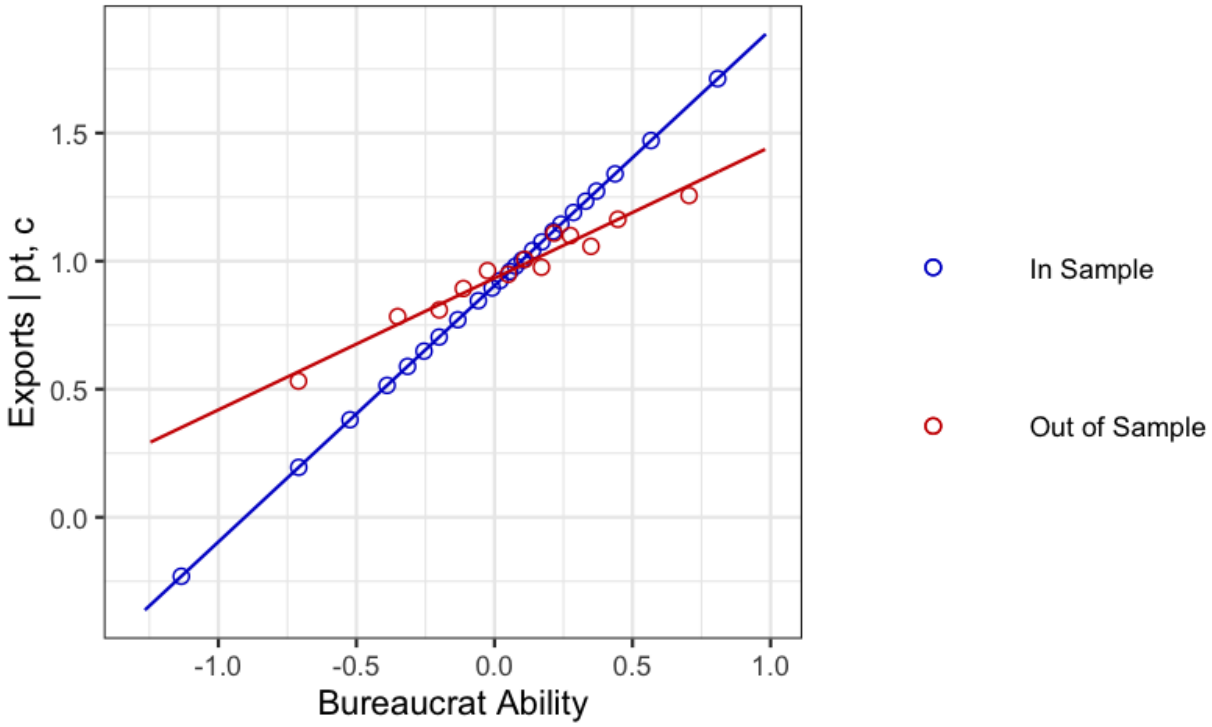
Notes: The figure shows the estimated effect of the change in bureaucrat fixed effects on exports around the time that the bureaucrat heading a country office changes. These estimates are $\hat{\beta}_k$ obtained from estimating equation (12). The horizontal axis indexes years in which bureaucrats work in a particular country. Year 0 is the first full year that the new bureaucrat heads the country office. Year -2 is the last full year that the old bureaucrat headed 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 13: Event study of Korean exports around switches between bureaucrats. Parallel pre-trends. Discontinuous jump in exports in line with change in bureaucrat ability.



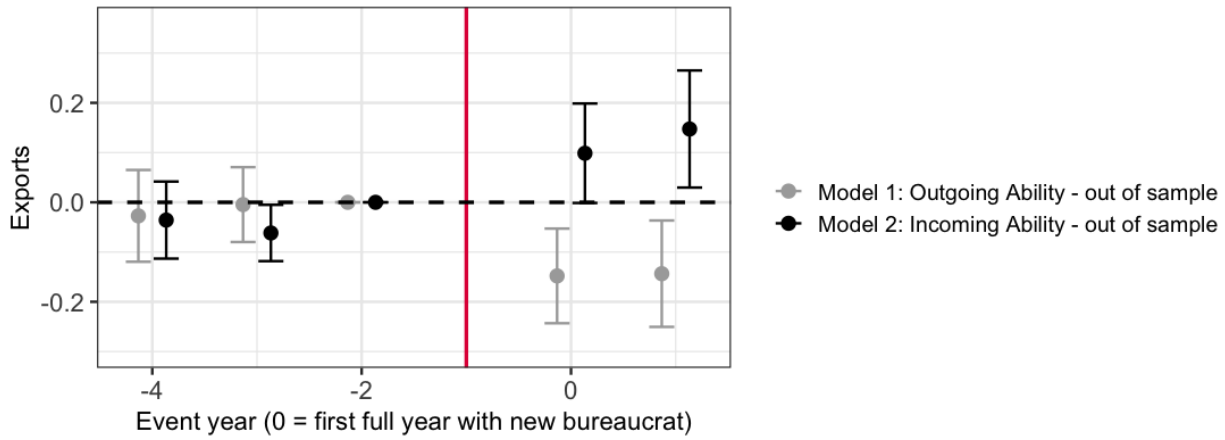
Notes: The figure shows time trends in exports around the time that the bureaucrat heading a country office changes. The horizontal axis indexes years in which bureaucrats work in a particular country. Year 0 is the first full year that the new bureaucrat heads the country office. Year -2 is the last full year that the old bureaucrat headed 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 product-country and product-year fixed effects. Bureaucrats are classified into quartiles according to the fixed effects obtained after residualizing exports by product-country and product-year fixed effects.

Figure 14: Bureaucrat fixed effects and exports: In and Out of Sample



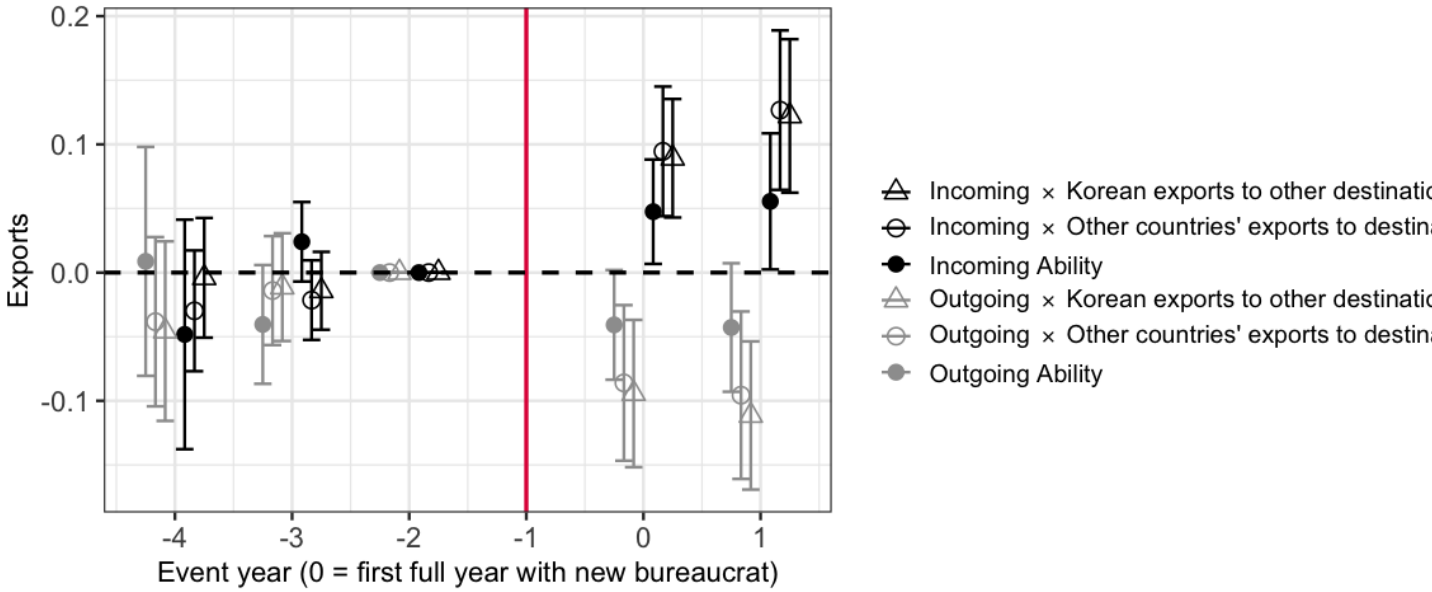
Notes: The figure displays a binned scatterplot. The y-axis shows exports after subtracting product-year fixed effects (pt) and country-year fixed effects. The two above fixed effects, as well as in-sample bureaucrat ability (fixed effects) are estimated using equation (4) and all country-years. Hence, 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 15: Event study estimates: Out-of-sample bureaucrat fixed effects



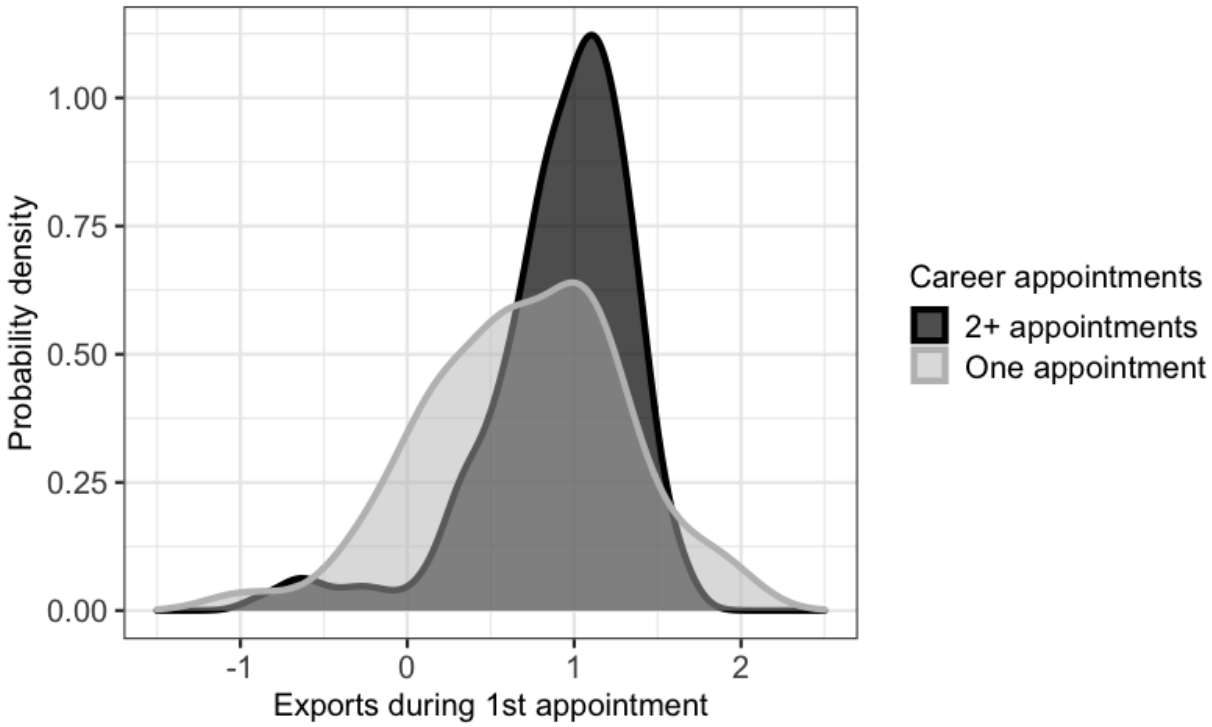
Notes: The figure shows the estimated effect of the change in bureaucrat fixed effects, estimated out of sample, on exports around the time that the bureaucrat heading a country office changes. These estimates are $\hat{\beta}_k$ obtained from estimating equation (12). As out-of-sample fixed effects are not available for every bureaucrat, to maximize power, we report coefficients from two different models. First, we estimate equation (12) using *out-of-sample* estimates for the outgoing bureaucrat and *in-sample* estimates for the incoming bureaucrat. Second, we estimate equation (12) 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 heads the country office. Year -2 is the last full year that the old bureaucrat headed 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 16: Event study estimates: Decomposition
Good bureaucrats increase elasticity to market conditions



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}). We view this as a proxy for this destination's product-specific demand. The triangles give the interaction with Korean exports of the same product to the other destinations (β_k^{supply}, δ_k^{supply}). We view this as a proxy for 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 heads the country office. Year -2 is the last full year that the old bureaucrat headed the office. Bureaucrats effectiveness are fixed effects obtained after residualizing exports by product-country and product-year fixed effects.

Figure 17: 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.

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

	1st opening	Non-Korean imports in 1962	Predicted	Predicted (Omit own opening)
Italy	1966	4	1967	1969
Netherlands	1966	5	1969	1969
Germany	1967	2	1966	1966
UK	1967	1	1966	1966
France	1969	3	1967	1967
Sweden	1969	7	1971	1972
Austria	1971	11	1973	1974
Belgium	1972	6	1969	1969
Spain	1972	9	1972	1973
Denmark	1973	8	1972	1972
Norway	1973	10	1973	1973
Finland	1974	12	1974	1974
Greece	1974	14	1974	1975
Portugal	1974	16	1981	NA
Turkey	1975	15	1975	1981
Ireland	1981	13	1974	1974

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 (1969) - the 4th when omitting the actual opening in Italy.

Table 2: Appointments Descriptives.

	Full Sample (1)	Country-Level Analysis (2)
Directors	475	397
Countries/Offices	138	86
Directors $>$ 1 Office over the Sample Period	252	194
Countries $>$ 1 Director over the Sample Period	121	82
Events / Distinct Appointments	974	728
Country-years		2060
Observations		1,772,452

An observation is a product-country-year. 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. All directors and countries in the country-level analysis are part of the same connected set. Restricting the analysis to this connected set is natural as only a single country and a single bureaucrat are outside the largest connected set.

Table 3: Variance decomposition of exports into bureaucrat and country components

		Actual data				Placebo check: Bureaucrats randomly shuffled to countries			
		All bureaucrats		Bureaucrats with ≥ 2 appointments		All bureaucrats		Bureaucrats with ≥ 2 appointments	
		Component (1)	% Share (2)	Component (3)	% Share (4)	Component (5)	% Share (6)	Component (7)	% Share (8)
Var(exports|*pt*), spell-level		0.732	100	0.737	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 (5). 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.

Table 4: The effect of EP on exports depends on the individual bureaucrat. Bureaucrats explain substantial variation in exports compared to countries.

	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	
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 (3) 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

Table 5: Learning-by-doing in 1st appointment is one source of bureaucratic capacity.
Korean exports increase in product-specific experience

	OLS			IV	
	Exports in Product			First Stage Experience in p	Second Stage Exports in p
Model:	(1)	(2)	(3)	(4)	(5)
Tenure	0.640***	0.159***	0.157***	0.041*	0.154***
	(0.059)	(0.035)	(0.035)	(0.023)	(0.035)
Experience in Product	0.106***	0.035***			0.073**
	(0.032)	(0.013)			(0.037)
Instrument 1 for Experience in Product			0.022*	0.303***	
			(0.011)	(0.028)	
Product-year FE	Yes	Yes	Yes	Yes	Yes
Product-country FE		Yes	Yes	Yes	Yes
Observations	1,772,452	1,772,452	1,772,452	1,772,452	1,772,452
KP F-statistic of excluded instruments				116.47	

Notes: In parentheses: Standard errors clustered by bureaucrat. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1. An observation is a product-country-year. 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. "Experience in Product" is measured using Korean exports of this product during a bureaucrat's first appointment relative to the three previous years. Instead of actual Korean exports, instrument$_1$ uses non-Korean exports in the relevant product-country-year normalized by Korean relative to non-Korean exports to all other countries in the relevant product-year.

Table 6: Korean exports increase in product-specific experience
Results from table 5 robust to including country-year FE and alternative instrument.

	IV Second Stage Exports in Product				
	(1)	(2)	(3)	(4)	(5)
Tenure	0.154***	—	0.151***	—	—
	(0.035)		(0.035)		
Experience in Product	0.073**	0.062**	0.097***	0.073***	0.071***
	(0.037)	(0.030)	(0.034)	(0.024)	(0.024)
Instrument for experience in product					
Instrument 1	Yes	Yes			Yes
Instrument 2			Yes	Yes	Yes
KP F-statistic of excluded instruments	116.47	91.80	352.35	291.55	161.81
SH overidentification test - p-value					0.583
Country-product FE	Yes	Yes	Yes	Yes	Yes
Product-year FE	Yes	Yes	Yes	Yes	Yes
Country-year FE		Yes		Yes	Yes
Observations	1,772,452	1,772,452	1,772,452	1,772,452	1,772,452

Notes: In parentheses: Standard errors clustered by bureaucrat. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1. Effect of tenure is not identified with country-year fixed effects An observation is a product-country-year. 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. "Experience in Product" is measured using Korean exports of this product during a bureaucrat's first appointment relative to the three previous years. Instead of actual Korean exports, instrument_1 uses non-Korean exports in the relevant product-country-year normalized by Korean relative to non-Korean exports to all other countries in the relevant product-year. Instrument_2 additionally normalizes by Korean relative to non-Korean exports of all other products in the relevant country-year.

Table 7: Bureaucrat's product-specific experience matter within appointment-product.
Korean exports respond more to demand shocks with bureaucrat experience

	Exports in Product					
	(1)	(2)	(3)	(4)	(5)	(6)
Demand Shock	0.194***	0.194***	0.194***	0.194***	0.194***	0.194***
	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)
... × Tenure	0.056**	0.054*	0.056*	0.056*	0.056*	0.056*
	(0.028)	(0.028)	(0.029)	(0.029)	(0.028)	(0.029)
... × Experience in Product	0.025***			0.024***		
	(0.009)			(0.009)		
... × Instrument 2 for Exp. in Prod.		0.025**			0.025**	
		(0.012)			(0.012)	
... × Instrument 1 for Exp. in Prod.			0.014*			0.013*
			(0.007)			(0.007)
Appointment-product FE	Yes	Yes	Yes	Yes	Yes	Yes
Country-product FE	Yes	Yes	Yes	Yes	Yes	Yes
Bureaucrat-product FE	Yes	Yes	Yes	Yes	Yes	Yes
Year within appointment × Experience in product				Yes	Yes	Yes
Observations	1,772,452	1,772,452	1,772,452	1,772,452	1,772,452	1,772,452

Note: In parentheses: Standard errors clustered by bureaucrat. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1. Main effects of tenure and experience are not identified with appointment-product fixed effects. An observation is a product-country-year. 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. "Experience in Product" is measured using Korean exports of this product during a bureaucrat's first appointment relative to the three previous years. Instead of actual Korean exports, instrument_1 uses non-Korean exports in the relevant product-country-year normalized by Korean relative to non-Korean exports to all other countries in the relevant product-year. Instrument_2 additionally normalizes by Korean relative to non-Korean exports of all other products in the relevant country-year. The demand shock using product-specific non-Korean exports during the year when outcomes are observed, normalized like instrument_1.