# HUMAN MOBILITY AND THE GLOBALIZATION OF KNOWLEDGE PRODUCTION: CAUSAL EVIDENCE FROM MULTINATIONAL ENTERPRISES*

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**Abstract**

We investigate how reforms that ease or restrict human mobility affect global innovation. We leverage a unique dataset merging patent data with exhaustive information on business-related migration reforms that take place in 15 countries over 26 years, and we employ a novel event-study approach. Our results show that reforms favoring inventor mobility increase the patenting, including global collaborations, of MNE subsidiaries within a country, while the opposite is true for reforms discouraging inventor mobility. Further, we show that positive migration reforms partly explain the increasing share of global knowledge production by countries—such as with low initial patenting observed over the past decades. This suggests that policies affecting human mobility contributed to the global shift in the geography of innovation toward emerging markets.

**JEL Codes:** O33, O34, O38, J61, K37

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# 1 Introduction

In 2019, the World Intellectual Property Organization (WIPO) reported that China alone accounted for almost half of all the world's patent filings, with India also registering impressive increases in global patent production. "Asia has become a global hub for innovation," declared WIPO Director General Francis Gurry.[1] Just a few decades ago, these emerging markets constituted a negligible share of global patent production. Given that most formal innovation is carried out by multinational enterprises (MNEs), it is reasonable to assume that this trend is also reflected in overall MNE activity over the past decades; this would include both more innovation overall as well as subsidiaries taking a bigger role in innovative activities. In fact, by 2018, according to the U.S. Bureau of Economic Analysis (BEA), the 20-year growth rate of R&D activities of U.S. MNEs in foreign countries—estimated to be 6%—exceeded the growth rate of R&D within the U.S., which was estimated at 4%. The question that arises, is What mechanisms contributed to MNEs increasing their innovation output globally while at the same time shifting innovative activities between countries? This paper focuses on this question by studying the role human mobility has played in this process.

Recent literature in economics acknowledges that the geography of innovation of MNEs is changing. Earlier work argued that knowledge-generating activities such as patenting should be conducted within the high-skill labor-intensive headquarters of the MNE and that inventions patented at home could then generate profits in foreign markets through production abroad (see Hymer 1960; Caves 1971; Carr et al. 2001). However, recent evidence, notably Branstetter et al. (2006), Foley and Kerr (2013), Branstetter et al. (2014), Miguelez (2016), and Kerr and Kerr (2018), documents a changing view of innovation within MNEs where international co-invention and global collaborative patenting become increasingly central.[2] This view suggests that technological devel-

[1] Source: https://www.wipo.int/pressroom/en/articles/2019/article_0012.html.

[2] MNE innovation is increasingly linked to international localization. Branstetter et al. (2014) document that MNEs from advanced industrial economies are largely responsible for the "exponential" growth in U.S. patents filed from China and India, such that "MNE sponsorship accounts for the majority of new U.S. patents granted to Indian or Chinese inventors in recent years" (pp. 139-140, ibid.). Further, Kerr and Kerr (2018) cite analysis from the Bureau of Economic Analysis to state that the share of R&D for U.S. MNEs conducted by foreign subsidiaries rose from 6% in 1982 to 14% in 2004.

opment may depend on localization, as MNE innovation is increasingly recognized to rely on the knowledge production and absorptive capacity of its subsidiaries. In this theory, the subsidiary acts as a source of knowledge that relies on locally hired workers (Cohen and Levinthal, 1990; Minbaeva et al., 2003; Minbaeva, 2007; Chang et al., 2012) and/or as a source of knowledge flows that rely on transferred human capital (Kerr et al., 2016). Cross-border mobility of inventors is highlighted as a key mechanism for global knowledge production by MNEs, but evidence of this relationship remains thin, especially in a multicountry setting, which is essential to understanding geographic shifts in the production of global innovation.[3]

The purpose of this study is to explore the role that human mobility has played in changing the geography of innovation of the MNE. Specifically, we do this through investigating whether and to what extent MNEs' subsidiary-level output in innovation changes following immigration reforms that ease or harden barriers for migration into a country. To do this, we put together a new dataset with the exhaustive list of business-related migration reforms adopted in 15 countries over the period from 1990 to 2016 (61 reforms in total), which we match with the patenting activities of 28,443 MNEs and their 70,624 country-level subsidiaries.[4] We take subsidiary information from the universe of all USPTO patents, which allows us to link subsidiaries with disambiguated MNEs and to follow inventors over time and, thus, identify movers across countries (or global migrant inventors (GMIs)), following the term used by Bahar et al. (2021).[5] In our analyses, we consider patent outcomes of three types: (a) overall patent counts; (b) global collaborative patents, or GCPs (defined by Kerr and Kerr, 2018 as those patents with geographic footprints that cross international borders); and (c) domestic patents (patents where all inventors reside in the same

[3]Starting with Edström and Galbraith (1977), scholars have documented that geographic mobility of human capital enables multinational firms to transfer and exploit knowledge more efficiently in the intra-firm context than would be possible through external market mechanisms (Almeida and Kogut, 1999; Rosenkopf and Almeida, 2003; Oettl and Agrawal, 2008; Foley and Kerr, 2013; Singh, 2005; Choudhury, 2016). In addition, extensive literature examines cross-border ethnicities as a key mechanism in facilitating global knowledge coproduction as documented by Branstetter et al. (2014), Foley and Kerr (2013), Kerr (2008), Kerr and Kerr 2018, Saxenian (2002); Saxenian et al. (2002); Saxenian (2007). However, relatively few studies examine how migration policy influences the geography of patenting within MNEs, especially *across* countries.

[4]The countries included in our data are Brazil, Canada, Chile, China, Germany, India, Japan, Mexico, the Philippines, Portugal, South Korea, Spain, Taiwan, the United Kingdom, and the United States.

[5]In our data, subsidiaries are identified as the interaction between MNEs and countries where patents are filed. Following (Bahar et al., 2021), an inventor is considered a GMI if he or she is observed patenting in a different country with respect to the one of first appearance in the data.

country at the time of filing). We also use fine-grained data on inventors' mobility to measure how the changes in migration policy affect cross-border human capital flows and consequent patenting of GMIs themselves; we also use this data to examine the patenting of never-movers in an effort to explore spillover effects.

A key challenge for causal inference is that MNE subsidiaries' behavior is not necessarily exogenous to the country-level enactment of migration policy changes. In fact, while unlikely, MNEs may anticipate such events and redeploy resources dedicated to innovation accordingly. In an effort to reduce endogeneity concerns and in order to establish causal estimates of how MNE subsidiaries are affected by such business-related migration policy changes, we employ an exposure-based event study design that identifies plausibly exogenous variations in the level of exposure of different subsidiaries to these reforms, prior to the reform itself. In particular, we leverage the fact that subsidiaries belonging to MNEs with strong habits of international human capital rotation, which we measure through the historical rate of inventor mobility observed within the MNE in all other countries of operation, might be more responsive ex post to policies affecting business-related migration.

Our results show that policies deterring business-related migration decrease the number of GCPs and domestic patents filed by the MNE within a country, while pro-business migration reforms significantly increase the number of GCPs filed. Subsidiaries with 1 standard deviation higher exposure see a 2.2% reduction in GCPs and a 17.5% reduction in domestic patents following a negative business reform, while they see an increase of 0.6% in GCPs following a positive business reform. The positive effect is entirely driven by additional patents filed by teams of inventors that include at least one GMI, while negative migration reforms decrease both patents filed by teams with GMIs and by teams composed uniquely by never-moving inventors, which we interpret as evidence of negative spillovers. We look at various measures of patent quality and do not find strong evidence of quality effects. Further, we show that negative migration reforms significantly decrease the share of global patents filed by the country that implemented such policies, regardless of its position in the global innovation ranking. On the contrary, positive migration reforms show heterogeneous effects since they increase only the share of global patents filed in countries with low initial shares of knowledge production. This finding suggests that policies encouraging human mobility have contributed to the observed shift in the geography of innovation toward emerging markets.

Our back-of-the-envelope calculations reveal that without positive migration reforms, the countries in our sample would have produced 39% fewer patents by the end of the period; without negative reforms, they would have produced 35% more patents than we actually observe. Our calculations also reveal that in the absence of positive migration reforms, the share of global innovation produced by emerging markets would have grown from 5% to only 12% from 1990 to 2015, instead of reaching 25% as we observe in the data. These results provide strong evidence that inventor mobility causally facilitates MNEs' global production of inventions and shifts the geography of patenting production, carrying important policy implications. In particular, the presence of strong spillover effects associated with negative reforms underlines how policies deterring human capital mobility are heavily detrimental to local and global knowledge production. Additionally, countries with relatively low levels of innovation can exploit policies encouraging international mobility as a catch-up strategy.

These results contribute to three strands of the literature. We first show that GMIs are a key input to the production of innovations among the modern MNE and that MNEs react to policy changes affecting mobility costs by relocating their invention activities. Here, we contribute to the nascent literature on international coinvention and MNEs' global collaborative patenting activities (Kerr and Kerr, 2018; Branstetter et al., 2014); and we are the first to show that even the production of domestic patents is causally dependent on the migration policy context. Second, the results emphasize the role of MNE subsidiaries in the knowledge-generating process and, thus, they underline the importance of their "absorptive capacity." This provides support for the knowledge-based view of the MNE—namely that subsidiaries exist due to their ability to manage knowledge transfers in the face of international barriers to market transactions (e.g., Kogut and Zander 1996; Caves 1971; Cohen and Levinthal 1990).[6] Finally, we contribute to the literature on the role of

[6]This more broadly relates to the literature on the cost of knowledge transfers across borders (Giroud, 2013; Gumpert, 2018; Bahar, 2020).

migration policy for innovation outcomes of firms and regions by shedding light on the implications of business-related migration reforms on MNEs' local innovation. Glennon (2020) shows that the 2020 H-1B visa freeze in the United States pushed U.S. MNEs to offshore employment. We complement that study by looking at the effect of both positive and negative migration reforms across many countries and years and by investigating the effect on the geography of innovation. We further add nuance to prior research by outlining the implications of immigration policy changes for subsequent innovation via the mechanism of knowledge transfer and knowledge recombination (e.g., Kerr and Lincoln 2010; Borjas and Doran 2012; Doran et al. 2014; Hornung 2014; Peri et al. 2015; Kahn and MacGarvie 2016; Beerli et al. 2018; Choudhury and Kim 2019; Bahar et al. 2020; Burchardi et al. 2020, Sequeira et al. 2020).[7]

In addition, we highlight data and methodology contributions. We collected and introduce with this study a novel database indexing 61 migration policy changes in 15 countries spanning the years 1990 to 2016, as described in Appendix D. With regard to methods, we outline an empirical approach for dealing with the econometric difficulties imposed by high-frequency events that are clustered over time and for estimating causal effects given such a setting.[8]

The remainder of the paper is organized as follows: section II covers the data constructed for estimation, section III outlines the empirical strategy, section IV presents the results on the number of patents filed by subsidiaries, section V presents the results on the geography of knowledge production, and section VI concludes. The paper is accompanied by an online appendix with supplementary materials.

[7] In the broader field, other research presents evidence on migration patterns and their shifts over time (e.g., Kerr et al. 2016; Czaika and Parsons 2017) as well as the empirical implications of immigration for local labor market outcomes (e.g., Borjas 2004, 2009; Hunt and Gauthier-Loiselle 2010). Even within the larger field, this study is one of the first to estimate effects across multiple countries and multiple events, as opposed to engaging in "case study" analyses.

[8] The context we study suffers from an embarrassment of riches of sorts—the frequency of reform events is so high for some countries that several events of the same general type occur across several consecutive periods. This clustered nature of reforms limits estimation under classical event study methods, where current practice is to consider only events that are, to some extent, isolated over time from other events. If the current study were to follow this practice and drop observations with consecutive reform events, we would quickly suffer from a loss of statistical power, as our reforms are measured across only 15 countries. Instead, we take steps to adjust event-study methods to deal with the closely time-clustered nature of the reforms, and we go to lengths to demonstrate the relative robustness of the estimation approaches we employ in Appendix E.

# 2 Data

## 2.1 Migration Reforms Dataset

One of our main data sources is the information we compiled on dozens of migration reforms in 15 countries over 26 years.[9] Our sample includes 61 business-related migration reforms enacted during the years 1990 to 2016 that either increased or decreased the expected flows of immigrants to those countries.[10]

To select the countries in our sample upon which the data collection was based, we started with the 16 countries used by Branstetter et al. (2006), who study the impact of systematic reforms designed to strengthen and standardize intellectual property on MNEs' foreign direct investments from 1982 to 1999.[11] We depart from their list by adding four major countries that count more than 1% of inventors that are GMIs in the patent data (Canada, Germany, the United Kingdom, and the United States) and by dropping five countries from the Branstetter et al. (2006) sample that patent very little and have a share of GMIs among all inventors that is 0.1% or less (Argentina, Colombia, Thailand, Turkey, and Venezuela).[12] Our final sample is reported in table 1. In the robustness analysis, we test that our results survive the exclusion of particular countries in our sample, to ensure that our findings are not driven by our sample choices.

Following collection of the data related to reform events, we analyzed the primary documents and sources describing the reforms to derive their anticipated effects on the volume and rights of different migrant types. For the sample considered, we isolated the reforms that specifically impact business-related migration. The reforms—which we detail fully in appendix D—largely

[9] These reforms were identified as part of a larger project to construct a systematic index of all unilateral policy reforms and governmental programs instituted across 15 countries and over more than a century, that were anticipated to drive changes in the migration patterns of high-skilled immigrants. (We provide more details on this project in appendix D).

[10] We focus on unilateral policy reforms adopted independently by countries and exclude regional agreements such as the European Union enlargement. We do this as an effort not to confound effects for firms in a given country with effects resulting from dynamics happening in other countries. As a robustness check, however, we present the coefficients obtained after excluding entire regions from our sample.

[11] The sample of Branstetter et al. (2006) includes Argentina, Brazil, Chile, Colombia, India, Japan, Korea, Mexico, Philippines, Portugal, Spain, Thailand, Turkey, Taiwan, and Venezuela.

[12] See appendix D for a detailed discussion on the selection of countries in the sample.

consist of changes in the visa application processes that either facilitate or harden the access to a country for business travelers (e.g., standardization of entry procedures, introduction of "point-based" systems selecting migrants with technical skill sets), or in changes in the benefits foreign workers received after entering the country (e.g., allowing for access to health benefits and facilities).

Some examples of reforms include:

- In 2009, South Korea introduced Contract Korea, which substantially restructured the ways in which business migrants could access the country. This program established a public office in charge of centralizing and supporting firm recruitment of global talents. The office's functions include identifying business and recruitment needs as well as providing visa recommendation, immigration support, and relocation assistance. A year later, the government implemented HuNet Korea, a three-way platform that standardized business-related migration processes and digitally matched three groups: high-skilled foreign workers searching for employment, companies seeking employees with technical skill sets, and the governmental system necessary for approving visa applications. Together, these reforms established a cohesive platform for long-term business-related migration into South Korea. Thus, these reforms are coded as promoting both the volume of business-related migration (e.g., through incentivizing migration directly) and the rights of such migrants (e.g., through facilitating paths to residency).

- In 2009, the Philippines Department of Justice issued a memorandum requiring foreigners that have been granted a visa of more than 6 months to apply for an Emigration Clearance Certificate if they want to leave the country. This ensures that the applicant has no derogatory records in the country and has no pending obligations with the government. In the same year, the Department of Labor made changes in the assignment of employment permits to migrants, aiming to prevent foreigners from "taking jobs that could be filled up by Filipinos." Following this reform, government officials might inspect establishments employing migrants to verify the legitimacy of their employment, while foreigners whose employment permit applications are denied are not allowed to submit new applications. Thus, these reforms are coded as decreasing both the volume and the rights of economic migrants.

Table 1 summarizes the countries and timing of all the reforms included in the sample, with further classification into positive and negative ones.[13] It also reports the subsample of them that affects permanent migration, which identifies reforms affecting stays of one year or longer. The number of positive changes outweighs the negative ones by more than three times, which is in line with the general observation that international migration flows have been growing over the past 20 years (Kerr et al., 2016). Some countries in our sample, such as Korea and Japan, experience numerous reforms that are temporally close to each other, which raises some challenges for the econometric strategy. In the next section, we propose a novel solution to cope with the high frequency of these events.

## 2.2 MNE Global Patenting Activity

Patent data comes from PatentsView, a data visualization tool maintained by the Office of the Chief Economist at the USPTO.[14] Among its many offerings, the open data platform contains the universe of patents *granted* by the USPTO from 1976 to present (naturally, many patents in the dataset have application dates prior to 1976) with some important characteristics that makes such dataset stands out. In particular, PatentsView uses complex algorithms to disambiguate the names of inventors and of assignees across time, resulting in a unique identifier for both inventors and assignees. The data on patents also includes the location of inventors at the time of filing of the patent, which along the unique identifier, allows us to track the inventors also across space (see Monath et al. 2020 for more information on the disambiguation methods).[15]

[13] Two policies include both positive and negative elements and are, thus, double counted in this table. They concern the United Kingdom in 2006 and Italy in 1998. For more details, see appendix D.

[14] The tool is a joint effort by the USPTO, American Institutes for Research (AIR), University of Massachusetts Amherst, New York University, University of California, Berkeley, Twin Arch Technologies, and Periscopic.

[15] Extensive prior work describes both the USPTO data and assignee disambiguation efforts (see Hall et al. 2001; Jaffe 2017; Balsmeier et al. 2018) as well as the role of patent data as an indicator of innovation (Trajtenberg, 1990; Hall et al., 2001).

Using the inventors' locations alongside the unique identifiers for the patent assignee (typically an MNE), we index the international "geographic footprint" of each MNE subsidiary's innovation activity by measuring aggregate patent counts at the assignee-country level. We then limit our sample to MNEs and their subsidiaries with patent production in at least two of the 15 countries for which we have gathered reform information over the sample period (this is because MNEs patenting in only one of them would be dropped by the fixed effects included in the analysis anyway). With these data, we create a number of outcome measures (as defined below).

### 2.2.1 Outcome Measures

Our primary outcome measures are counts of patents assigned to an MNE subsidiary in a given year. We consider the combination of an assignee and a country-of-inventor as an MNE subsidiary. In terms of time, since our goal is to exploit the point of time when the innovation happens (consistent with the standards in this literature), we define the patent date as the earliest between the application date and the priority date.[16] As our focus is on how global patenting activity shifts following such reforms, we focus on subsidiary-year production of patents classified as follows:

- **Total Patent Counts:** The sum of granted USPTO patent applications to a given assignee, applied for in year $t$ by inventors in a given country of residence.
- **Global Collaborative Patent (GCP) Counts:** A subset of total patent counts that includes only patents to a given assignee applied for in year $t$, where at least one inventor lives in a country other than the subsidiary under consideration.[17]
- **Domestic Patent Counts:** A subset of total patent counts, counting only patents belonging to a given assignee where *all* inventors reside in the same country as the subsidiary.

[16] For patents that have been filed only in the USPTO, the application and priority date should be the same. For patents that have been filed in another patent office (such as the European Patent Office or the Japanese Patent Office, for instance), the priority date (often recorded in the patent record) refers to the date in which the patent was filed for the first time in any patent office.

[17] Kerr and Kerr (2018) first described the concept of GCP, and we draw on that paper as our motivation for using GCPs to measure globalized innovation processes. While defined in that study as an MNE patent with a U.S. and an international invention team, we define a GCP as any patent with a geographic footprint crossing an international border.

Since we are interested in incorporating measures that reflect inventor mobility—as responding to migration reforms—as part of patenting activity, we use these data also to count patents by inventors who have moved across borders following migration reforms. Consistent with the work of Bahar et al. (2021), we refer to inventors crossing borders as GMIs. An inventor is considered a GMI starting from the point where he or she is observed patenting in a country different from the one of the GMI's first appearance.[18] With this definition, we create a number of count variables to complement the ones above that will serve us in our empirical strategy:

- **GMI Patent Counts:** The sum of granted USPTO patent applications applied for in year $t$ by the MNE subsidiary in a given country, filed by a team in which at least one inventor is identified as a GMI.
- **Non-GMI Patent Counts:** As above, but for all patents filed by a team in which *none* of the inventors is identified as a GMI.

Finally, we use different indicators constructed by the OECD to capture a measure of quality of the patents (Squicciarini et al., 2013). We end up with five distinct proxies for quality, which we aggregate for each MNE subsidiary per year: (a) patent generality; (b) patent originality; (c) patent radicalness; (d) share of patents considered breakthrough; and (e) number of citations per patent. We use these measures to present results for the impact of migration reforms on all five innovation quality measures.

### 2.2.2 Reform Exposure Measures

As part of our identification strategy, we additionally use patenting activity to estimate MNE subsidiaries' exposure to the enacted reforms. Conceptually, reforms impact MNEs by easing or complicating their effort to transfer human capital across countries. We posit that a subsidiary that is part of an MNE where the labor force is very mobile is likely to respond more to changes

[18] We tested the robustness of our findings using different measures of GMIs (e.g., an inventor being considered a GMI only during the first year after his or her cross-border moved is observed), and we find our results to hold. These results are available upon request.

in migration incentives. For instance, following a reform restricting the rights of foreign workers, subsidiaries of very mobile MNEs might be more capable or willing to redeploy their employees elsewhere. We can imagine the opposite when a reform introduces new advantages for migrants.

Our measure of exposure is computed as the ratio between the number of mobile inventors that patented in all the other subsidiaries of the MNE, except for the one where the reform takes place, scaled by the total number of inventors in all the other subsidiaries of the MNE.[19] This ratio is computed over a moving window of five years prior to each observation.[20] Given that our measure of exposure might still be somewhat correlated with the timing of reforms (even if it is computed using the mobility rate observed in other countries), we test the robustness of our results to an exposure measure that applies the same formula but uses the moving window spanning 5 to 10 years prior to each observation. We report results using this specification in appendix B.1.

### 2.2.3 Final Sample

When the reforms are combined with the patent measures, the data consists of a finalized panel at the MNE-country-year level that is balanced within country and which consists of 297,919 observations indexing 28,443 MNEs with a total 70,624 subsidiaries across the 26 years observed. We present descriptive statistics in table 2. A few observations are of note. First, GCPs and patenting by GMIs represent the minority of patenting by the MNEs, since domestic patents represent, on average, approximately 88% of patent production by MNE subsidiaries. The summary statistics show that patents filed by teams including at least one GMI represent about 21% of MNE patenting activity; the rest are filed by teams of never-movers. GMIs are more prevalent in the production of GCPs, since more than 50% of these international collaborations are filed by teams with at least one GMI. In a given year, the average subsidiary in the sample produces 13 patents. The distribution is, however, highly skewed: the median subsidiary files only two patents

[19] For this measure we consider only inventor mobility happening within the same MNE and across countries, in order to capture the HR policy of the firm.

[20] We assign an exposure of zero to subsidiaries belonging to MNEs that file patents only by teams of never-movers in all the other countries over the window of interest. We also assign an exposure of zero to MNEs that are not observed patenting at all over the window of interest.

per year, while the one at the 95th percentile files 40 patents, and the maximum reaches more than 7,000. On average, each subsidiary counts 1.4 mobile inventors, which amounts to 13% of their total number of inventors. High-exposure subsidiaries have four times more GMIs, which accounts for double the share of total inventors. Finally, MNEs with higher inventor mobility rates—our measure of exposure—are also the firms that patent the most. This is consistent with the fact that large MNEs can invest more in the mobility of their employees through the creation of dedicated HR teams dealing with, for instance, travel formalities. Interestingly, the quality of patents filed—according to a number of measures—is similar for both low- and high-exposure subsidiaries.

Appendix table B1 displays the frequency of subsidiaries and patents of the different types across the reform countries during our sample years. There is substantial heterogeneity among the MNE subsidiaries across the countries, with Western countries (Germany, the United Kingdom, and the United States) showing the largest concentration of MNE, followed by Asian countries (e.g., Japan, China, and Taiwan). Additionally, certain countries produce global collaborative patents at greater rates than they do domestic patents—and at significantly higher rates than those found in Kerr and Kerr (2018). This underlines wide heterogeneity in the knowledge production strategies.[21]

Finally, patenting rates rose significantly post-1980 (an increase that is well documented in Kortum and Lerner 1999), and domestic patents rose substantially more than GCPs, as shown in figure 1a. At the end of the period, there is a slight decline due to rightward censoring, explained by the time lag existing between patent filing and approval. In fact, to avoid our results being affected by this censoring, we limit our sample period to just the year 2016, though this has no qualitative impact on our findings. Beyond the observed growth in the number of patents registered in the USPTO data, we also observe significant growth in the share of inventors that move internationally, going from about 1% in the 1970s to 12% in 2015 (figure 1b), consistent with Bahar et al. (2021). We further observe a substantial shift in the distribution of patents across countries over the period (figures 1c and 1d). In 1995, the United States filed 60% of all

[21] They measure collaborative patenting rates among U.S. MNEs and find a rate approximately 30% to 55%.

patents in our sample, followed by Japan (25%) and Germany (7%). Emerging markets such as China, India, and Taiwan accounted for a negligible share of global patents. In 2015, the United States and Japan remained the leaders of innovation activities, but their global patent shares decreased significantly, while China, Korea, Taiwan, and India started playing an important role in global knowledge production. Over this period, there was an important shift in the geography of innovation production away from developed countries (such as the U.S., Germany, and Japan) toward emerging markets. Our analysis below explores whether policies affecting human mobility had a role in explaining such shift.

# 3 Empirical Strategy

Our empirical strategy applies an event study framework in which the identification relies on the assumption that migration policy reforms—our "treatment" events—are exogenous to the MNE subsidiaries within the enacting country. To ensure exogeneity, we exploit the fact that although assignment of reform events is potentially endogenous to country-level characteristics and trends, subsidiaries within the same country vary in the extent to which they are capable of reacting to a given policy change. Thus, our identification strategy does not rely only on comparing countries with and without reforms before and after (given that governments may enact reforms in anticipation of shifting innovation trends, inducing reverse causality), but compares MNE subsidiaries *within* the same country with different ex ante exposure to these reforms. In particular, subsidiaries belonging to MNEs with high levels of initial inventor mobility are expected to be more responsive to legal changes affecting migration incentives ex post. We model this as:

$$Y_{fct} = \beta_0 + \beta_1 exp_{fct} + \beta_2(exp_{fct} \times PRef_{ct}) + \beta_3(Exp_{fct} \times NRef_{ct}) + \gamma_{ct} + \delta_{ft} + \epsilon_{fct}, \quad (1)$$

where $Y_{fct}$ represents the innovation outputs in year $t$ of an MNE subsidiary, defined as the combination of MNE firm $f$ and country $c$. Given that the distribution of the number of patents filed by a subsidiary in a given year is very skewed, we run the regressions on arcsinh transformed outcomes, such that the coefficients can be interpreted in terms of growth rates, and the variables are defined at zero (Card et al., 2020). The outputs are a function of $exp_{fct}$, the mobility rate of the MNE observed across the other subsidiaries, and the interaction of the latter with positive ($PRef_{ct}$) and negative ($NRef_{ct}$) reform events taking place in the country.

Formally, the exposure measure is defined by the following formula:

$$Exp_{fct} = \frac{\sum_{c',t'} MobInv_{fc't'}}{\sum_{c't'} Inv_{fc't'},}$$

where $c' \in C|\{c\}$ and where $t' \in (t-5, ..., t-1)$.

To ease the interpretation of the results, the exposure measure $exp_{fct}$ is standardized to have a mean of 0 and a standard deviation of 1. Given that in many countries we observe more than one reform over the period, both $PRef_{ct}$ and $NRef_{ct}$ are count variables indexing the cumulative number of reforms enacted by year $t$ in the subsidiary country $c$ (more on this approach below). The key parameters of interest are, thus, $\beta_2$ and $\beta_3$. The outputs are additionally conditioned on fixed effects at the levels of MNE-year ($\delta_{ft}$) and country-year ($\gamma_{ct}$), in order to identify the effects of reforms independent of MNE and country trends.[22] We estimate the model using OLS, and we cluster the standard errors at the subsidiary level.

The counterfactual modeled by this approach compares the change in innovation output of high-exposure subsidiaries observed after the reform events with the same change observed among low-exposure subsidiaries, while netting out changes attributable to the country and the firm over time. For our identification strategy to produce unbiased estimates, we must make two assumptions: (a) that subsidiaries with initial low exposure serve as a control group for treated (high-exposure) subsidiaries in the context of migratory reform; and (b) that subsidiaries with

[22]We do not add subsidiary-level fixed effects because it would absorb 92% of the variation in output, up from 48% without them.

similar levels of exposure located in places without reforms in a given period serve as a control group for those located in countries that experience reforms in that period. In particular, our identification strategy relies on the fact that both the timing of the reform and the ex ante exposure of the subsidiary, combined, are exogenous to the subsidary's future patenting activity. We believe these are reasonable assumptions in our context, and we present several tests showing that exposure is not correlated with differential trends in patents in absence of reforms.

As alluded to earlier, an estimation challenge in this setting is the presence of repeated reforms that are highly clustered in time. Standard econometric practice suggests isolating those observations "treated" only once or estimating treatment effects only in short-run windows that do not include any repeated treatment events. However, neither technique is well suited to the current setting. As reform events are enacted repeatedly within the large majority of our countries (the only exceptions being Brazil, Canada, Chile, and India), omitting repeatedly treated observations would excessively reduce the sample. Reform events are additionally clustered in time, which severely limits the sample of periods for which it is possible to estimate short-run treatment effects independent of other reform events (see table 1).

To resolve this, we introduce a novel empirical approach to estimating treatment effects given repeated and clustered-over-time events. We use regressions that estimate the marginal treatment effect of each additional reform event. Specifically, we allow the event indicator terms ($PRef_{ct}$ and $NRef_{ct}$) to dynamically vary over time, changing in level as treatment events accumulate.[23] In our linear regressions, the key coefficients $\beta_2$ and $\beta_3$ are interpreted as the marginal effect of one additional reform on innovation outputs.[24]

[23] This term is akin to employing an "intensity of treatment" variable in difference-in-differences, in which treatment obtains multiple levels or reflects an observation's propensity to treatment (similar to specifications employed in, e.g., Duflo 2001; Acemoglu et al. 2004), but where the intensity of treatment varies with time.

[24] Appendix E reports simulations that validate the estimator, discusses the additional assumptions it imposes on causal inference, and outlines a generalized version of the estimator that allows the treatment effect to vary conditional on the level of consecutive events. Appendix A tests the validity of the main assumptions behind this estimator. We find that using our dependent variable as a count of reforms is a good approximation to the average effect of each reform separately.

# 4 Results

## 4.1 Stage "Zero" Results: Reforms and GMIs

Before moving to the main results, we test empirically for the basic premise behind our research question—namely, whether there is a change in the number of mobile inventors following a reform and whether our exposure measure is indeed correlated with such changes. Table 3 tests the first stage obtained from the main specification displayed in Equation 1, using both contemporary and historical exposure in columns (1) and (2), respectively.

Results show that subsidiaries that are 1 standard deviation more exposed have, on average, 20% more GMIs than the mean subsidiary. One additional positive reform increases that value by 1.8%, while one additional negative reform decreases it by 6%, when considering contemporary exposure. Historic exposure shows a slightly larger coefficient associated with positive reforms (+2%) and a smaller coefficient associated with negative reforms (-3.6%), but confirms our finding that negative reforms have, on average, stronger marginal effects on international flows of inventors than positive ones. This heterogeneity is confirmed in all the innovation outcomes presented in the next section. Given that our reforms are heterogeneous and not directly comparable to each other, we cannot know whether the stronger effects of negative policies are due to higher intensity of reforms or to higher impact at comparable intensity.

Taken together, these results confirm that migration reforms do affect the international mobility of inventors. In the next section, we analyze how this affects the location of knowledge production.

## 4.2 Main Results

In this section, we present the results obtained from applying the model described in Equation 1 on the main outcomes of interest. Table 4 reports the results for the total number of patents filed within a given subsidiary and for the breakdown count between global collaborative patents and domestic patents.

Results show that subsidiaries affiliated with MNEs with higher inventor internal mobility patent much more on average, since 1 standard deviation higher exposure is associated with 56% more patents overall, 11% more GCPs, and 72% more domestic patents. More interestingly, we see that additional negative reforms significantly decrease by 15% the total number of patents filed by exposed subsidiaries, which is explained by a 2.2% drop in GCPs and a 17.5% drop in domestic patents. Positive reforms do not have a significant effect on the overall number of patents, but increase significantly the number of GCPs by 0.6%. These results underline how the location of MNEs' knowledge production is highly dependent on the opportunities for mobility offered by countries, such that policies unilaterally adopted by different countries can long-lastingly change the geography of patenting activities.

It is worth considering the extent to which our identifying assumptions are reasonable and, thus, whether our results can be interpreted as causal. A first test consists of exploring the timing of the effect—namely, that the effect indeed occurs after the reform, and (as an important signal of our identification strategy being credible) that the effects we identify cannot be attributed to previous (pre-reform) innovation trends among the treated MNE subsidiaries. This is somewhat empirically challenging in our setting given that some reforms are clustered back-to-back in time. Nevertheless, we perform a number of tests, including Monte Carlo simulations, to explore our treatment's dynamic effects both before and after reforms. We are able to rule out the existence of pre-trends in knowledge production and find that the effects, indeed, show up in the estimations following the reforms, as expected. See appendix A for details and a summary of these results.

To tease out the mechanisms behind these findings, table 5 tests the effect of reforms on the patents filed by teams of inventors that include at least one GMI (direct effect) and on patents filed by teams that include only never-movers (spillover effect). Once again, we report results for the same three categories of patents. Exposure is associated with 29% additional patents overall, 8.6% additional GCPs, and 34% additional domestic patents within GMI teams. Positive reforms significantly increase the number of patents filed by exposed subsidiaries, and this is driven by both significant increases in their filed GCPs (+0.6%) and in their filed domestic patents (+2%). On the contrary, the number of patents and GCPs filed by teams of never-movers do not react to positive migration reforms and to the associated inflow of GMIs. Strikingly, when it comes to negative reforms, we find significant effects on both patents that directly involve GMIs and patents that do not, and the magnitude of the effect of the second is larger. Negative reforms are associated with a decrease of 7% in patenting within GMI teams in subsidiaries with 1 standard deviation higher exposure and with a decrease of 17% in patenting within never-mover teams. Given that the effect of exposure alone is not the same across the two types of teams, we can compare the coefficients of the effect of reforms as the percent of additional effect with respect to the effect of exposure alone. If we do that, we find that negative reforms diminish by 24% the advantage in GMI patenting of higher exposed subsidiaries, and diminish by 26% their advantage in non-GMI patenting. We interpret these results as evidence that decreasing the presence of mobile inventors generates large negative spillovers on the innovation produced by teams of never-movers.

Table 6 reports the results for our four measures of patent quality scaled by the number of patents: generality, originality, radicalness, breakthroughs, and number of citations. For the sake of conciseness, we present only the results for the aggregate number of patents. Higher inventor mobility overall—our measure of exposure—is associated with higher originality, radicalness, and number of citations per patent; it is not correlated with either generality and share of breakthrough patents. Positive reforms do not appear to significantly improve the quality of innovations produced and, if anything, they have a mild negative effect on originality. Negative reforms significantly decrease the number of citations per patent (-1.4%) but do not affect other measures of quality. These results suggest that reforms affecting the mobility of inventors mostly affect the number of patents filed, not necessarily their quality.

One might wonder about the economic significance of these results. We compute some simple back-of-the-envelope calculations to discover how much of the observed growth in patenting over the period is explained by migration policies. We estimate the main model reported in Equation 1 on the number of patents filed by each subsidiary $f$ and recover the estimated effect of the reforms by multiplying $\beta_2$ and $\beta_3$ by the subsidiary exposure $exp_{fct}$ and the cumulative count of positive and negative reforms respectively ($Pref_{ct}$ and $Nref_{ct}$). We then aggregate the effect of reforms over the entire sample and subtract it from the observed outcomes. This exercise is not a perfect counterfactual analysis since it assumes the absence of spillovers and general equilibrium effects. However, we think it can provide a useful benchmark to interpret the magnitude of our results. Figure 2 shows the graph obtained from this exercise. Overall, in the absence of all reforms, the total number of patents filed at the end of our period would have been very similar to what we actually observe in the data (figure 2a). The latter is explained by the fact that the effect of positive and negative reforms counterbalance each other, since over the period we have many more positive reforms than negative ones, but each negative reform has a stronger effect. If only negative reforms had been avoided, we would have observed 35% more patents by the end of the period, while if only positive reforms had been avoided, we would have observed 39% fewer patents in 2013 (figure 2b).

## 4.3 Robustness Tests

### 4.3.1 Exogeneity of the Exposure Measure

Appendix tables B2, B3, and B4 present the regressions relying on a measure of exposure computed as the mobility of inventors within the MNE observed over the period going from $t-6$ to $t-10$. The additional time lag reinforces the hypothesis that such a measure is exogenous to current patenting trends, but loses some variation since a larger portion of MNEs in the sample is unobserved so far back in time (in which case we assign an exposure of 0). In addition to the reduced-form results, we also present IV coefficients where current exposure is instrumented by historic exposure. The reduced-form results using historic exposure are similar in significance and magnitude to our preferred specification. The effect of negative reforms on GCPs loses some precision but remains marginally significant, and the effect of positive reforms becomes significant on the overall sample. The IV coefficients are, in general, larger. If we interpret the effect of reforms in terms of percentage change relative to the effect of exposure alone, we find that positive reforms increase the advantage of more exposed subsidiaries by an additional 5% in overall patenting, while negative reforms decrease that advantage by 13%. We confirm the finding that negative reforms have stronger effects on average than positive reforms when using historic exposure.

Appendix tables B5 and B6 perform two placebo tests to ensure that our measure of exposure is not correlated with differential trends in patenting that are unrelated to the reforms. In the first placebo test (Table B5), we randomly assign 49 positive and 13 negative fictitious reforms over the sample of 15 countries and 26 years (following the actual number and types of reforms), and then we run our main specification on this modified dataset. We repeat the operation over 1,000 replications, and we report the mean of the three coefficients of interest as well as the bootstrapped standard errors. In the second placebo test (appendix table B6), we do the same procedure, but we randomly assign 61 fictitious reforms to our country-year sample, randomly classifying them as positive or negative, therefore relaxing further the structure of the data by not imposing a fixed number of positive and negative events. Both of these exercises result in small and insignificant coefficients associated with positive and negative pseudo-reforms, while the exposure coefficient alone remains significantly positive and similar in magnitude to the one obtained in the main analysis, as expected. These placebos confirm that exposure alone is not associated with differential time trends if not interacted with the timing of actual reforms.

To ensure that our results are picking up independent effects of positive and negative reforms, we compare them to regressions introducing positive and negative reform counts separately. Appendix tables B7, B8, B9, and B10 present the results for the main outcomes, the direct effects on patents filed by teams with GMIs, the spillover effects on patents filed by teams of never-movers, and the quality of patents produced. The sign and significance of the results are very similar to the ones in the main analysis.

#### 4.3.2 Intensity of Reforms

A caveat in our analysis is that we cannot disentangle whether the stronger effect of negative reforms is driven by differences in reform intensity or asymmetric effects within a similar intensity. Keeping this caveat in mind, we can look at the heterogeneity of the effects across reforms affecting long- versus short-term stays and reforms affecting volume versus rights of migrants. Appendix tables B11 and B12 introduce a separate patent count for reforms affecting permanent migration—defined as changing the conditions for migrants staying more than one year in the country—and for reforms affecting short stays of less than one year. The distinction between these two categories is presented in table 1. Results show that all of the effects are driven by permanent reforms. This finding confirms that policies affecting GMIs' long-term stays have a more intensive effect on subsidiary innovation. Even among reforms affecting long-term migration, we find that our negative reforms have stronger impacts than positive ones. This rules out the possibility that the stronger effect of negative reforms is explained by a different composition of permanent and temporary reforms.

Appendix tables B13 and B14 evaluate whether the intensity of the effect is heterogeneous depending on whether reforms affect the quotas of foreigners allowed into the country or the rights that such foreigners have once they have moved. Results show that all of the effects are driven by volume reforms. Among reforms affecting quotas, we find that our negative reforms have stronger impacts on domestic patents than the positive ones do, while the effect of GCPs is more similar. This again rules out the possibility that the stronger effect of negative reforms on overall patents is explained by a different composition of rights and volume reforms.

Appendix tables B15 and B16 test the heterogeneity of the effect across MNE size. We split the sample in half according to the average number of subsidiaries that an assignee has over the period. Small MNEs are those that file seven patents in a given year on average, while large MNEs are those that file 48 patents in a given year on average. Findings show that large MNEs benefit much more from positive reforms and suffer less from negative reforms. In fact, the effect of positive and negative reforms is symmetric on large MNEs: a positive reform increases the patenting of subsidiaries with a 1 standard deviation higher exposure by 7.6%, while a negative reform decreases it by 8%. On the contrary, small MNEs do not benefit at all from positive reforms—the coefficient is even negative—and suffer more than twice as much from negative reforms as do large firms. This result signals that large firms are able to take greater advantage of positive migration reforms and are somewhat protected from the detrimental impact of negative reforms. Small firms are less able to profit from increases in access to global mobile inventors, but suffer significantly from reforms restricting such access. Thus, it seems that migration reforms widen the inequality in patenting between large and small multinational companies. This result also hints toward the fact that the stronger effect of negative reforms is driven by the heterogeneity in the reaction of small firms, rather than by intrinsic differences of these reforms that would affect all MNEs equally.

#### 4.3.3 Selection of the Sample

We test the sensitivity of our results to excluding one of the major countries from the sample. Each column of appendix table B17 reports the effect obtained after the sequential exclusion of one of the nine countries that account for more than 5,000 observations in the data.[25] We present results for the total number of patents (panel A), GCPs (panel B), and domestic patents (panel C). The coefficient associated with negative reforms is significant across all samples, and the magnitude of the effect on total number of patents is stable across regressions, except for the sample excluding Japan, where the magnitude halves in size (goes from 15% in the main sample to 8%). The effect of positive reforms on GCPs is significant in five of the nine samples (when China, Germany, Korea, Taiwan, or the U.K. are excluded), and the magnitude of the effect is comparable across all samples. Overall, these results confirm that our coefficients are not driven by one particular country in the sample.

Next, we explore how our coefficients vary after sequentially excluding each one of the three major regions in our sample: North America, Europe, and Asia. Results are provided in appendix table B18. Excluding Europe leaves the results largely unchanged. Excluding North America increases the strength and magnitude of positive reforms and decreases the strength and magnitude of one of the negative reforms. This suggests that positive reforms are particularly effective in regions other than North America, while negative reforms are particularly damaging in North America. This is in line with the finding that positive reforms have been particularly beneficial for emerging

[25]The countries with more than 5,000 observations in our sample are Canada, China, Germany, India, Japan, South Korea, Taiwan, the United Kingdom, and the United States.

markets (which are presented in the next section). Finally, excluding Asia reduces the magnitude of the effect of negative reforms, and the results are qualitatively similar—except for the effect of positive reforms on domestic patents, where the sign flips. The latter can be decomposed into a zero effect on teams including GMIs and a negative effect on teams of never-movers.

#### 4.3.4 Attrition

By construction, in our data we observe only subsidiaries that file at least one patent in a given year. Consequently, our estimates on the total number of patents have to be interpreted as the effect on the intensive margin: reforms affect the quantity of inventors migrating and the amount of patents filed among subsidiaries that do patent. In order to explore whether attrition in the sample is affecting our results, we input subsidiaries in the years when they do not patent if the MNE is observed patenting in other countries. For these observations, all the patent counts are set to zero. We then estimate the effect combining the intensive and the extensive margin by applying the same model to the new data. Given that our outcomes are modified using the arcsinh transformation, they are defined in zero. Appendix tables B19 and B20 present the results. The effect of negative reforms on GCPs loses significance in the overall sample, but the other coefficients remain unchanged, suggesting that attrition is not the main driver of our results.

#### 4.3.5 Extensions

Appendix section C.1 explores productivity outcomes at the inventor and subsidiary levels. We show that GMIs' productivity increases after they move to their destination countries. Further, positive migration reforms increase the number of GCPs per inventor filed in the subsidiary, but have no effect on domestic patent per inventor or overall patent productivity. Negative migration reforms decrease domestic patent productivity but increase GCP productivity, hinting that inventors still in the subsidiary might continue to collaborate with their GMI colleagues that have left. Moreover, we find that the negative effect of restrictive migration policies on the number of patents filed by each subsidiary is partly explained by changes in the number of inventors—both

GMI and domestic inventors—that patent there. That is, consistent with Kerr et al. (2015), we find suggestive evidence of strong complementarity in production between mobile and domestic human capital. Taken together, these findings suggest that our results on the number of patents filed by MNE subsidiaries are explained by both changes in the average productivity of inventors and changes in the number of inventors.

# 5 Changes in the Geography of Knowledge Production

One of the most important questions we can answer in our setting is whether human mobility—facilitated or hindered by the migration reforms in our sample—explains shifts in the geography of global knowledge production. Figure 1d shows that during our period of interest, emerging markets such as China, Korea, Taiwan, and India increased drastically their share of total patent production, at the expenses of advanced countries such as the United States, Japan, and Germany. We investigate the role mobility policies played by estimating our main model on the share of total yearly patents filed by each subsidiary—a measure of global innovation share—and by evaluating the heterogeneity of the effect across countries with initially high and low shares of global patent production. In particular, we measure the initial share of global innovation by computing the total number of patents filed from 1985 to 1990 by each country in our data as a share of the total. The United States, Japan, and Germany are the countries with (by far) the highest initial shares of global patents, and they account for 67% of our sample. We treat all the other countries in our dataset as "low initial share." We then reestimate our main specification by adding a triple interaction as follows:

$$\begin{aligned} Y_{fct} = \beta_0 + \beta_1 exp_{fct} + \beta_2(exp_{fct} \times PRef_{ct}) + \beta_3(Exp_{fct} \times NRef_{ct}) + \beta_4(exp_{fct} \times LIS_c) \\ + \beta_5(exp_{fct} \times LIS_c \times PRef_{ct}) + \beta_6(exp_{fct} \times LIS_c \times NRef_{ct}) + \gamma_{ct} + \delta_{ft} + \epsilon_{fct}, \quad (2) \end{aligned}$$

where $Y_{fct}$ captures the share of total patents filed in year $t$ across all countries in the sample coming from subsidiary $f$ in country $c$, and $LIS_c$ is a binary indicator identifying countries with low initial shares in global patent production. Table 7 reports the results from estimating the baseline model reported in Equation 1 as well as the triple interactions reported in Equation 2. Column (1) of table 7 shows that positive reforms do not significantly impact the share of total patents filed by a subsidiary, but negative reforms do decrease it significantly. We find similar results for GCPs and domestic patents when considered separately (Columns (3) and (5)). Interestingly, results are highly heterogeneous across the initial share of innovation. Countries that counted very little in global knowledge production at the beginning of the period gain significantly more following positive migration reforms, while the initial leaders in knowledge production appear to lose more following negative migration reforms even if the difference is not significant. This result highlights how policies encouraging inventor mobility effectively helped emerging markets gain importance in the geography of global innovation. We observe these patterns once again for both GCPs and domestic patents (columns (4) and (6)).

To get a sense of the economic significance of these effects relative to overall shifts in the distribution of patents, we compute some simple back-of-the-envelope calculations. We want to determine how much of the observed growth in the share of patents filed by emerging markets is explained by migration policies. We follow a similar procedure as with total patents by using our triple interaction model to predict the effect of positive and negative reforms on the share of global patents filed by each subsidiary $f$ located in a country with low initial shares. We then use them to calculate the total effect of reforms on the share of total patents filed by each country $c$ within the low initial share group in year $t$ as follows:

$$\left(\frac{PA\hat{T}_{ct}}{PAT_t}\right) = \sum_{f=1}^{F} exp_{fct}\Big((\beta_2 + \beta_5)PRef_{ct} + (\beta_3 + \beta_6)NRef_{ct}\Big). \tag{3}$$

Finally, we compute the predicted aggregate trends in the geography of innovation in the absence of migration reforms by subtracting $\frac{PA\hat{T}_{ct}}{PAT_t}$ from the actual share observed in each country $\frac{PAT_{ct}}{PAT_t}$ and aggregating it over all countries with low initial shares. Figure 3a shows that countries with low shares of patents at the beginning of the period would have grown only from roughly 5% to 19% of total innovation in the absence of migration reforms, while the actual change that occurs over the period brings them to 25% of total innovation. Figure 3b further distinguishes between the predicted outcome in absence of positive migration reforms and in absence of negative migration reforms, showing that positive reforms have substantially helped these countries become leading inventors. If emerging markets would not have adopted any negative migration reform, they would have reached up to 30% of patents filed by 2015. On the contrary, if they would have adopted only negative migration reforms (but no positive ones), they would have remained at 12% of total knowledge production. These results strongly suggest that policies favoring human mobility have helped emerging markets in their global innovation race. Migration reforms are, thus, crucial elements in helping us understand global trends in the geography of innovation over the past decades. Appendix figure B4 disaggregates the comparison between actual and predicted trends by country, showing that positive migration reforms generated a particularly large boost for China and Korea.

# 6 Conclusion

The impressive rise of China and India as destinations for the production of global innovation in the past two decades has often been attributed to MNEs shifting their patenting activity toward these countries. MNEs' innovative capacity is increasingly recognized to rely on the knowledge and absorptive capacity of its local subsidiaries. In this context, we highlight inventor' cross-border mobility as a key mechanism for MNE subsidiaries developing absorptive capacity and global knowledge production, but evidence of this relationship remains thin. The purpose of this study is to explore this interrelationship. Specifically, we do so through investigating whether and to what extent MNEs' subsidiary-level investments in innovation change following migration reforms that either ease or reinforce barriers to immigration into the country. We match the full list of business-related migration reforms adopted since 1990 within 15 countries to the patenting activities of the country-level MNE subsidiaries identified in the database of USPTO patents.

We find that pro-business migration reforms significantly increase MNE innovation within a country, especially in terms of GCPs, while reforms that discourage migration lead to significant declines in both domestic patents and GCPs. The effect of positive reforms is driven by teams involving GMIs; the effect of negative reforms is driven by a change in innovation produced by teams that directly involve GMIs as well as by domestic teams entirely composed of never-movers. This highlights the presence of important spillovers associated with inventors' mobility. Finally, positive migration reforms help explain the increased importance of emerging markets in global knowledge production, while negative migration reforms were a setback for historical leaders in the innovation race. This finding suggests that policies affecting human mobility have contributed to the shift in the geography of innovation toward emerging markets.

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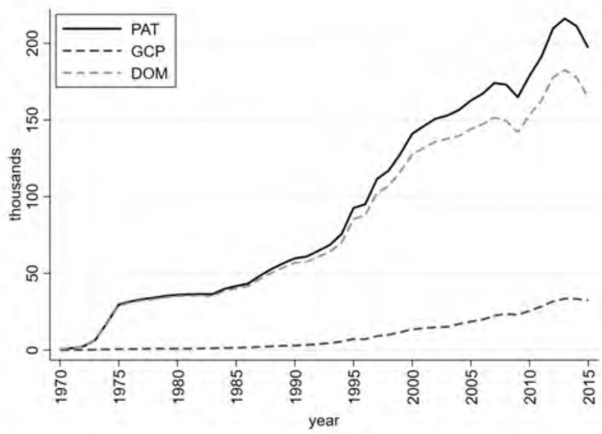
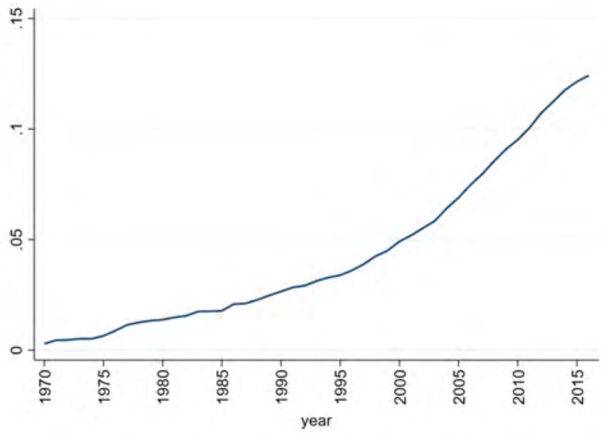
**Figure 1:** GLOBAL TRENDS IN PATENTING AND MIGRATION

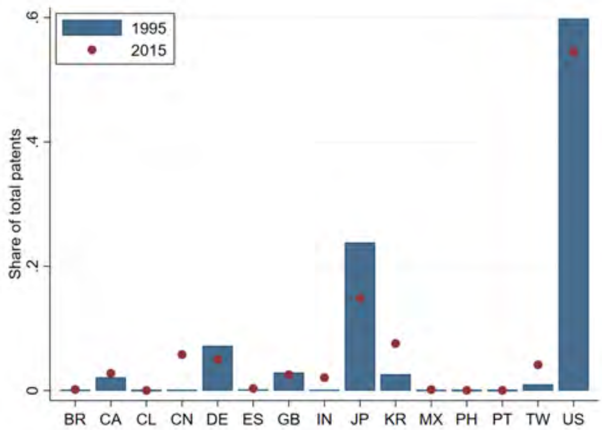
**(a)** Number of patents



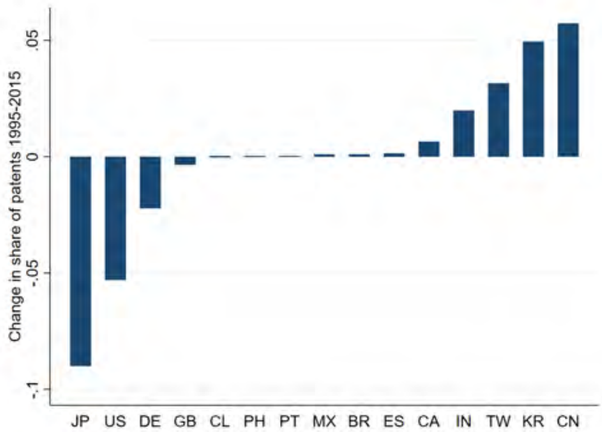

**(b)** Share of GMIs




**(c)** Share of global patents by country



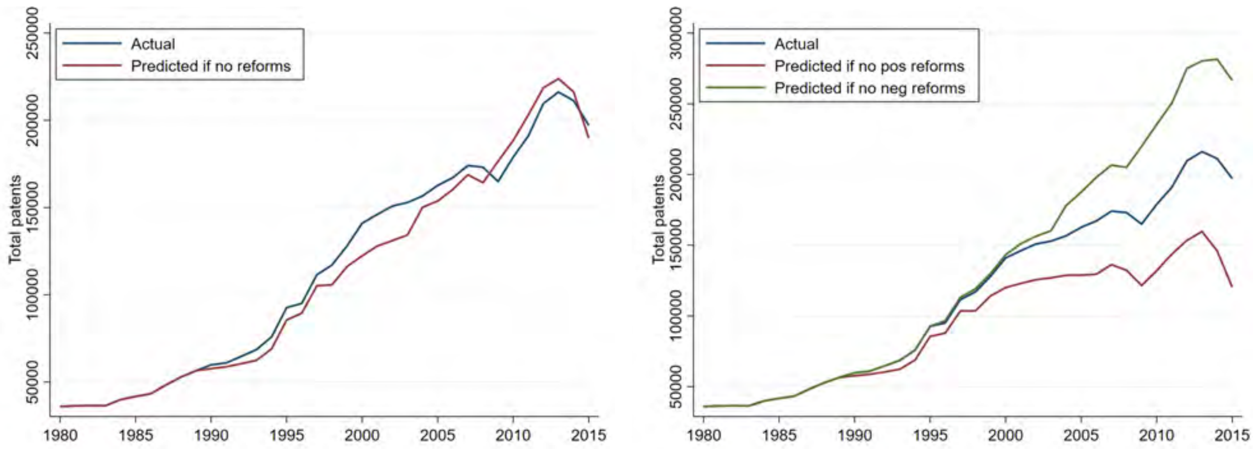

**(d)** Change in share of global patents by country




Panel (a) shows the evolution of the total number of patents reported in the USPTO data (solid line), as well as the breakdown between domestic patents and GCPs. Panel (b) shows the evolution of the share of global migrant inventors out of the total population of inventors. An inventor is considered a GMI if he or she is observed patenting in a different country with respect to the first country of appearance in the data. Panel (c) shows the share of total patents in the sample filed by each country in 1995 and 2015, and Panel (d) shows the change in that share.

**Figure 2:** PREDICTED AGGREGATE TRENDS IN TOTAL PATENTS

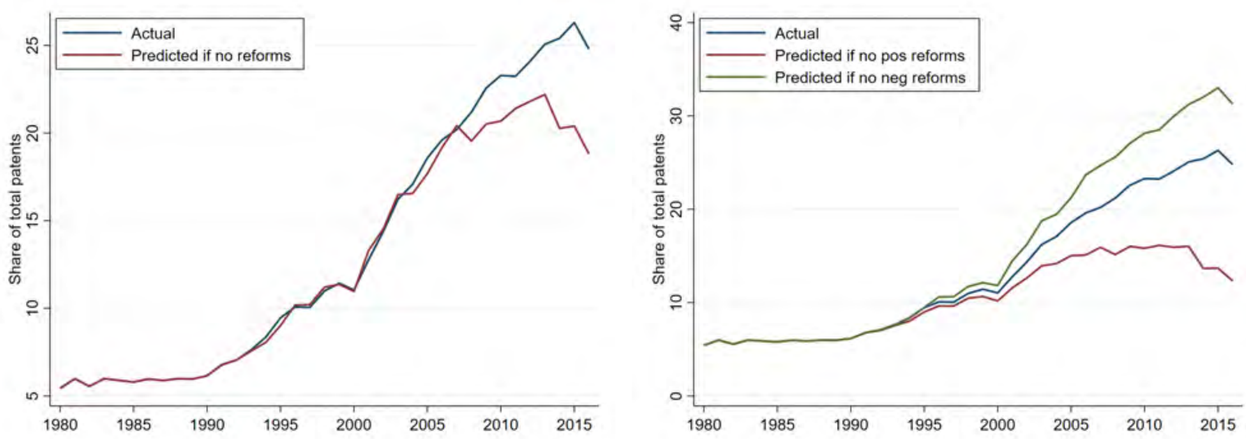
**(a)** Effect of all reforms **(b)** Effect of positive and negative reforms




The actual outcomes are the total patents filed in our sample across the period of interest. We obtain the predicted outcomes by subtracting the predicted effect of positive and negative migration reforms from the actual outcomes.

**Figure 3:** PREDICTED AGGREGATE TRENDS OBSERVED IN COUNTRIES WITH LOW INITIAL SHARES OF PATENTS

**(a)** Effect of all reforms **(b)** Effect of positive and negative reforms




The actual outcomes are the share of total patents observed in countries with low initial shares across the period of interest (includes all countries except the U.S., Japan, and Germany). We obtain the predicted outcomes by subtracting the predicted effect of positive and negative migration reforms from the actual outcomes.

**Table 1:** List of migration reforms by country

| Country | Positive Business Reforms | Negative Business Reforms | Permanent Positive Business Reforms | Permanent Negative Business Reforms |
|---|---|---|---|---|
| Brazil | 2014 | - | - | - |
| Canada | - | 2001 | - | 2001 |
| Chile | 2005 | - | 2005 | - |
| China | 1994, 2004, 2008, 2013 | 1996 | 1994, 2004, 2008, 2013 | 1996 |
| Germany | 2000, 2005, 2012, 2016 | 2004 | 2005, 2012, 2016 | - |
| Spain | 1996, 2003, 2009 | - | 1996, 2003, 2009 | - |
| United Kingdom | 2006 | 1996, 2006 | 2006 | 1996, 2006 |
| India | 2005, 2016 | - | 2005 | - |
| Japan | 1992, 1993, 2010, 2012, 2014, 2015 | - | 1992, 1993, 2010, 2012, 2014 | - |
| Korea | 1991, 1992, 1993, 1994, 1995, 1996, 1998, 1999, 2002, 2004, 2007, 2009, 2010 | - | 1998, 2009, 2010 | - |
| Mexico | 2010, 2011, 2014 | 2012 | 2010, 2011, 2014 | - |
| Philippines | 1996, 2002, 2013 | 2009, 2012, 2015 | 1996 | 2009, 2012, 2015 |
| Portugal | 2001, 2012 | 2003 | 2001 | 2003 |
| Taiwan | 2014, 2015 | 1992 | 2014, 2015 | - |
| United States | 1990, 1998, 2000, 2015 | 2004, 2009 | 1990, 1998, 2000, 2015 | 2004, 2009 |
| Total N. of reforms | 49 | 13 | 32 | 10 |

This table details the year of implementation for each of the 61 reforms enacted over the period of interest and reports the sub-sample of them that affect stays of one year or longer (called "permanent"). The reform introduced in the United Kingdom in 2006 has both positive and negative elements and is, thus, double counted in this table.

**Table 2:** Summary statistics of main outcomes

| | Full sample | | Low exposure subsidiaries | | High exposure subsidiaries | |
|---|---|---|---|---|---|---|
| VARIABLES | Mean | (SD) | Mean | (SD) | Mean | (SD) |
| N. of patents | 12.9 | (86.4) | 5.6 | (30.6) | 18.3 | (156.4) |
| N. of GCP | 1.5 | (8.9) | 0.6 | (3.9) | 1.2 | (16.1) |
| N. of domestic patents | 11.3 | (80.9) | 5.0 | (26.7) | 18.1 | (146.4) |
| *Patents by teams with at least one migrant inventor* | | | | | | |
| N. of patents | 2.8 | (23.5) | 0.9 | (7.3) | 4.6 | (42.8) |
| N. of GCP | 0.8 | (5.5) | 0.2 | (2.2) | 0.8 | (10.0) |
| N. of domestic patents | 1.9 | (19.7) | 0.7 | (5.1) | 4.4 | (35.8) |
| *Patents by teams without any migrant inventor* | | | | | | |
| N. of patents | 10.1 | (67.6) | 4.7 | (23.3) | 15.7 | (122.2) |
| N. of GCP | 0.7 | (3.8) | 0.4 | (1.6) | 0.9 | (6.8) |
| N. of domestic patents | 9.4 | (65.5) | 4.4 | (21.6) | 15.6 | (118.4) |
| *Quality of patents* | | | | | | |
| Average patent generality | 0.51 | (0.22) | 0.52 | (0.23) | 0.51 | (0.21) |
| Average patent originality | 0.77 | (0.16) | 0.77 | (0.17) | 0.77 | (0.14) |
| Average patent radicalness | 0.39 | (0.22) | 0.40 | (0.22) | 0.38 | (0.20) |
| Share of breakthrough patents | 0.01 | (0.08) | 0.01 | (0.09) | 0.01 | (0.08) |
| N. of citations per patent | 14.52 | (37.08) | 14.70 | (38.69) | 14.08 | (32.88) |
| *Migrant inventors* | | | | | | |
| N. of migrant inventors | 1.4 | (9.3) | 0.6 | (3.6) | 2.4 | (16.7) |
| Share of migrant inventors | 0.13 | (0.27) | 0.10 | (0.20) | 0.24 | (0.31) |
| N. observations | 297,919 | | 211,605 | | 86,314 | |

Summary statistics computed over the sample of subsidiaries, identified by MNE x country pair, in the sample spanning from 1990 to 2016.

**Table 3:** First-stage regressions

| | (1) | (2) |
|---|---|---|
| | asinh N. Migrant Inventors | |
| VARIABLES | OLS, Cont. Exp. | OLS, Hist. Exp. |
| Exposure x positive business reform | 0.0176** | 0.0206*** |
| | (0.00685) | (0.00767) |
| Exposure x negative business reform | -0.0623*** | -0.0356*** |
| | (0.0108) | (0.0113) |
| Exposure | 0.214*** | 0.223*** |
| | (0.0183) | (0.0207) |
| Observations | 166,360 | 166,360 |
| R-squared | 0.500 | 0.507 |

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the subsidiary level. Period of analysis: 1990-2016. Continuous exposure to the reforms is computed as the mobility rate of inventors observed within all the other subsidiaries of the MNE over the preceding 5 years, and then standardized to have mean 0 and standard deviation of 1. Historical exposure is computed in the same way but over the period going from 5 to 10 years prior to the observation.

**Table 4:** Main results

| | (1) | (2) | (3) |
|---|---|---|---|
| | asinh Patents | asinh GCP | asinh Domestic Patents |
| VARIABLES | OLS | OLS | OLS |
| Exposure x positive reforms | 0.00940 | 0.00575* | 0.00737 |
| | (0.00877) | (0.00345) | (0.0102) |
| Exposure x negative reforms | -0.149*** | -0.0219*** | -0.175*** |
| | (0.0141) | (0.00603) | (0.0169) |
| Exposure | 0.565*** | 0.111*** | 0.722*** |
| | (0.0246) | (0.0104) | (0.0294) |
| Observations | 166,360 | 166,360 | 166,360 |
| R-squared | 0.524 | 0.666 | 0.507 |

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.
Standard errors clustered at the subsidiary level. MNE x year fixed effects and country x year fixed effects included in all regressions. Period of analysis: 1990-2016. Continuous exposure to the reforms is computed as the mobility rate of inventors observed within all the other subsidiaries of the MNE over the preceding 5 years.

**Table 5:** Direct and spill-over effects

| | (1) | (2) | (3) |
|---|---|---|---|
| | asinh Patents | asinh GCP | asinh Domestic Patents |
| VARIABLES | OLS | OLS | OLS |
| *Panel A: Patents by teams with at least one GMI* | | | |
| Exposure x positive reforms | 0.0185** | 0.00637** | 0.0202** |
| | (0.00775) | (0.00324) | (0.00884) |
| Exposure x negative reforms | -0.0706*** | -0.0120** | -0.0740*** |
| | (0.0117) | (0.00582) | (0.0136) |
| Exposure | 0.290*** | 0.0861*** | 0.341*** |
| | (0.0200) | (0.0100) | (0.0237) |
| Observations | 166,360 | 166,360 | 166,360 |
| R-squared | 0.583 | 0.694 | 0.445 |
| *Panel B: Patents by teams with no GMIs* | | | |
| Exposure x positive reforms | 0.0130 | 0.00374 | 0.0103 |
| | (0.00943) | (0.00278) | (0.00993) |
| Exposure x negative reforms | -0.170*** | -0.0204*** | -0.188*** |
| | (0.0155) | (0.00461) | (0.0168) |
| Exposure | 0.646*** | 0.0826*** | 0.713*** |
| | (0.0272) | (0.00821) | (0.0296) |
| Observations | 166,360 | 166,360 | 166,360 |
| R-squared | 0.519 | 0.647 | 0.501 |

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.
Standard errors clustered at the subsidiary level. MNE x year fixed effects and country x year fixed effects included in all regressions. Period of analysis: 1990-2016. Continuous exposure to the reforms is computed as the mobility rate of inventors observed within all the other subsidiaries of the MNE over the preceding 5 years. Outcomes are divided into teams where at least one inventor is a GMI (has patented in a different country in earlier years) and teams of never-moving inventors.

**Table 6:** Results on patent quality

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | asinh Generality per Patent | asinh Originality per Patent | asinh Radicalness per Patent | asinh Share of Breakthrough Patents | asinh Citations per Patents |
| VARIABLES | OLS | OLS | OLS | OLS | OLS |
| Exposure x positive reforms | 5.88e-05 | -0.000366** | -9.76e-05 | -9.59e-05 | 5.69e-05 |
| | (0.000256) | (0.000175) | (0.000282) | (0.000116) | (0.00183) |
| Exposure x negative reforms | -0.000292 | -0.000169 | -0.00106 | -0.000110 | -0.0143*** |
| | (0.000661) | (0.000440) | (0.000683) | (0.000442) | (0.00495) |
| Exposure | -0.000619 | 0.00118* | 0.00347*** | -0.000123 | 0.0685*** |
| | (0.000936) | (0.000656) | (0.000976) | (0.000445) | (0.00605) |
| Observations | 129,929 | 144,936 | 144,952 | 146,221 | 146,221 |
| R-squared | 0.729 | 0.728 | 0.704 | 0.719 | 0.772 |

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.
Standard errors clustered at the subsidiary level. MNE x year fixed effects and country x year fixed effects included in all regressions. Period of analysis: 1990-2016. Exposure to the reforms is computed as the mobility rate of inventors observed within all the other subsidiaries of the MNE over the preceding 5 years and then standardized to have mean 0 and standard deviation of 1. Columns (1), (2), and (3) weight the count of the number of patents by the generality, originality, and radicalness coefficients, respectively, and then divide them by the patent count. Column (4) computes the share of patents that are considered breakthrough. Column (5) computes the number of citations per patent.

**Table 7:** Effect on geography of knowledge production

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Share of Total Patents Filed by Subsid. | | Share of Total GCPs Filed by Subsid. | | Share of Total Domestic Patents Filed by Subsid. | |
| VARIABLES | OLS | OLS | OLS | OLS | OLS | OLS |
| Exposure | 0.0201*** | 0.0321*** | 0.00630*** | 0.0116*** | 0.0212*** | 0.0332*** |
| | (0.00413) | (0.00604) | (0.00128) | (0.00251) | (0.00446) | (0.00632) |
| Exposure x low initial share | | -0.0207*** | | -0.00973*** | | -0.0210*** |
| | | (0.00561) | | (0.00263) | | (0.00587) |
| Exposure x positive reforms | 0.00162 | -0.00381*** | -3.88e-05 | -0.00228*** | 0.00199 | -0.00350*** |
| | (0.00141) | (0.00128) | (0.000348) | (0.000716) | (0.00159) | (0.00132) |
| Exposure x negative reforms | -0.00990*** | -0.00696*** | -0.00159*** | -0.000429 | -0.0108*** | -0.00784*** |
| | (0.00209) | (0.00168) | (0.000616) | (0.000451) | (0.00227) | (0.00186) |
| Exposure x pos. reforms x low initial share | | 0.00706*** | | 0.00277*** | | 0.00723*** |
| | | (0.00232) | | (0.000842) | | (0.00255) |
| Exposure x neg. reforms x low initial share | | 0.00308 | | 0.00126 | | 0.00356 |
| | | (0.00254) | | (0.000911) | | (0.00281) |
| Observations | 166,360 | 166,360 | 166,360 | 166,360 | 166,360 | 166,360 |
| R-squared | 0.246 | 0.248 | 0.361 | 0.362 | 0.242 | 0.244 |

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at the subsidiary level. MNE x year fixed effects and country x year fixed effects included in all regressions. Period of analysis: 1990-2016. Exposure to the reforms is computed as the mobility rate of inventors observed within all the other subsidiaries of the MNE over the preceding 5 years and then standardized to have mean 0 and standard deviation of 1. The outcomes measure the share of total patents, GCPs, and domestic patents produced in a year filed by each subsidiary. Low initial share identifies the 50% of the sample with the lowest share of global patents observed over the period 1985-1990.

# Online Appendix for

## *HUMAN MOBILITY AND THE GLOBALIZATION OF KNOWLEDGE PRODUCTION: CAUSAL EVIDENCE FROM MULTINATIONAL ENTERPRISES*

## A Validity of the Main Assumptions

The cardinal assumption of difference-in-differences estimations is the common trend hypothesis. Namely, it supposes that the treated group would have evolved following the same trend of the control group in the absence of the treatment event. In our context, this assumption supposes that subsidiaries with different levels of exposure to the reform would have shown similar trends in patenting in the absence of the reforms. This hypothesis is untestable given the fact that we cannot observe what would have happened in the years following a reform in the absence of the latter. What is typically shown in the literature to assess the plausibility of this assumption are the trends observed before the reform: if treated and control subsidiaries evolved following similar patterns prior to the introduction of the policy, we can reasonably imagine that they would have continued doing so if the reform would not have been introduced. In our context, we can test that the trends in patenting were uncorrelated with reform exposure during the years that preceded the first reform in each country by estimating the following model:

$$Y_{fct} = \beta_0 + \beta_1 exp_{fct} + \sum_{k=-3}^{-1} \mathbb{1}_{\{t_{Ref_c}+k=t\}} \tau_k exp_{fct} + \gamma_{ct} + \delta_{ft} + \epsilon_{fct}, \tag{A1}$$

where $\mathbb{1}_{\{t_{Ref_c}+k=t\}}$ is a series of dummies identifying the three years preceding the first reform in a given country $c$, $exp_{fct}$ captures the level of exposure of each subsidiary in the country, and $\tau_k$ recovers the differential trends correlated with exposure relative to t-3, which is normalized to zero. We estimate this model separately for positive and negative reforms, restricting each sample to the countries that experience at least one reform of that type.

Results for the three main outcomes are shows in figure A1. Given that none of the coefficients are statistically different from zero, we can conclude that subsidiaries differently exposed to the reforms followed similar patenting trends prior to the first policy change in our sample. It is common practice to show the coefficients associated with the years following the reform as well, in order to get a sense of the dynamic effects at play. In our context, given the presence of subsequent reforms within the same country that are sometimes clustered in time, we have to adopt a more complex strategy to show the dynamic effects. The latter is presented in subsection A.1.

The second central assumption in our strategy is that the average treatment effect of a given reform type is equivalent across events, which means that the magnitude of the effect of the first reform in a given country is comparable to the second reform, the second is comparable to the third, and so forth. To test this assumption, we estimate the following model:

$$Y_{fct} = \beta_0 + \beta_1 exp_{fct} + \sum_{r=1}^{2} \alpha_r Ref_{ct}^r \times exp_{fct} + \gamma_{ct} + \delta_{ft} + \epsilon_{fct}, \quad \text{(A2)}$$

where $r$ indexes up to two consecutive reforms of a given type (positive or negative) in a given country, $Ref_{ct}^r$ identifies the period in country $c$ after reform $r$ and prior to reform $r+1$ and $\alpha_r$ recovers the distinct effect of each subsequent reform from the first to the second.[26] We run the regression separately for positive and negative reforms on the sample of countries that experience at least one of them, and on the sample of years preceding the third reform of the same type within each country.

[26] We limit ourselves to two consecutive reforms because the sample of countries experiencing more than 2 reforms of the same type becomes very small.

The recovered coefficients are reported in figure A2. What we can observe is that the effect is slightly increasing in magnitude, with the second positive reform having a larger effect than the first and the second negative reform having a slightly larger effects than the first. Nonetheless, the 95% confidence intervals overlap, suggesting that the effects are comparable in terms of magnitude.

## A.1 Dynamic Effects

The standard model used to recover the dynamic treatment effects is the following:

$$Y_{fct} = \beta_0 + \beta_1 exp_{fct} + \sum_{k=-3}^{+3} \mathbb{1}_{\{t_{PRef_c}+k=t\}} \alpha_k exp_{fct} + \sum_{k=-3}^{+3} \mathbb{1}_{\{t_{NRef_c}+k=t\}} \theta_k exp_{fct} + \gamma_{ct} + \delta_{ft} + \epsilon_{fct}, \quad \text{(A3)}$$

where $k$ indexes time to the nearest reform, $\mathbb{1}(t_{PRef_c} + k = t)$ is a series of indicator variables indexing observations $k$ periods before or after a positive reform event, and $\mathbb{1}(t_{NRef_c} + k = t)$ is the equivalent for negative reforms. $exp_{fct}$ represents our exposure measure. Here, $\alpha_k$ and $\theta_k$ identify the dynamic marginal treatment effects of positive and negative reforms at event-time $k$ relative to an omitted baseline period (the year prior to reform enactment). This estimate can be thought of as a by-year estimate of the $\beta_2$ coefficients in equation 1 that comes at the expense of omitting information on reform events' links to all but the most proximate years.

In the ideal setting, we would estimate the model reported in equation A3 on the full sample, assigning the timing with respect to the closer reform. Nevertheless, in our case the high frequency of reforms observed in certain countries makes it really difficult to distinguish between pre- and post-periods. Thus, we adopt an alternative strategy: we perform a Monte Carlo simulation in which we randomly draw 1,000 times one single positive and one single negative reform for each country, which we use to estimate equation A3. We then take the average over the 1,000 different

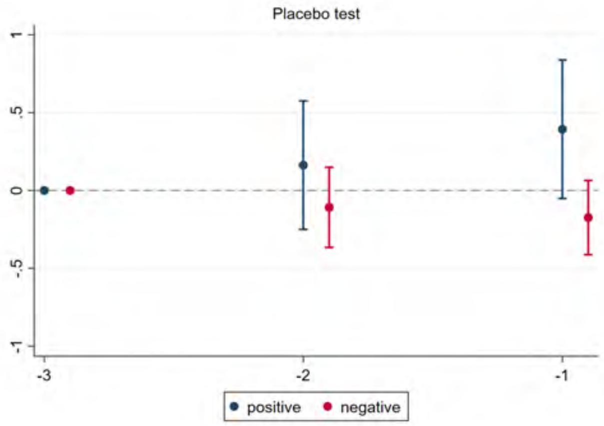
$\alpha_k$ and $\theta_k$ that we obtain, and we compute bootstrapped standard errors.[27]

Figure A3 plots the point estimates and corresponding 90% confidence intervals of $\alpha_k$ and $\theta_k$ for the three years leading to a reform and the three years following it. The year preceding the reform is used as a reference point. Figure A3a shows that patent production in subsidiaries with different levels of exposure followed the same exact trends in the years preceding a positive reform and, if anything, they showed slightly higher growth in the years preceding a negative reform. After the implementation of a positive policy, there is an increase in the number of patents filed by the subsidiary, but the effect on individual post-period years is not significant. After a negative reform, most exposed subsidiaries see a decline in patents compared to the rest, which becomes significant at t+3. When we disentangle between GCPs and domestic patents (figure A3b and figure A3c), we find no effect of positive reforms on GCPs and generally a larger effect in magnitude on domestic patents. These results are broadly consistent with the main (static) analysis, but with the difference that the majority of the coefficients on individual post-period years are insignificant. This might be explained by the fact that positive reforms have a significant effect if all post-reform years are considered together (including long-term effects), but not if individual years are considered separately. This exercise also underlines the difficulty performing the standard event study analysis in a context including multiple reforms clustered in time.

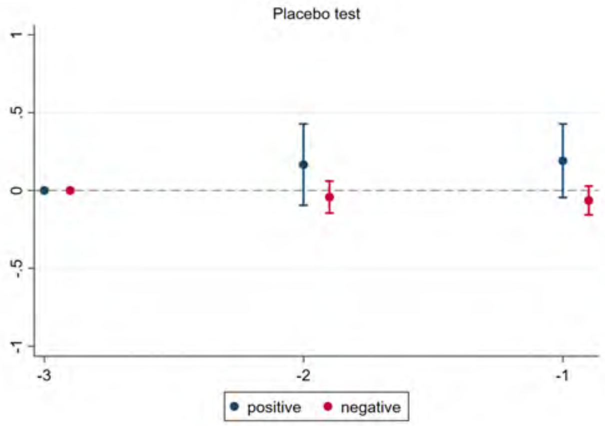
[27]In countries where both positive and negative reforms take place, each time we draw one from each of the two types. For the others, we draw from only the reform type that they have. In order to maintain all the observations in the regressions, for countries without positive reforms, we set all the time-to-reform dummies to 0, and we do the same for countries without negative reforms.

**Figure A1:** Test for pre-trends

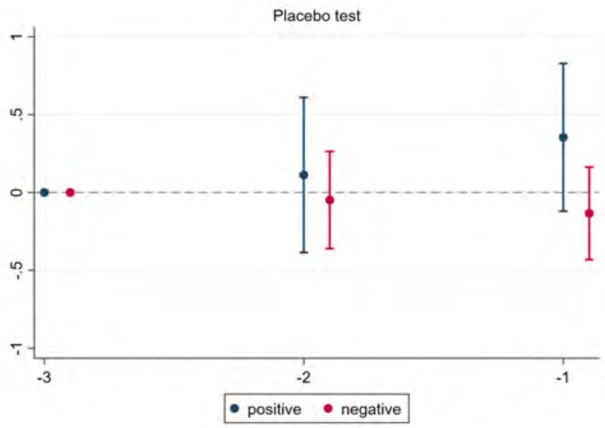
**(a)** asinh Patents




**(b)** asinh GCPs



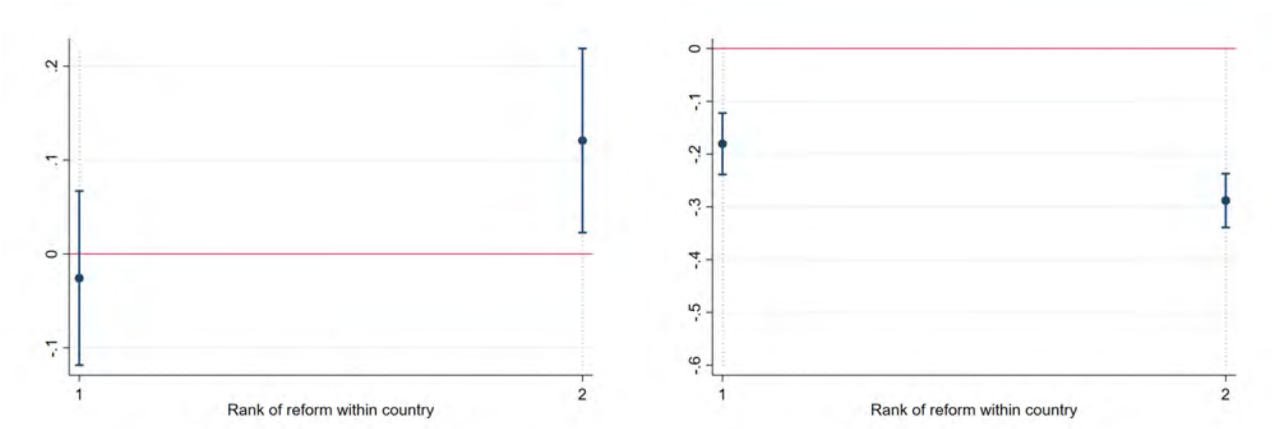

**(c)** asinh Domestic Patents




These graphs plot the dynamic effects obtained by running Equation A1 on the 3 years preceding the first reform in each country. Time t-3 is normalized to zero. The model is estimated separately for positive and negative reforms. The bars represent the 95% confidence intervals.

**Figure A2:** Test for equivalence of effect across subsequent reforms

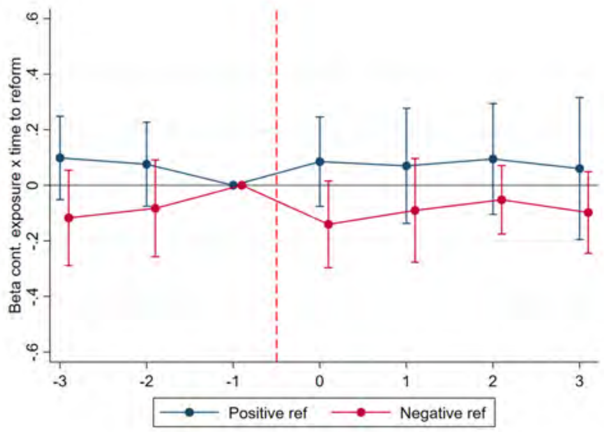
**(a)** asinh Patents, subsequent positive reforms **(b)** asinh Patents, subsequent negative reforms



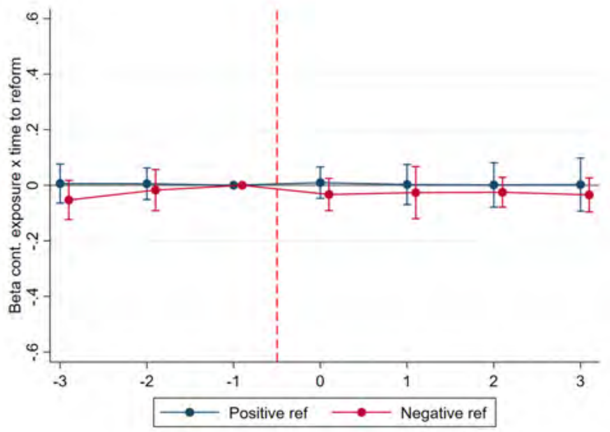

These graphs plot the separate effect of the first and second reform taking place in a country obtained by running equation A2 on the sample cut before the third subsequent reform. The model is estimated separately for positive and negative reforms. The bars represent the 95% confidence intervals.

**Figure A3:** Dynamic effect of reforms

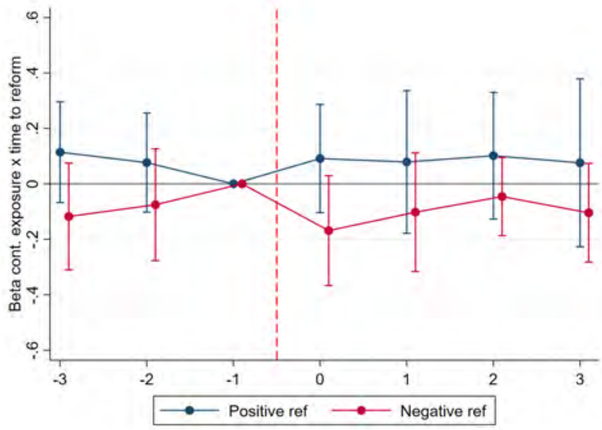
**(a)** asinh Patents




**(b)** asinh GCPs




**(c)** asinh Domestic Patents




These graphs plot the dynamic effects obtained by running equation A3 on the three years preceding and the three years following the reforms, for total number of patents (panel a), GCPs (panel b), and domestic patents (panel c). The bars represent the 90% confidence intervals. Instead of estimating the model on the full sample of reforms, the graph is obtained by running a Monte Carlo simulation on 1,000 random samples where one positive and one negative reform are picked for each country and by averaging the effect over all of them.

# B Additional Tables and Figures

## B.1 Tables

Table B1 displays the frequency of subsidiaries and patents of the different types across the reform countries during the years of the sample. There is substantial heterogeneity among the presence of MNE subsidiaries across the countries, with Western countries (e.g., Germany, the United Kingdom, the United States, etc.) showing the largest frequency of MNE implantation, followed by Asian countries (e.g., China, Japan, and Taiwan). Additionally, certain countries produce global collaborative patents at greater rates than domestic patents and at significantly higher rates than those found in Kerr and Kerr (2018). It is the case for Chile, Spain, Mexico, the Philippines, and Portugal, thus underlining wide heterogeneity in knowledge production strategies.

Tables B2, B3, and B4 present the regressions relying on a measure of exposure computed as the mobility of inventors within the MNE across all the other subsidiaries observed over the period going from $t-6$ to $t-10$. The additional time lag reinforces the hypothesis that such a measure is exogenous to current patenting trends, but loses some variation since a larger portion of MNEs in the sample is unobserved so far back in time (in which case we assign an exposure of 0). In addition to the reduced-form results, we also present IV coefficients where current exposure is instrumented by the historic exposure. The reduced-form results using historic exposure are similar in significance and magnitude to our preferred specification. The effect of negative reforms on GCPs loses some precision but remains marginally significant, and the effect of positive reforms becomes significant on the overall sample. The IV coefficients are, in general, larger. If we interpret the effect of reforms in terms of percentage change relative to the effect of exposure alone, we find that positive reforms increase the advantage of more exposed subsidiaries by an additional 5% in overall patenting, while negative reforms decrease that advantage by 13%. The finding that negative reforms in our sample have a stronger effect on average than positive reforms is confirmed when using historic exposure.

Tables B5 and B6 in the appendix perform two placebo tests to ensure that our measure of exposure is not correlated with differential trends in patenting that are unrelated to the reforms. In the first placebo test (table B5), we randomly assign 49 positive and 13 negative fictitious reforms over the sample of 15 countries and 26 years (following the actual number and types of reforms), and then we run our main specification on this modified dataset. We repeat the operation over 1,000 replications, and we report the mean of the three coefficients of interest, as well as the bootstrapped standard errors. In the second placebo test (table B6), we do the same procedure, but we randomly assign 61 fictitious reforms to our country-year sample, randomly classifying them as positive or negative, therefore relaxing further the structure of the data by avoiding imposing a fixed number of positive and negative events. Both of these exercises result in small and insignificant coefficients associated with positive and negative pseudo-reforms, while the exposure coefficient alone remains significantly positive and similar in magnitude to the one obtained in the main analysis, as expected. These placebos confirm that exposure alone is not associated with differential time trends if not interacted with the timing of actual reforms.

To ensure that our results are picking up independent effects of positive and negative reforms, we compare them to regressions introducing positive and negative reform counts separately. Tables B7, B8, B9, and B10 present the results for the main outcomes, the direct effects on patents filed by teams with GMIs, the spillover effects on patents filed by teams of never-movers, and the quality of patents produced. The sign and significance of the results are very similar to the ones presented in the main analysis.

Tables B11 and B12 introduce a separate patent count for reforms affecting permanent migration, defined as changing the conditions for migrants staying more than one year in the country and for reforms affecting short stays of less than one year. The distinction between these two categories is presented in table 1. Results show that all of the effects are driven by permanent reforms. This finding confirms that policies affecting GMI long-term stays have more intensive effects on subsidiary innovation. Even among reforms affecting long-term migration, we find that our negative reforms have stronger impacts than positive ones.

Tables B13 and B14 evaluate whether the intensity of the effect is heterogeneous depending on whether reforms affect the quotas of foreigners allowed into the country or the rights that such foreigners have once they have moved. Results show that all of the effects are driven by volume reforms. Among reforms affecting quotas, we find that our negative reforms have stronger impacts on domestic patents than positive ones, while the effect of GCPs is more similar.

Tables B15 and B16 test the heterogeneity of the effect across MNE size. Here the sample is split in half according to the average number of subsidiaries that an assignee has over the period. Small MNEs are those firms that file seven patents in a given year on average, while large MNEs file 48 patents in a given year on average. Findings show that large MNEs benefit much more from positive reforms and suffer less from negative reforms. In fact, the effect of positive and negative reforms on large MNEs is symmetric: a positive reform increases the patenting of subsidiaries with a 1 standard deviation higher exposure by 7.6%, while a negative reforms decreases it by 8%. On the contrary, small MNEs do not benefit at all from positive reforms—the coefficient is even negative—and suffer more than twice as much as large firms. This result signals that large firms are able to take greater advantage of positive migration reforms and are somewhat protected from the detrimental impact of negative reforms. Small firms are less able to profit from an increase in access to cross-border mobility, but suffer significantly from reforms restricting such access. Thus, it seems migration reforms widen the inequality in patenting existing between large and small multinational companies. This result also hint toward the fact that the stronger effect of negative reforms is driven by the reaction of small firms.

Each column of table B17 reports the effect obtained after the exclusion of one of the nine countries that account for more than 5,000 observations in the data, sequentially. Results are presented for the total number of patents (panel A), GCPs (panel B), and domestic patents (panel C). The coefficient associated with negative reforms is significant across all samples, and the magnitude of the effect on total number of patents is very stable across regressions, except for the sample excluding Japan, where the magnitude halves in size (goes from 15% in the main sample to 8%). The effect of positive reforms on GCPs is significant in five of the nine samples (when China, Germany, Korea, Taiwan, or the U.K. are excluded), and the magnitude of the effect is comparable across all samples. Overall, these results confirm that our coefficients are not driven by one particular country in the sample.

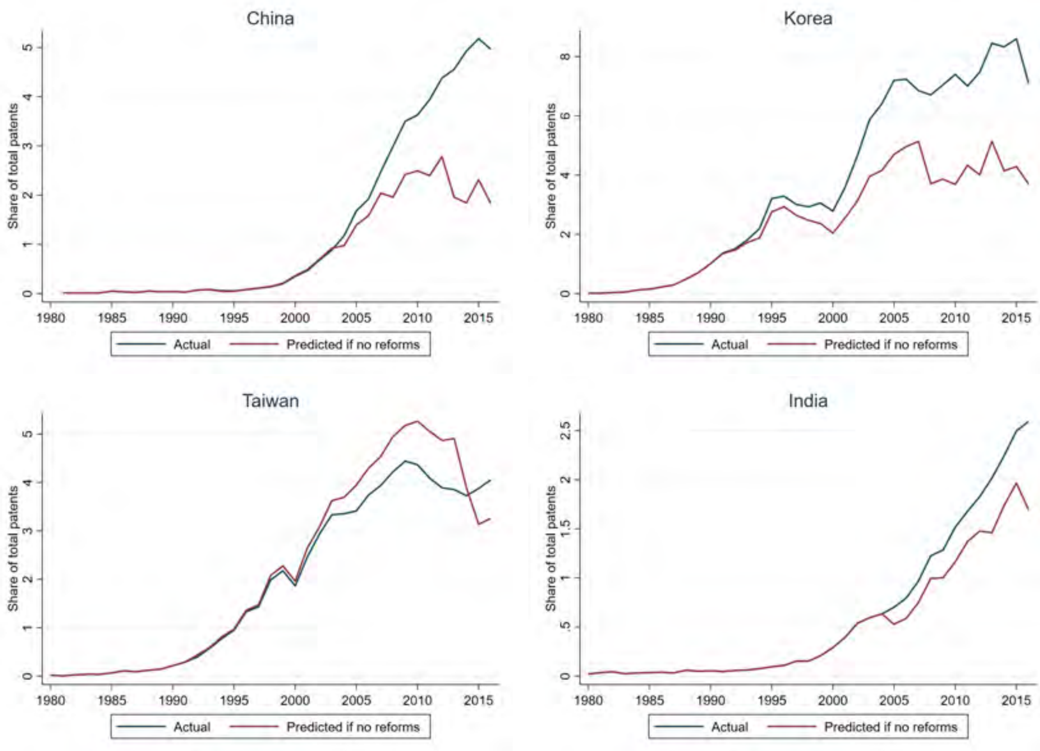
Next, we explore how our coefficients vary after excluding sequentially each one of the three major regions in our sample: North America, Europe, and Asia. Results are provided in table B18. Excluding Europe leaves the results largely unchanged. Excluding North America increases the strength and magnitude of positive reforms and decreases the strength and magnitude of negative reforms. This suggests that positive reforms are particularly effective in regions other than North America, while negative reforms are particularly damaging in North America, which is in line with the findings presented in the next sections, highlighting how positive reforms have been particularly beneficial for emerging markets. Finally, excluding Asia reduces the magnitude of the effect of negative reforms and flips the sign on the effect of positive reforms on domestic patents. The latter can be decomposed into a zero effect on teams including GMIs and a negative effect on teams of never-movers.

By construction, in our data we observe a subsidiary only if it files at least one patent in a given year. Consequently, our estimates on the total number of patents have to be interpreted as the effect on the intensive margin: reforms affect the quantity of inventors migrating and the amount of patents filed among subsidiaries that do patent. In order to explore whether attrition in our sample is affecting our results, we input subsidiaries in the years when they do not patent if the MNE is observed patenting in other countries in that year. For these observations, all the patent counts are set to 0. We then estimate the effect combining the intensive and the extensive margin by applying the same model to the new data. Given that our outcomes are modified using the arcsinh transformation, they are defined in zero. Tables B19 and B20 present the results. The negative effect of restrictive reforms on GCPs loses significance in the overall sample, but the other coefficients remain widely unchanged, suggesting that attrition is not the main driver of our results.

## B.2 Figures

Figure B4 shows how much countries with initially low shares of patents would have grown in the absence of migration reforms, comparing to the actual change that occurs over the period. Positive migration reforms generated a particularly large boost for China and Korea, while the counterfactual is more similar to the observed trend in innovation observed in Taiwan and India.

**Figure B4:** Predicted trends in share of global patents after subtracting the effect of reforms




The actual outcomes are the total patent shares observed in each country across the period of interest. We obtain the predicted outcomes by subtracting the predicted effect of positive and negative migration reforms from the actual outcomes. We select the countries in the low initial share group that have a large number of observations.

**Table B1:** Summary of patents by country

| | N. of patents | | N. of GCP | | N. of domestic patents | | Sh. of migrant inventors | | N. Obs |
|---|---|---|---|---|---|---|---|---|---|
| Country | Mean | sd | Mean | sd | Mean | sd | Mean | sd | count |
| Brazil | 2,17 | (3,53) | 1,24 | (1,90) | 0,93 | (2,44) | 0,13 | (0,31) | 1944 |
| Canada | 3,57 | (15,44) | 1,36 | (4,90) | 2,21 | (11,97) | 0,20 | (0,33) | 26085 |
| Chile | 1,26 | (0,72) | 0,87 | (0,81) | 0,39 | (0,68) | 0,14 | (0,34) | 304 |
| China | 7,20 | (44,05) | 2,38 | (8,94) | 4,82 | (40,14) | 0,34 | (0,38) | 12408 |
| Germany | 7,87 | (34,51) | 1,71 | (6,32) | 6,16 | (30,27) | 0,12 | (0,25) | 34932 |
| Spain | 2,39 | (4,72) | 1,27 | (2,17) | 1,12 | (3,36) | 0,15 | (0,33) | 4037 |
| United Kingdom | 3,98 | (10,04) | 1,54 | (3,87) | 2,44 | (7,69) | 0,19 | (0,33) | 28420 |
| India | 6,03 | (23,30) | 2,99 | (12,63) | 3,04 | (12,33) | 0,20 | (0,34) | 6831 |
| Japan | 33,60 | (150,96) | 1,02 | (3,31) | 32,58 | (149,06) | 0,08 | (0,20) | 27914 |
| Korea | 33,27 | (258,02) | 1,46 | (10,47) | 31,81 | (249,00) | 0,18 | (0,31) | 6705 |
| Mexico | 2,02 | (3,90) | 1,21 | (1,81) | 0,82 | (2,82) | 0,11 | (0,30) | 1516 |
| Philippines | 2,18 | (2,34) | 1,31 | (1,58) | 0,87 | (1,68) | 0,17 | (0,34) | 488 |
| Portugal | 1,43 | (1,49) | 0,95 | (0,87) | 0,48 | (1,26) | 0,16 | (0,37) | 535 |
| Taiwan | 11,71 | (56,67) | 1,56 | (8,53) | 10,15 | (51,81) | 0,14 | (0,29) | 10163 |
| United States | 14,17 | (86,11) | 1,53 | (11,22) | 12,64 | (77,03) | 0,09 | (0,21) | 135637 |

Summary statistics computed for the sample of subsidiaries belonging to an MNE over the period spanning from 1990 to 2016.

**Table B2:** Main results using historic exposure

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | asinh Patents | | asinh GCP | | asinh Domestic Patents | |
| VARIABLES | OLS | IV | OLS | IV | OLS | IV |
| Exposure x positive reforms | 0.0176* | 0.0918* | 0.00756 | 0.0365** | 0.0175 | 0.0977 |
| | (0.00980) | (0.0524) | (0.00466) | (0.0184) | (0.0112) | (0.0630) |
| Exposure x negative reforms | -0.102*** | -0.240** | -0.0112* | 0.0138 | -0.116*** | -0.246* |
| | (0.0143) | (0.105) | (0.00675) | (0.0387) | (0.0172) | (0.130) |
| Exposure | 0.476*** | 1.905*** | 0.110*** | 0.437*** | 0.594*** | 2.376*** |
| | (0.0260) | (0.167) | (0.0133) | (0.0548) | (0.0308) | (0.199) |
| Observations | 166,360 | 166,360 | 166,360 | 166,360 | 166,360 | 166,360 |
| R-squared / RMSE | 0.521 | 1.607 | 0.668 | 0.722 | 0.501 | 1.967 |
| K-P F-statistic | | 50.16 | | 50.16 | | 50.16 |

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at the subsidiary level. MNE x year fixed effects and country x year fixed effects included in all regressions. Period of analysis: 1990-2016. Historical exposure to the reforms is computed as the mobility rate of inventors observed within all the other subsidiaries of the MNE over the period going from 5 to 10 years prior to the observation, and then standardized to have mean 0 and standard deviation of 1. The IV regressions instrument current exposure with historic exposure. The bottom of the table reports the Kleibergen-Paap F-statistic for instrument strength.

**Table B3:** Direct and spill-over effects using historic exposure

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | asinh Patents | | asinh GCP | | asinh Domestic Patents | |
| VARIABLES | OLS | IV | OLS | IV | OLS | IV |
| *Panel A: Patents by teams with at least one GMI* | | | | | | |
| Exposure x positive reforms | 0.0213** | 0.0990** | 0.00809* | 0.0376** | 0.0230** | 0.108** |
| | (0.00896) | (0.0406) | (0.00444) | (0.0171) | (0.0102) | (0.0465) |
| Exposure x negative reforms | -0.0475*** | -0.0611 | -0.00522 | 0.0315 | -0.0505*** | -0.0499 |
| | (0.0121) | (0.0774) | (0.00658) | (0.0355) | (0.0143) | (0.0882) |
| Exposure | 0.275*** | 1.103*** | 0.0844*** | 0.336*** | 0.320*** | 1.279*** |
| | (0.0229) | (0.112) | (0.0133) | (0.0508) | (0.0273) | (0.125) |
| Observations | 166,360 | 166,360 | 166,360 | 166,360 | 166,360 | 166,360 |
| R-squared | 0.586 | 1.211 | 0.695 | 0.661 | 0.448 | 1.318 |
| K-P F-statistic | | 50.16 | | 50.16 | | 50.16 |
| *Panel B: Patents by teams with no GMIs* | | | | | | |
| Exposure x positive reforms | 0.0216** | 0.109** | 0.00544 | 0.0266* | 0.0208* | 0.109* |
| | (0.0103) | (0.0557) | (0.00391) | (0.0156) | (0.0108) | (0.0608) |
| Exposure x negative reforms | -0.120*** | -0.299*** | -0.00989* | 0.00423 | -0.129*** | -0.314** |
| | (0.0158) | (0.115) | (0.00557) | (0.0325) | (0.0171) | (0.127) |
| Exposure | 0.526*** | 2.108*** | 0.0860*** | 0.343*** | 0.584*** | 2.339*** |
| | (0.0287) | (0.178) | (0.0112) | (0.0468) | (0.0308) | (0.197) |
| Observations | 166,360 | 166,360 | 166,360 | 166,360 | 166,360 | 166,360 |
| R-squared | 0.512 | 1.742 | 0.649 | 0.643 | 0.494 | 1.909 |
| K-P F-statistic | | 50.16 | | 50.16 | | 50.16 |

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at the subsidiary level. MNE x year fixed effects and country x year fixed effects included in all regressions. Period of analysis: 1990-2016. Historical exposure to the reforms is computed as the mobility rate of inventors observed within all the other subsidiaries of the MNE over the period going from 5 to 10 years prior to the observation, and then standardized to have mean 0 and standard deviation of 1. Outcomes are divided into teams where at least one inventor is a migrant (has patented in a different country in earlier years), and teams of never-moving inventors. The IV regressions instrument current exposure with historic exposure. The bottom of the table reports the Kleibergen-Paap F-statistic for instrument strength.

**Table B4:** Quality effects using historic exposure

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| | asinh Generality per Patent | | asinh Originality per Patent | | asinh Radicalness per Patent | | asinh Share of Breakthrough Patents | | asinh citations per patents | |
| VARIABLES | OLS | IV | OLS | IV | OLS | IV | OLS | IV | OLS | IV |
| Exposure x positive reforms | -5.06e-05 | -0.000205 | -0.000574*** | -0.00218*** | -0.000501 | -0.00182 | 0.000137 | 0.000520 | 0.00115 | 0.00702 |
| | (0.000363) | (0.00139) | (0.000206) | (0.000818) | (0.000343) | (0.00131) | (0.000198) | (0.000743) | (0.00231) | (0.0103) |
| Exposure x negative reforms | 0.000155 | 0.000644 | -6.67e-06 | -9.39e-05 | 0.000202 | 0.00204 | -5.12e-05 | -0.000214 | -0.000905 | 0.0267 |
| | (0.000673) | (0.00317) | (0.000381) | (0.00182) | (0.000645) | (0.00309) | (0.000323) | (0.00163) | (0.00477) | (0.0279) |
| Exposure | -0.000134 | -0.000539 | 0.00164** | 0.00632** | 0.00329*** | 0.0127*** | -0.000314 | -0.00119 | 0.0523*** | 0.203*** |
| | (0.00106) | (0.00404) | (0.000698) | (0.00283) | (0.00101) | (0.00405) | (0.000618) | (0.00237) | (0.00695) | (0.0324) |
| Observations | 129,929 | 129,929 | 144,936 | 144,936 | 144,952 | 144,952 | 146,221 | 146,221 | 146,221 | 146,221 |
| R-squared | 0.729 | 0.136 | 0.728 | 0.0893 | 0.704 | 0.138 | 0.719 | 0.0537 | 0.772 | 0.821 |
| K-P F-statistic | | 41.65 | | 44.77 | | 44.78 | | 44.94 | | 44.94 |

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.
Standard errors clustered at the subsidiary level. MNE x year fixed effects and country x year fixed effects included in all regressions. Period of analysis: 1990-2016. Historical exposure to the reforms is computed as the mobility rate of inventors observed within all the other subsidiaries of the MNE over the period going from 5 to 10 years prior to the observation, and then standardized to have mean 0 and standard deviation of 1. The IV regressions instrument current exposure with historic exposure. The bottom of the table reports the Kleibergen-Paap F-statistic for instrument strength.

## **Table B5:** Placebo Test 1

| | asinh N. migrant inventors | asinh N. of patents | asinh N. of GCPs | asinh N. of domestic patents |
|---|---|---|---|---|
| VARIABLES | mean coef / [bootstrapped se] | mean coef / [bootstrapped se] | mean coef / [bootstrapped se] | mean coef / [bootstrapped se] |
| Exposure x Placebo positive reform | 0,001 | -0,023 | -0,002 | -0,027 |
| | [0,024] | [0,042] | [0,008] | [0,048] |
| Exposure x Placebo negative reform | 0,000 | -0,021 | -0,003 | -0,024 |
| | [0,059] | [0,104] | [0,020] | [0,120] |
| Exposure | 0,216 | 0,545 | 0,114 | 0,690 |
| | [0,046]*** | [0,085]*** | [0,015]*** | [0,098]*** |

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at the subsidiary level. MNE x year fixed effects and country x year fixed effects included in all regressions. Period of analysis: 1990-2015. Continuous exposure to the reforms is computed as the mobility rate of inventors observed within all the other subsidiaries of the MNE over the preceding 5 years, and then standardized to have mean 0 and standard deviation of 1. Results obtained over 1000 replications where 49 positive reforms and 13 negative reforms are selected randomly over the 15 countries and 26 years of interest. We report the average beta coefficient and the bootstrapped standard errors.

## Table B6: Placebo Test 2

| | asinh N. migrant inventors | asinh N. of patents | asinh N. of GCPs | asinh N. of domestic patents |
|---|---|---|---|---|
| VARIABLES | mean coef / [bootstrapped se] | mean coef / [bootstrapped se] | mean coef / [bootstrapped se] | mean coef / [bootstrapped se] |
| Exposure x Placebo positive reform | 0,000 | -0,024 | -0,002 | -0,028 |
| | [0,035] | [0,062] | [0,012] | [0,071] |
| Exposure x Placebo negative reform | 0,001 | -0,023 | -0,002 | -0,027 |
| | [0,036] | [0,063] | [0,012] | [0,072] |
| Exposure | 0,216 | 0,545 | 0,114 | 0,690 |
| | [0,047]*** | [0,087]*** | [0,015]*** | [0,099]*** |

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at the subsidiary level. MNE x year fixed effects and country x year fixed effects included in all regressions. Period of analysis: 1990-2015. Continuous exposure to the reforms is computed as the mobility rate of inventors observed within all the other subsidiaries of the MNE over the preceding 5 years. Results obtained over 1000 replications where 61 reforms are selected randomly over the 15 countries and 26 years of interest, and then randomly assigned into positive or negative. We report the average beta coefficient and bootstrapped standard errors.

**Table B7:** Main results of positive and negative reforms separately

| | (1) | (2) | (3) |
|---|---|---|---|
| | asinh Patents | asinh GCP | asinh Domestic Patents |
| VARIABLES | OLS | OLS | OLS |
| **Panel A: Positive reforms only** | | | |
| Exposure x positive reforms | 0.00108 | 0.00454 | -0.00237 |
| | (0.00865) | (0.00338) | (0.0100) |
| Exposure | 0.467*** | 0.0968*** | 0.607*** |
| | (0.0232) | (0.00975) | (0.0276) |
| Observations | 166,360 | 166,360 | 166,360 |
| R-squared | 0.521 | 0.666 | 0.504 |
| **Panel B: Negative reforms only** | | | |
| Exposure x negative reforms | -0.147*** | -0.0202*** | -0.173*** |
| | (0.0133) | (0.00575) | (0.0160) |
| Exposure | 0.585*** | 0.123*** | 0.737*** |
| | (0.0211) | (0.00877) | (0.0250) |
| Observations | 166,360 | 166,360 | 166,360 |
| R-squared | 0.524 | 0.666 | 0.507 |

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at the subsidiary level. MNE x year fixed effects and country x year fixed effects included in all regressions. Period of analysis: 1990-2016. Exposure to the reforms is computed as the mobility rate of inventors observed within all the other subsidiaries of the MNE over the preceeding 5 years, and then standardized to have mean 0 and standard deviation of 1. Panel A estimates the effect of positive reforms on the sample of countries that experience at least one of them, while Panel B does the same for negative reforms.

**Table B8:** Direct effects with positive and negative reforms separately

| | (1) | (2) | (3) |
|---|---|---|---|
| | asinh Patents | asinh GCP | asinh Domestic Patents |
| VARIABLES | OLS | OLS | OLS |
| **Panel A: Positive reforms only** | | | |
| Exposure x positive reforms | 0.0145* | 0.00570* | 0.0161* |
| | (0.00754) | (0.00315) | (0.00858) |
| Exposure | 0.244*** | 0.0782*** | 0.293*** |
| | (0.0194) | (0.00928) | (0.0228) |
| Observations | 166,360 | 166,360 | 166,360 |
| R-squared | 0.582 | 0.694 | 0.444 |
| **Panel B: Negative reforms only** | | | |
| Exposure x negative reforms | -0.0653*** | -0.0102* | -0.0682*** |
| | (0.0108) | (0.00557) | (0.0127) |
| Exposure | 0.329*** | 0.0994*** | 0.383*** |
| | (0.0172) | (0.00850) | (0.0201) |
| Observations | 166,360 | 166,360 | 166,360 |
| R-squared | 0.583 | 0.694 | 0.445 |

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at the subsidiary level. MNE x year fixed effects and country x year fixed effects included in all regressions. Period of analysis: 1990-2016. Exposure to the reforms is computed as the mobility rate of inventors observed within all the other subsidiaries of the MNE over the preceeding 5 years, and then standardized to have mean 0 and standard deviation of 1. Panel A estimates the effect of positive reforms on the sample of countries that experience at least one of them, while Panel B does the same for negative reforms.

**Table B9:** Spill-over effects with positive and negative reforms separately

| | (1) | (3) | (5) |
|---|---|---|---|
| | asinh Patents | asinh GCP | asinh Domestic Patents |
| VARIABLES | OLS | OLS | OLS |
| **Panel A: Positive reforms only** | | | |
| Exposure x positive reforms | 0.00353 | 0.00260 | -0.000144 |
| | (0.00918) | (0.00274) | (0.00973) |
| Exposure | 0.535*** | 0.0692*** | 0.591*** |
| | (0.0254) | (0.00787) | (0.0275) |
| Observations | 166,360 | 166,360 | 166,360 |
| R-squared | 0.515 | 0.647 | 0.497 |
| **Panel B: Negative reforms only** | | | |
| Exposure x negative reforms | -0.166*** | -0.0194*** | -0.185*** |
| | (0.0147) | (0.00440) | (0.0160) |
| Exposure | 0.673*** | 0.0904*** | 0.735*** |
| | (0.0231) | (0.00669) | (0.0252) |
| Observations | 166,360 | 166,360 | 166,360 |
| R-squared | 0.518 | 0.647 | 0.501 |

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at the subsidiary level. MNE x year fixed effects and country x year fixed effects included in all regressions. Period of analysis: 1990-2016. Exposure to the reforms is computed as the mobility rate of inventors observed within all the other subsidiaries of the MNE over the preceeding 5 years, and then standardized to have mean 0 and standard deviation of 1. Panel A estimates the effect of positive reforms on the sample of countries that experience at least one of them, while Panel B does the same for negative reforms.

**Table B10:** Patent quality with positive and negative reforms separately

| | (1) asinh generality per patent | (2) asinh originality per patent | (3) asinh radicalness per patent | (4) asinh share of breakthrough patents | (5) asinh citations per patents |
|---|---|---|---|---|---|
| VARIABLES | OLS | OLS | OLS | OLS | OLS |
| **Panel A: Positive reforms only** | | | | | |
| Exposure x positive reforms | 4.43e-05 | -0.000375** | -0.000157 | -0.000102 | -0.000746 |
| | (0.000254) | (0.000172) | (0.000279) | (0.000123) | (0.00183) |
| Exposure | -0.000794 | 0.00108* | 0.00282*** | -0.000189 | 0.0599*** |
| | (0.000818) | (0.000557) | (0.000863) | (0.000385) | (0.00553) |
| Observations | 129,929 | 144,936 | 144,952 | 146,221 | 146,221 |
| R-squared | 0.729 | 0.728 | 0.704 | 0.719 | 0.772 |
| **Panel B: Negative reforms only** | | | | | |
| Exposure x negative reforms | -0.000277 | -0.000275 | -0.00109 | -0.000138 | -0.0143*** |
| | (0.000656) | (0.000434) | (0.000678) | (0.000449) | (0.00495) |
| Exposure | -0.000501 | 0.000449 | 0.00327*** | -0.000315 | 0.0686*** |
| | (0.000781) | (0.000580) | (0.000815) | (0.000364) | (0.00492) |
| Observations | 129,929 | 144,936 | 144,952 | 146,221 | 146,221 |
| R-squared | 0.729 | 0.728 | 0.704 | 0.719 | 0.772 |

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at the subsidiary level. MNE x year fixed effects and country x year fixed effects included in all regressions. Period of analysis: 1990-2016. Exposure to the reforms is computed as the mobility rate of inventors observed within all the other subsidiaries of the MNE over the preceeding 5 years, and then standardized to have mean 0 and standard deviation of 1. Panel A estimates the effect of positive reforms on the sample of countries that experience at least one of them, while Panel B does the same for negative reforms. Columns (1), (2) and (3) weight the count of the number of patents by the generality, originality and radicalness coefficients, respectively and then divide them by the patent count. Column (4) computes the share of patents that are considered breakthrough. Column (5) computes the number of citations per patent.

**Table B11:** Effect of permanent and temporary reforms on main outcomes

| | (1) | (2) | (3) |
|---|---|---|---|
| | asinh Patents | asinh GCP | asinh Domestic Patents |
| VARIABLES | OLS | OLS | OLS |
| Exposure x positive permanent reforms | 0.0124 | 0.0104*** | 0.0125 |
| | (0.00955) | (0.00385) | (0.0114) |
| Exposure x negative permanent reforms | -0.144*** | -0.0268*** | -0.168*** |
| | (0.0151) | (0.00658) | (0.0180) |
| Exposure x positive temporary reforms | 0.0133 | 0.00424 | 0.0118 |
| | (0.0145) | (0.00546) | (0.0167) |
| Exposure x negative temporary reforms | -0.00179 | 0.0115 | 0.00263 |
| | (0.0246) | (0.0112) | (0.0295) |
| Exposure | 0.546*** | 0.102*** | 0.695*** |
| | (0.0250) | (0.0103) | (0.0304) |
| Observations | 166,360 | 166,360 | 166,360 |
| R-squared | 0.524 | 0.666 | 0.507 |

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at the subsidiary level. MNE x year fixed effects and country x year fixed effects included in all regressions. Period of analysis: 1990-2016. Only permanent reforms are considered, and Brazil is excluded from the sample because it does not adopt any permanent reform over the period. Exposure to the reforms is computed as the mobility rate of inventors observed within all the other subsidiaries of the MNE over the preceding 5 years, and then standardized to have mean 0 and standard deviation of 1. Permanent reforms are policy changes affecting stays of longer than one year, while temporary are policy changes affecting stays of less than one year.

**Table B12:** Effect of permanent reforms on migrant patents and spill-overs

| | (1) | (2) | (3) |
|---|---|---|---|
| | asinh Patents | asinh GCP | asinh Domestic Patents |
| VARIABLES | OLS | OLS | OLS |
| *Panel A: Patents by teams with at least one GMI* | | | |
| Exposure x positive permanent reforms | 0.0202*** | 0.0121*** | 0.0237*** |
| | (0.00757) | (0.00364) | (0.00851) |
| Exposure x negative permanent reforms | -0.0715*** | -0.0191*** | -0.0771*** |
| | (0.0124) | (0.00635) | (0.0144) |
| Exposure x positive temporary reforms | 0.0184 | 0.00301 | 0.0190 |
| | (0.0131) | (0.00511) | (0.0147) |
| Exposure x negative temporary reforms | 0.00168 | 0.00968 | 0.00359 |
| | (0.0199) | (0.0107) | (0.0232) |
| Exposure | 0.283*** | 0.0774*** | 0.331*** |
| | (0.0190) | (0.00987) | (0.0226) |
| Observations | 166,360 | 166,360 | 166,360 |
| R-squared | 0.583 | 0.694 | 0.445 |
| *Panel B: Patents by teams with no GMIs* | | | |
| Exposure x positive permanent reforms | 0.0266** | 0.00534* | 0.0216* |
| | (0.0106) | (0.00295) | (0.0115) |
| Exposure x negative permanent reforms | -0.175*** | -0.0221*** | -0.189*** |
| | (0.0166) | (0.00476) | (0.0179) |
| Exposure x positive temporary reforms | 0.0124 | 0.00388 | 0.0131 |
| | (0.0154) | (0.00440) | (0.0162) |
| Exposure x negative temporary reforms | 0.0136 | 0.00599 | 0.0124 |
| | (0.0268) | (0.00877) | (0.0289) |
| Exposure | 0.610*** | 0.0780*** | 0.678*** |
| | (0.0278) | (0.00850) | (0.0307) |
| Observations | 166,360 | 166,360 | 166,360 |
| R-squared | 0.519 | 0.647 | 0.501 |

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at the subsidiary level. MNE x year fixed effects and country x year fixed effects included in all regressions. Period of analysis: 1990-2016. Only permanent reforms are considered, and Brazil is excluded from the sample because it does not adopt any permanent reform over the period. Exposure to the reforms is computed as the mobility rate of inventors observed within all the other subsidiaries of the MNE over the preceding 5 years, and then standardized to have mean 0 and standard deviation of 1.

**Table B13:** Heterogeneity of effect by reform type

| | (1) | (2) | (3) |
|---|---|---|---|
| | asinh Patents | asinh GCP | asinh Domestic Patents |
| VARIABLES | OLS | OLS | OLS |
| Exposure x positive volume reforms | 0.0187* | 0.0123*** | 0.0193* |
| | (0.00977) | (0.00428) | (0.0116) |
| Exposure x negative volume reforms | -0.137*** | -0.0215*** | -0.163*** |
| | (0.0144) | (0.00637) | (0.0173) |
| Exposure x positive rights reforms | 0.00174 | -0.0129 | -0.00166 |
| | (0.0232) | (0.00982) | (0.0275) |
| Exposure x negative rights reforms | -0.00866 | 0.0179 | -0.0156 |
| | (0.0286) | (0.0138) | (0.0341) |
| Exposure | 0.517*** | 0.0952*** | 0.666*** |
| | (0.0234) | (0.00952) | (0.0280) |
| Observations | 166,360 | 166,360 | 166,360 |
| R-squared | 0.524 | 0.666 | 0.507 |

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at the subsidiary level. MNE x year fixed effects and country x year fixed effects included in all regressions. Period of analysis: 1990-2016. Exposure to the reforms is computed as the mobility rate of inventors observed within all the other subsidiaries of the MNE over the preceding 5 years. Volume reforms affect the quantity of immigrants within a country, notably through changes in quotas, while rights reforms affect their conditions in the country.

**Table B14:** Heterogeneity of direct and spill-over effect by reform type

| | (1) | (2) | (3) |
|---|---|---|---|
| | asinh Patents | asinh GCP | asinh Domestic Patents |
| VARIABLES | OLS | OLS | OLS |
| *Panel A: Patents by teams with at least one GMI* | | | |
| Exposure x positive volume reforms | 0.0260*** | 0.0132*** | 0.0283*** |
| | (0.00837) | (0.00409) | (0.00980) |
| Exposure x negative volume reforms | -0.00313 | -0.0171* | -0.00362 |
| | (0.0202) | (0.00948) | (0.0234) |
| Exposure x positive rights reforms | -0.0676*** | -0.0119* | -0.0718*** |
| | (0.0121) | (0.00614) | (0.0144) |
| Exposure x negative rights reforms | 0.0117 | 0.0179 | 0.00915 |
| | (0.0248) | (0.0132) | (0.0297) |
| Exposure | 0.264*** | 0.0721*** | 0.314*** |
| | (0.0183) | (0.00920) | (0.0216) |
| Observations | 166,360 | 166,360 | 166,360 |
| R-squared | 0.583 | 0.694 | 0.445 |
| *Panel B: Patents by teams with no GMIs* | | | |
| Exposure x positive volume reforms | 0.0224** | 0.00753** | 0.0198* |
| | (0.0107) | (0.00325) | (0.0115) |
| Exposure x negative volume reforms | 0.00465 | -0.00533 | 0.00860 |
| | (0.0251) | (0.00746) | (0.0267) |
| Exposure x positive rights reforms | -0.147*** | -0.0187*** | -0.164*** |
| | (0.0160) | (0.00485) | (0.0172) |
| Exposure x negative rights reforms | -0.00423 | 0.00763 | -0.00652 |
| | (0.0318) | (0.0102) | (0.0340) |
| Exposure | 0.588*** | 0.0711*** | 0.650*** |
| | (0.0268) | (0.00749) | (0.0293) |
| Observations | 166,360 | 166,360 | 166,360 |
| R-squared | 0.518 | 0.647 | 0.500 |

Robust standard errors in parentheses *** $p<0.01$, ** $p<0.05$, * $p<0.1$
Standard errors clustered at the subsidiary level. MNE x year fixed effects and country x year fixed effects included in all regressions. Period of analysis: 1990-2016. Exposure to the reforms is computed as the mobility rate of inventors observed within all the other subsidiaries of the MNE over the preceding 5 years. Volume reforms affect the quantity of immigrants within a country, notably through changes in quotas, while rights reforms affect their conditions in the country.

**Table B15:** Heterogeneity of effect by MNE size

| | (1) | (2) | (3) |
|---|---|---|---|
| | asinh Patents | asinh GCP | asinh Domestic Patents |
| VARIABLES | OLS | OLS | OLS |
| Exposure x positive reforms | 0.0759*** | 0.0310*** | 0.0867*** |
| | (0.0179) | (0.00692) | (0.0212) |
| Exposure x negative reforms | -0.0806*** | 0.0131 | -0.0801** |
| | (0.0302) | (0.0131) | (0.0362) |
| Exposure x positive reforms x small MNEs | -0.119*** | -0.0447*** | -0.141*** |
| | (0.0175) | (0.00683) | (0.0206) |
| Exposure x negative reforms x small MNEs | -0.120*** | -0.0609*** | -0.165*** |
| | (0.0320) | (0.0134) | (0.0384) |
| Exposure | 0.584*** | 0.118*** | 0.744*** |
| | (0.0244) | (0.0102) | (0.0291) |
| Observations | 166,360 | 166,360 | 166,360 |
| R-squared | 0.533 | 0.670 | 0.517 |

Robust standard errors in parentheses *** $p<0.01$, ** $p<0.05$, * $p<0.1$
Standard errors clustered at the subsidiary level. MNE x year fixed effects and country x year fixed effects included in all regressions. Period of analysis: 1990-2016. Exposure to the reforms is computed as the mobility rate of inventors observed within all the other subsidiaries of the MNE over the preceding 5 years. Small MNEs are identified as the 60% of assignees with the smallest average number of subsidiaries over the period, while large MNEs are the ones with the 40% largest number of subsidiaries (we split the sample 60%-40% to keep a balanced number of observations in both groups, given that large MNEs have more observations each).

**Table B16:** Heterogeneity of direct and spill-over effects by MNE size

| | (1) | (2) | (3) |
|---|---|---|---|
| | asinh Patents | asinh GCP | asinh Domestic Patents |
| VARIABLES | OLS | OLS | OLS |
| *Panel A: Patents by teams with at least one GMI* | | | |
| Exposure x positive reforms | 0.0719*** | 0.0286*** | 0.0807*** |
| | (0.0151) | (0.00652) | (0.0175) |
| Exposure x negative reforms | -0.0110 | 0.0208* | 0.000481 |
| | (0.0253) | (0.0126) | (0.0295) |
| Exposure x positive reforms x small MNEs | -0.0953*** | -0.0393*** | -0.108*** |
| | (0.0147) | (0.00648) | (0.0170) |
| Exposure x negative reforms x small MNEs | -0.104*** | -0.0573*** | -0.130*** |
| | (0.0266) | (0.0127) | (0.0312) |
| Exposure | 0.306*** | 0.0920*** | 0.358*** |
| | (0.0194) | (0.00984) | (0.0230) |
| Observations | 166,360 | 166,360 | 166,360 |
| R-squared | 0.591 | 0.697 | 0.458 |
| *Panel B: Patents by teams with no GMIs* | | | |
| Exposure x positive reforms | 0.0860*** | 0.0232*** | 0.0902*** |
| | (0.0201) | (0.00547) | (0.0214) |
| Exposure x negative reforms | -0.0935*** | 0.00338 | -0.106*** |
| | (0.0334) | (0.0104) | (0.0358) |
| Exposure x positive reforms x small MNEs | -0.130*** | -0.0346*** | -0.143*** |
| | (0.0195) | (0.00529) | (0.0207) |
| Exposure x negative reforms x small MNEs | -0.133*** | -0.0416*** | -0.143*** |
| | (0.0356) | (0.0107) | (0.0380) |
| Exposure | 0.667*** | 0.0880*** | 0.736*** |
| | (0.0271) | (0.00804) | (0.0295) |
| Observations | 166,360 | 166,360 | 166,360 |
| R-squared | 0.528 | 0.650 | 0.510 |

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at the subsidiary level. MNE x year fixed effects and country x year fixed effects included in all regressions. Period of analysis: 1990-2016. Exposure to the reforms is computed as the mobility rate of inventors observed within all the other subsidiaries of the MNE over the preceding 5 years. Small MNEs are identified as the 60% of assignees with the smallest average number of subsidiaries over the period, while large MNEs are the ones with the 40% largest number of subsidiaries (we split the sample 60%-40% to keep a balanced number of observations in both groups, given that large MNEs have more observations each).

## **Table B17:** Robustness of main outcomes to excluding large countries

| | (1) without US | (2) without Canada | (3) without China | (4) without Germany | (5) without UK | (6) without India | (7) without Japan | (8) without S. Korea | (9) without Taiwan |
|---|---|---|---|---|---|---|---|---|---|
| Panel A: Asinh. N. of patents | | | | | | | | | |
| Exposure x positive reforms | 0.0329* | 0.00450 | 0.0121 | 0.0130 | 0.0125 | 0.00782 | 0.000330 | 0.0107 | 0.0111 |
| | (0.0180) | (0.0111) | (0.00923) | (0.00908) | (0.00955) | (0.00870) | (0.00931) | (0.00822) | (0.00901) |
| Exposure x negative reforms | -0.123** | -0.153*** | -0.138*** | -0.156*** | -0.164*** | -0.152*** | -0.0897*** | -0.151*** | -0.151*** |
| | (0.0488) | (0.0156) | (0.0150) | (0.0155) | (0.0160) | (0.0146) | (0.0149) | (0.0145) | (0.0142) |
| Exposure | 0.712*** | 0.621*** | 0.581*** | 0.559*** | 0.599*** | 0.583*** | 0.452*** | 0.561*** | 0.559*** |
| | (0.0582) | (0.0311) | (0.0253) | (0.0268) | (0.0267) | (0.0254) | (0.0239) | (0.0238) | (0.0245) |
| Observations | 63,429 | 135,914 | 151,062 | 134,708 | 134,326 | 157,793 | 146,043 | 160,345 | 157,011 |
| R-squared | 0.513 | 0.532 | 0.528 | 0.549 | 0.529 | 0.527 | 0.554 | 0.537 | 0.533 |
| Panel B: Asinh. N. of GCPs | | | | | | | | | |
| Exposure x positive reforms | 0.00826 | 0.00474 | 0.00625* | 0.00683* | 0.00613* | 0.00539 | 0.00445 | 0.00822** | 0.00757** |
| | (0.00767) | (0.00437) | (0.00361) | (0.00349) | (0.00370) | (0.00337) | (0.00368) | (0.00374) | (0.00340) |
| Exposure x negative reforms | -0.0174 | -0.0206*** | -0.0181*** | -0.0201*** | -0.0215*** | -0.0213*** | -0.0139** | -0.0236*** | -0.0238*** |
| | (0.0277) | (0.00683) | (0.00643) | (0.00635) | (0.00685) | (0.00624) | (0.00638) | (0.00633) | (0.00598) |
| Exposure | 0.207*** | 0.125*** | 0.115*** | 0.102*** | 0.119*** | 0.116*** | 0.0958*** | 0.107*** | 0.105*** |
| | (0.0296) | (0.0132) | (0.0105) | (0.0108) | (0.0110) | (0.0106) | (0.0106) | (0.0104) | (0.00974) |
| Observations | 63,429 | 135,914 | 151,062 | 134,708 | 134,326 | 157,793 | 146,043 | 160,345 | 157,011 |
| R-squared | 0.568 | 0.670 | 0.666 | 0.681 | 0.669 | 0.672 | 0.671 | 0.672 | 0.667 |
| Panel C: Asinh. N. of domestic patents | | | | | | | | | |
| Exposure x positive reforms | 0.0350* | 0.00242 | 0.0104 | 0.0112 | 0.0107 | 0.00558 | -0.00295 | 0.00978 | 0.00868 |
| | (0.0204) | (0.0130) | (0.0107) | (0.0106) | (0.0111) | (0.0101) | (0.0108) | (0.00985) | (0.0104) |
| Exposure x negative reforms | -0.133** | -0.178*** | -0.160*** | -0.182*** | -0.193*** | -0.178*** | -0.108*** | -0.178*** | -0.177*** |
| | (0.0571) | (0.0187) | (0.0180) | (0.0186) | (0.0191) | (0.0176) | (0.0179) | (0.0174) | (0.0170) |
| Exposure | 0.865*** | 0.787*** | 0.741*** | 0.715*** | 0.766*** | 0.743*** | 0.595*** | 0.717*** | 0.717*** |
| | (0.0669) | (0.0370) | (0.0302) | (0.0322) | (0.0321) | (0.0304) | (0.0291) | (0.0288) | (0.0294) |
| Observations | 63,429 | 135,914 | 151,062 | 134,708 | 134,326 | 157,793 | 146,043 | 160,345 | 157,011 |
| R-squared | 0.504 | 0.516 | 0.512 | 0.533 | 0.512 | 0.509 | 0.531 | 0.518 | 0.515 |

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at the subsidiary level. MNE x year fixed effects and country x year fixed effects included in all regressions. Period of analysis: 1990-2016. Exposure to the reforms is computed as the mobility rate of inventors observed within all the other subsidiaries of the MNE over the preceding 5 years, and then standardized to have mean 0 and standard deviation of 1. Each column excludes from the sample one country with more than 5 thousands observations in the dataset (in order from left to right: United States, Canada, China, Germany, United Kingdom, India, Japan, South Korea, Taiwan). Panel A shows the regressions on the total number of patents, and Panel B and C disentangle the outcome into GCPs and domestic patents.

**Table B18:** Robustness of main outcomes to excluding the main macro-regions

| | (1) without Europe | (2) without North America | (3) without Asia |
|---|---|---|---|
| Panel A: Asinh. N. of patents | | | |
| Exposure x positive reforms | 0.0168* | 0.0407* | -0.0177** |
| | (0.00995) | (0.0240) | (0.00850) |
| Exposure x negative reforms | -0.176*** | -0.0633 | -0.0656*** |
| | (0.0177) | (0.0579) | (0.0162) |
| Exposure | 0.600*** | 0.770*** | 0.488*** |
| | (0.0293) | (0.0700) | (0.0249) |
| Observations | 99,353 | 49,273 | 108,582 |
| R-squared | 0.559 | 0.528 | 0.594 |
| Panel B: Asinh. N. of GCPs | | | |
| Exposure x positive reforms | 0.00743** | 0.00999 | 0.00905** |
| | (0.00373) | (0.0105) | (0.00388) |
| Exposure x negative reforms | -0.0211*** | 0.00638 | -0.0112 |
| | (0.00720) | (0.0337) | (0.00747) |
| Exposure | 0.111*** | 0.230*** | 0.0924*** |
| | (0.0112) | (0.0362) | (0.00988) |
| Observations | 99,353 | 49,273 | 108,582 |
| R-squared | 0.695 | 0.586 | 0.696 |
| Panel C: Asinh. N. of domestic patents | | | |
| Exposure x positive reforms | 0.0153 | 0.0463* | -0.0223** |
| | (0.0116) | (0.0272) | (0.0106) |
| Exposure x negative reforms | -0.206*** | -0.0643 | -0.0830*** |
| | (0.0213) | (0.0678) | (0.0200) |
| Exposure | 0.768*** | 0.926*** | 0.641*** |
| | (0.0355) | (0.0807) | (0.0311) |
| Observations | 99,353 | 49,273 | 108,582 |
| R-squared | 0.541 | 0.522 | 0.569 |

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at the subsidiary level. MNE x year fixed effects and country x year fixed effects included in all regressions. Period of analysis: 1990-2016. Exposure to the reforms is computed as the mobility rate of inventors observed within all the other subsidiaries of the MNE over the preceding 5 years, and then standardized to have mean 0 and standard deviation of 1. Each column exclude from the sample one of the three main macro-regions in the dataset (Europe, North America, Asia). Panel A shows the regressions on the total number of patents, and Panel B and C disentangle the outcome into GCPs and domestic patents.

**Table B19:** Main results including extensive margin

| | (1) | (2) | (3) |
|---|---|---|---|
| | asinh Patents | asinh GCP | asinh Domestic Patents |
| VARIABLES | OLS | OLS | OLS |
| Exposure x positive reforms | -0.000774 | 0.00213 | -0.000756 |
| | (0.00390) | (0.00164) | (0.00404) |
| Exposure x negative reforms | -0.0730*** | -0.00454 | -0.0758*** |
| | (0.00686) | (0.00288) | (0.00714) |
| Exposure | 0.351*** | 0.0620*** | 0.365*** |
| | (0.0114) | (0.00509) | (0.0120) |
| Observations | 693,928 | 693,928 | 693,928 |
| R-squared | 0.377 | 0.522 | 0.349 |

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at the subsidiary level. MNE x year fixed effects and country x year fixed effects included in all regressions. Period of analysis: 1990-2016. Patents set to zero if a subsidiary does not patent in a given year while the MNE patents in a different country (combines intensive and extensive margin effect). Exposure to the reforms is computed as the mobility rate of inventors observed within all the other subsidiaries of the MNE over the preceding 5 years.

**Table B20:** Direct and spill-over results including extensive margin

| | (1) | (2) | (3) |
|---|---|---|---|
| | asinh Patents | asinh GCP | asinh Domestic Patents |
| VARIABLES | OLS | OLS | OLS |
| *Panel A: Patents by teams with at least one migrant* | | | |
| Exposure x positive reforms | 0.00922*** | 0.00276* | 0.00962*** |
| | (0.00329) | (0.00144) | (0.00336) |
| Exposure x negative reforms | -0.0240*** | 0.00154 | -0.0256*** |
| | (0.00537) | (0.00257) | (0.00549) |
| Exposure | 0.145*** | 0.0407*** | 0.140*** |
| | (0.00903) | (0.00454) | (0.00919) |
| Observations | 693,928 | 693,928 | 693,928 |
| R-squared | 0.406 | 0.501 | 0.311 |
| *Panel B: Patents by teams with no migrants* | | | |
| Exposure x positive reforms | 0.000141 | 0.000393 | 0.000185 |
| | (0.00374) | (0.00106) | (0.00379) |
| Exposure x negative reforms | -0.0795*** | -0.00601*** | -0.0806*** |
| | (0.00681) | (0.00170) | (0.00693) |
| Exposure | 0.350*** | 0.0363*** | 0.355*** |
| | (0.0115) | (0.00309) | (0.0117) |
| Observations | 693,928 | 693,928 | 693,928 |
| R-squared | 0.369 | 0.489 | 0.352 |

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at the subsidiary level. MNE x year fixed effects and country x year fixed effects included in all regressions. Period of analysis: 1990-2016. Patents set to zero if a subsidiary does not patent in a given year while the MNE patents in a different country (combines intensive and extensive margin effect). Exposure to the reforms is computed as the mobility rate of inventors observed within all the other subsidiaries of the MNE over the preceding 5 years.

# C Result Extensions

## C.1 Effect on productivity

In this extension, we ask whether individual inventor productivity increases after moving to another country and, thus, whether migration reforms affect the level of productivity of exposed subsidiaries. To answer the first question, we construct a new dataset counting the number of patents filed by each inventor over time (instead of collapsing everything at the subsidiary level), and we keep in the sample every inventor that is observed changing country at least once over the period of interest (1990-2016). We then regress the number of patents that each individual files in a given year on a variable indicating whether the inventor has (just) moved from a different country, controlling for individual fixed effects, MNE x year fixed effects, country x year fixed effects, and dummies for time since the first appearance of the inventor in the sample (which is a proxy for experience). The underlying assumption of this model is that the timing of mobile inventor migration is quasi-exogenous, once we control for trends explained by the MNE and the country of residence, as well as for inventor experience.

Table C21 presents the results. Column (1) considers the effect on patenting during the first year after moving, as compared to any other period. Column (2) does the same as column (1) but restricts the movements to changes across countries within the same MNE. Column (3) captures the average change in productivity in all the years following the first movement compared to the pre-movement period. Column (4) does the same as column (3) but restricts the movements to changes across countries within the same MNE. Results show that only movements within the same MNE lead the inventor to become more productive; international movements involving a change in MNE actually lead to less patent production. Table C22 shows that positive migration reforms are positively associated with the likelihood that inventors move into the country, after controlling for individual, country, MNE, and year fixed effects. We can, thus, use positive migration reforms as an instrument for inventor arrival, under the assumption that the adoption of such reforms in a different country from their origin is uncorrelated with productivity trends of moving inventors. When we do that, we find a positive and significant coefficient associated with the move, which suggests that the OLS coefficient might be driven by selection of inventors on a downward productivity trend to change country and firm. In particular, movements within MNE across countries might be more likely to be voluntary, while movements to other MNEs might be sometimes associated with the inventor being fired.

Finally, we check whether migration policies have an impact on the productivity of subsidiaries. So far we have shown that more exposed subsidiaries increase the number of patents filed after a positive migration reform and decrease it after a negative reform. This can be driven by either a change in the number of (patenting) inventors or by a change in the number of patents filed by each inventor. In order to control for the size effect and isolate the effect on productivity, we divide the number of patents by the number of inventors observed patenting in the subsidiary that year. Results are reported in table C23. Positive reforms increase the number of GCPs filed by each inventor, and negative reforms decrease the number of domestic patents per inventor but increase the number of GCPs per inventor. The latter might be due to the fact that local inventors might continue to collaborate with their colleague GMIs after they have left the country. Interestingly, exposure to reforms is associated with lower productivity on average. This suggests that large MNEs tend to have more inventors patenting, among which some are not very productive, while in the smaller MNEs, only very productive inventors patent. Given that the effects on patents per inventor are smaller than our main effects on total number of patents, we explore whether some of our main results are driven by changes in the size of subsidiaries.

Table C24 documents the effect of the reforms on overall subsidiary size, measured as the number of inventors observed patenting in a given year. What we see is that positive reforms impact only the number of GMIs in the subsidiary, but do not have significant effects on overall size. On the contrary, negative migration reforms affect both the number of GMI inventors and the number of never-movers, resulting in shrinking the size of subsidiaries. One standard deviation higher exposure is associated with a 6.2% decrease in GMIs and a 15% decrease in never-moving inventors following a negative reform. When interpreted in terms of additional effects relative to the effect of exposure alone, negative reforms decrease the number of GMIs by 29% and the number of never-movers by 21% relative to the effect of exposure alone. Our data does not allow disentangling whether the inventors disappearing have left the subsidiary, or whether they are still there but no longer patenting. What we can infer from these results is that negative migration reforms have very strong effects on the number of inventors observed patenting in a given country, and this effect goes beyond the impact on the number of GMIs alone. This result is very much in line with the findings of Kerr et al. (2015), who find strong complementarity in production between migrant and native workers, such that relaxation of H1-B visa restrictions lead to an expansion in native employment within affected firms. It also echoes a quote from Bill Gates during a congressional testimony stating that Microsoft hires four additional employees to support each worker hired on the H-1B visa.

**Table C21:** Results on individual inventor productivity (1)

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| VARIABLES | | asinh N. of Patents | | |
| Migration (1yr) | -0.0121*** | | | |
| | (0.00336) | | | |
| Migration same assignee (1yr) | | 0.0985*** | | |
| | | (0.00772) | | |
| Migration (always) | | | -0.0364*** | |
| | | | (0.00377) | |
| Migration same assignee (always) | | | | 0.0216*** |
| | | | | (0.00504) |
| Observations | 514,064 | 514,064 | 514,064 | 514,064 |
| R-squared | 0.375 | 0.377 | 0.375 | 0.375 |

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at the subsidiary level. Sample includes all inventors that are seen moving at least once over the period of interest (1990-2016). All regressions include individual FE, country x year FE, MNE x year FE, and fixed effects for years since first individual appearance (proxy for experience).

**Table C22:** Results on individual inventor productivity (2)

| | (1) | (2) | (3) |
|---|---|---|---|
| | Migration (always) | Asinh N. of Patents | |
| VARIABLES | FS | OLS | IV |
| Positive business reforms | 0.00668*** | | |
| | (0.00149) | | |
| Migration (always) | | -0.0301*** | 2.377** |
| | | (0.00364) | (1.188) |
| Observations | 553,811 | 553,811 | 553,811 |
| R-squared | 0.691 | 0.306 | -1.737 |
| RMSE | | | 0.860 |
| K-P WF | | | 20.13 |

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1 Standard errors clustered at the subsidiary level. Sample includes all inventors that are seen moving at least once over the period of interest (1990-2016). All regressions include individual FE, country FE, MNE FE, year FE, and fixed effects for years since first individual appearance (proxy for experience). Sample are mobile inventors in all countries with at least one positive business reform over the 1990-2016 sample. In IV, positive business reforms are used as exogenous instruments.

**Table C23:** Results on subsidiary productivity

| | (1) asinh Patents per inventor | (2) asinh GCP per inventor | (3) asinh Domestic Patents per inventor |
|---|---|---|---|
| VARIABLES | OLS | OLS | OLS |
| Exposure x positive reforms | 0.00163 | 0.00330** | -0.00189 |
| | (0.00147) | (0.00155) | (0.00151) |
| Exposure x negative reforms | 0.00482* | 0.0164*** | -0.0114*** |
| | (0.00246) | (0.00281) | (0.00236) |
| Exposure | -0.0689*** | -0.138*** | 0.0667*** |
| | (0.00470) | (0.00505) | (0.00421) |
| Observations | 166,360 | 166,360 | 166,360 |
| R-squared | 0.541 | 0.576 | 0.613 |

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at the subsidiary level. MNE x year fixed effects and country x year fixed effects included in all regressions. Period of analysis: 1990-2016. Exposure to the reforms is computed as the mobility rate of inventors observed within all the other subsidiaries of the MNE over the preceding 5 years, and then standardized to have mean 0 and standard deviation of 1. Outcomes are scaled by the number of inventors in the subsidiary

**Table C24:** Results on the size of subsidiary

| | (1) | (2) | (3) |
|---|---|---|---|
| | asinh N. GMIs | asinh N. domestic inventors | asinh N. all inventors |
| VARIABLES | OLS | OLS | OLS |
| Exposure x positive business reform | 0.0176** | 0.00834 | 0.00598 |
| | (0.00685) | (0.00908) | (0.00879) |
| Exposure x negative business reform | -0.0623*** | -0.159*** | -0.158*** |
| | (0.0108) | (0.0153) | (0.0147) |
| Exposure | 0.214*** | 0.750*** | 0.671*** |
| | (0.0183) | (0.0268) | (0.0257) |
| Observations | 166,360 | 166,360 | 166,360 |
| R-squared | 0.500 | 0.488 | 0.498 |

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at the subsidiary level. MNE x year fixed effects and country x year fixed effects included in all regressions. Period of analysis: 1990-2016. Exposure to the reforms is computed as the mobility rate of inventors observed within all the other subsidiaries of the MNE over the preceding 5 years, and then standardized to have mean 0 and standard deviation of 1.

# D Reform Data Construction

This appendix focuses on the collection and construction of the database of unilateral reforms to migration policy impacting high-skilled migrants. The first subsection provides the list of reforms, and the second subsection describes the collection of the larger dataset of reforms. The full dataset is available upon request. To select our sample, we started from the 16 countries used by Branstetter et al. (2006), who study the impact of systematic reforms designed to strengthen and standardize intellectual property on MNEs' foreign direct investments from 1982 to 1999. We depart from their list by adding four major innovation countries that count more than 1% of GMIs (Canada, Germany, the United Kingdom, and the United States), and by dropping their five countries that patent very little and have less than 0.2% of GMIs (Argentina, Colombia, Thailand, Turkey, and Venezuela). Table D26 reports the sample selection criteria, where BFF indicates the sample of Branstetter et al. (2006) and BCSS indicates our sample.

## D.1 Study Reforms

For each reform examined in this study, table D25 lists the country impacted, the year of implementation, the estimated impacts on migrants, and a brief description of the reform.

**Table D25:** Description of Study Reforms

| Country | Year | Title | Impacts | Brief Description |
|---|---|---|---|---|
| Brazil | 2014 | Amendment of Foreign Statute | Increase Volume, Increase Rights | The amended act supports electronic visa, and gives Ministry of Foreign Affairs the power to simplify visa application process. It also implies that aliens who wish to travel to Brazil on business, as an artist or athlete does not need a visa if their country treat Brazilians the same. |

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**Table D25 – continued from previous page**

| Country | Year | Title | Impacts | Brief Description |
|---|---|---|---|---|
| Canada | 2002 | Immigration and Refugee Protection Act | Decrease Rights | The act was the primary federal legislation regulating immigration to Canada and created a high-level framework detailing the goals and guidelines the Canadian government with regard to immigration to Canada by foreign residents. It sets out the core principles and concepts that govern Canada's immigration and refugee protection programs, including provisions relating to refugees, sponsorships and removals, detention reviews and admissibility hearings, and the jurisdiction and powers of tribunals. |
| Chile | 2005 | Ratification of 'The United Nations Convention on the protection of the rights of all migratory workers and their families' | Increase Rights | Chile ratified the United Nations convention on migrant workers and developed policies to assist in their integration. Allowed immigrant children to attend school and be treated equally to native students regardless of migratory status. Healthcare access in public hospitals were granted to immigrant children and pregnant women. |
| China | 1994 | The Hundred Talents Program | Increase Volume | The initiative is one of the earliest and biggest programs in China to attract qualified scholars to conduct research in China. One-time research grant of up to $2M RMB plus housing allowance are provided to qualified personnel. Applicants need to be under 40 and work full time in China. |

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**Table D25 – continued from previous page**

| Country | Year | Title | Impacts | Brief Description |
|---|---|---|---|---|
| China | 1996 | Administration of Employment of Foreigners in China | Decrease Rights | The law set the guidelines for the employment of foreigners in China. This includes provisions such as - Employees without Chinese nationality must obtain an employment license; foreigners entering China for employment purposes must hold an employment visa and can only be hired for positions which cannot be filled by a Chinese national; provides exemptions for UN employees. Labour contracts with foreign workers shall not exceed 5 years. Wage, minimum wage, labour disputes and working conditions of foreign employees shall be governed by local Chinese law, etc. |
| China | 2004 | Decree No. 47, 2004: Measures for the Administration of Examination and Approval of Aliens' Permanent Residence in China | Increase volume | The act specified ""Green Card"" policy for China into 3 categories: technical, investment, and marriage. To qualify for technical immigration, aliens need to hold title of associate director/associate professor equivalent or above. Investment category required at least $500,000 investment into national recommended industries or some less developed regions. Marriage category required living in China for at least 5 years with spouse who is Chinese or has obtained permanent residency. |
| China | 2013 | Administrative Regulations of the People's Republic of China on Entry and Exit of Foreigners | Increase volume | Visa categories were increased from 8 to 12 with adjusted scopes for F, X and Z visa. ""Illegal employment"" fine increased from 1,000 RMB to 10,000 RMB per person for the employer but not exceeding 100,000 RMB. Foreign individual would be fined for 5,000 - 20,000 RMB with potential detention of up to 15 days. Foreign students with X visa were allowed to work off-campus. |
| China | 2008 | The Thousands Talent program | Increase Volume | The program established in 2008 by the central government of China to recognize and recruit leading international experts in scientific research, innovation, and entrepreneurship. |

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**Table D25 – continued from previous page**

| Country | Year | Title | Impacts | Brief Description |
|---|---|---|---|---|
| Germany | 2000 | The Green Card Initiative | Increase volume | This initiative provided a non-bureaucratic means of bringing foreign experts in the information and communication technology (ICT) field to Germany. 20,000 temporary visas were created, but the program was discontinued at the end of 2004. |
| Germany | 2005 | Immigration Act of 2005 (Complete Overhaul of German Migration Policy) | Increase volume; Increase rights | This act amended the Nationality Act and introduced a new Residence Act. It simplified and reduced the number of residence titles to two: a temporary residence permit and a permanent settlement permit. For the first time, the focus was placed on long-term permanent residency for migrants, in particular for skilled workers, and on integration measures. |
| Germany | 2012 | EU Blue Card (Article 19a, German Residence Act) | Increase volume | The Blue Card introduced based on the Blue Card Directive (Directive 2009/50/EC) was designed to create a European equivalent of the popular US Green Card. In particular, this law has streamlined visa application and right of residence procedures for skilled professionals from abroad. Highly qualified members of third countries can apply for the Blue Card. Relatives of the applicant receive a work permit in parallel. |
| Germany | 2016 | Integration Act of 2016 | Increase rights | The Integration Act and the Regulation on the Integration Act aim to facilitate the integration of refugees into German society. |
| India | 2005 | Ramanujan Fellowship | Increase Volume | Ramanujan Fellowship is meant for brilliant Indian scientists and engineers from outside India to take up scientific research positions in India, those Indian scientists/engineers who want to return to India from abroad. The fellowship is scientist-specific and very selective. The Ramanujan Fellows could work in any of the scientific institutions and universities in the country and they would be eligible for receiving regular research grants through the extramural funding schemes of various S&T agencies of the Government of India. |

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Table D25 – continued from previous page

| Country | Year | Title | Impacts | Brief Description |
|---|---|---|---|---|
| India | 2016 | India Corporate Internship | Increase Volume | The program aims at attracting overseas Indians who are currently pursing graduate studies outside India in Management/Engineering/-Science & Technology to intern in India for 2 to 6 months. In summer 2016, 60 paid internship opportunities will be available at 23 well-known Indian companies. |
| Japan | 1992 | Foreign Trainee Program | Extend Duration | For foreign trainees in Japan, if certain proficiency was achieved for language and professional skills, they were allowed for another 1 year and 3 month of work status. |
| Japan | 1993 | Technical Internship Trainee Program | Increase Volume | Foreign workers were issued training status for 1 year and 2-year work status if they pass tests at the end of the training. Trainees could only be sent from Japanese company's overseas branch. |
| Japan | 2010 | Basic Guidelines related to Policies for Foreign Residents of Japanese Descent | Increase Rights | This guideline promotes the acceptance of Japanese descendants who lacks language proficiency. The government will provide daily life support, offer jobs and respect diverse culture. |
| Japan | 2012 | Point System for Highly Skilled Foreign Professionals | Increase Volume | A point-based system was established to attract highly-skilled foreign professionals. Three types of professionals are given preferential immigration treatment: advanced academic researcher, advanced specialist/technician and advanced business managers. In each category, points were given to academic achievement, work experience, annual income and other factors. If total points reach 70, the professional will be granted a status of residence. |

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Table D25 – continued from previous page

| Country | Year | Title | Impacts | Brief Description |
|---|---|---|---|---|
| Japan | 2014 | The Act for Partial Amendment of the Immigration Control and Refugee Recognition Act | Increase Volume | Reorganizes the statuses of residence such as by establishing a status of residence for foreign nationals who possess advanced and specialized skills in order to promote the acceptance of foreign nationals who will contribute to the development of the Japanese economy amid economic globalization, and takes such measures as further facilitating the procedures for landing examinations, etc. |
| Japan | 2015 | Revised Point System for Highly Skilled Foreign Professionals | Increase Volume | Highly skilled professional became a type of visa. The revision is meant to make foreign professionals come to Japan more easily than before. |
| Mexico | 2010 | Reform to Article 67 of General Law of Population | Increase Rights | The revision allowed migrants to report human rights violation and granted migrants rights to receive aid in event of disasters and medical treatment if their life is in danger. |
| Mexico | 2011 | Migratory Act of May 25th | Increase Rights | The Migration Law eliminated over 70 articles in the Gernal Law of Population and is now the immigration law in Mexico. The law guaranteed foreigners the right to education, health services and judicial rights. The Center for Evaluation and Control of Trust would be created to oversee the conduct of the immigration authorities. The new law has four new categories of immigration permits: Visitor, Student, Temporary Resident, and Permanent Resident. Recognition of the right's immigrants acquire, whereas foreigners with family, labor, and business ties to Mexico generate a series of rights and commitments as of the time in which they begin their day-to-day lives in Mexico, even if they have fallen into irregular migratory status for administrative reasons and provided, they have complied with applicable law. |

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Table D25 – continued from previous page

| Country | Year | Title | Impacts | Brief Description |
|---|---|---|---|---|
| Mexico | 2012 | Guidelines for Immigration Procedures and Proceedings | Decrease Rights | Mexican companies wishing to hire foreigners must obtain evidences of registration with the National Immigration Institute. Foreigners cannot change status within Mexico from a visitor visa to a work permit. |
| Mexico | 2014 | Amendment to the Immigration Law | Extend Duration, Increase Volume | A new 10-year visitor's visa was introduced for family members of a Mexican citizen or of current temporary resident and permanent resident. Income and saving requirements for temporary resident and permanent resident have been reduced. |
| Philippines | 1996 | Migrant Workers and Overseas Filipinos Act | Increase Volume | The act established the replacement and monitoring centre jointly responsible by the department of labor and employment, overseas workers welfare administration and Philippines overseas employment administration. The centre offers returnees skill training, job opportunities, livelihood programs and etc. |
| Philippines | 2002 | Balikbayan Program (Republic Act No. 9174) | Increase Volume, Increase Rights | This program amended the Republic Act No. 6768 enacted in 1989 and granted more benefits and privileges to the balikbayan (overseas Filipino returning to the Philippines, including former Filipinos who have acquired foreign citizenship). The program granted balikbayan and their immediate families visa-free entry and stay for up to one year and tax exemption for certain purchase. |
| Philippines | 2009 | Changes to Alien Employment Permits (Department Order 97-09) | Decrease Volume | The order aims to prevent foreigners from taking jobs that could be filled up by Filipinos. DOLE may inspect the establishments employing aliens to verify the legitimacy of the employment. Aliens whose Alien Employment Permit (AEP) application was denied would not be allowed to apply for a new AEP application. |

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**Table D25 – continued from previous page**

| Country | Year | Title | Impacts | Brief Description |
|---|---|---|---|---|
| Philippines | 2012 | Changes to Alien Employment Permits (Department Order 120-12) | Decrease Volume | This change requires that aliens to appy for a new AEP if a new job position is assumed within their current organization or start employment in a new company. Fines were established for aliens found working in the Philippines without a valid AEP as well as for organizations employing them. Processing time of AEP application was reduced. |
| Philippines | 2013 | Extension of Visa Stay | Extend Duration | Duration of stay for aliens without visa from 151 countries (including US) was extended from 21 days for 30 days |
| Philippines | 2015 | Changes to Alien Employment Permits | Decrease Volume | This change affects aliens who wish to work in Philippines and the processes to acquire an AEP. Notable changes include a more detailed description of an AEP needs to be published in newspaper and on the DOLE for 30 days; an understudy training program for training two Filipino nationals is required for each AEP application; and the processing fees was increased. |
| Portugal | 2001 | Law-Decree nº4/2001 of January 10: immigration law | Increase Volume | A new temporary work visa category ""stay permit"" was created for foreigners who has a work contract. The stay permit was valid for one year with the possibility of extending to a maximum of five years. Foreigners were allowed to bring their family members to the Philippines and at the end of the five-year period, foreigners can apply for a resident permit. |
| Portugal | 2003 | Law-Decree nº34/2003 of February 25: immigration law | Decrease Volume | ""Stay permit"" was abolished in this version of the immigration law. A system of quotas was established based on a report on domestic skill shortage in each sector. Employers need to go through a complex procedure to employ foreigners. |

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**Table D25 – continued from previous page**

| Country | Year | Title | Impacts | Brief Description |
|---|---|---|---|---|
| Portugal | 2012 | Golden Visa Program | Increase Volume | This scheme grants foreign individuals a golden visa (permanent residency) if they fall into three categories: 1) invest 500,000 in real estate; 2) make capital transfer of at least 1M Euro or 3) create 10 jobs. If visa holder stayed at least 7 days in year 1 and 14 days in the remaining 4 years, he/she can apply for citizenship. |
| South Korea | 1991 | Industrial and Technical Training Program for Foreigners (ITTP) | Increase Volume | This program allowed Korean companies overseas to train foreign employees. The trainees could stay for six months with a possible extension for another six months. |
| South Korea | 1992 | ITTP | Increase Volume | The change allowed small and medium businesses without overseas presence to bring in foreign trainees as well. The duration of stay for trainees was one year. |
| South Korea | 1993 | Industrial Trainee System (ITS) | Increase Volume | This program was an extended application of ITTP. The duration of stay for trainees was extended to two years. ITS specifically targeted small and medium enterprises in the manufacturing sector that was experiencing labor shortage. The quota for industrial trainee was set at 20,000. |
| South Korea | 1994 | ITS | Increase Volume | The quota for industrial trainee was increased to 30,000 |
| South Korea | 1995 | A Measure Pertaining to the Protection and Control of Foreign Industrial and Technical Trainees | Increase Rights | Foreign trainees should be paid directly from the employers and at least the minimum wage set by the government. Trainees no longer need to surrender their passports to employers or to any other party. |
| South Korea | 1996 | ITS | Increase Volume | The quota for industrial trainee was increased to 80,000 |

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**Table D25 – continued from previous page**

| Country | Year | Title | Impacts | Brief Description |
|---|---|---|---|---|
| South Korea | 1998 | Working After Training Program for Foreigners | Extend Duration, Increase Rights | Foreign trainees who passed certain skill tests after a two-year training period were allowed to work in Korea for another year under visa category of ""working after training (E-8)"". Workers after training were entitled to the same rights enjoyed by their Korean colleagues. |
| South Korea | 1999 | Act on Immigration and Legal Status of Overseas Koreans (The Overseas Korean Act) | Increase Volume, Increase Rights | The act allowed overseas Korean to stay and work in Korea without restrictions upon receiving an Overseas Korean (F-4) visa. The act grants the same economic and social rights held by Korean citizens to overseas Korean. |
| South Korea | 2002 | ITS | Increase Volume | The quota for industrial trainee was increased to 85,500 |
| South Korea | 2004 | Employment Permit System | Increase Volume | This program allows employers to hire foreign workers in the labor shortage industries such as agriculture & stockbreeding, fishery, construction and manufacturing with less than 300 regular workers. Foreign workers are granted 'Nonprofessional Employment' (E-9) visas. |
| South Korea | 2007 | Working Visit Program | Increase Volume, Extend Duration | This program grants ethnic Koreans who hold foreign citizenship, mainly from China and Soviet Unions a working visit (H-2) visa. Visa holders can freely enter and exit Korea for five years and get employed in any company in Korea for three years. |
| South Korea | 2009 | Contact Korea | Increase Volume; Increase Rights | Contact Korea is the government organization representing the Republic of Korea that is exclusively charged with the attraction of global talented professionals. Contact Korea includes an online platform for global talents to apply for jobs in both private and public sectors in Korea. The platform serves as a one-stop shop by providing services such as arranging online interviews, verfiying academic and professional background and dealing with visa and immigration issues. |

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Table D25 – continued from previous page

| Country | Year | Title | Impacts | Brief Description |
|---|---|---|---|---|
| South Korea | 2010 | HuNet Korea Immigration Network and Policy | Increase Volume, Increase Rights | A new online visa application system (HuNet Korea) would be implemented to include visa application and job bank for foreign professionals. Re-entry procedure for foreign spouses and students was simplified. A point system would be implemented for professionals who wish to obtain resident or permanent resident status in Korea. Foreigners could also obtain residency by investing in real estate in designated local areas, for example in Jeju-si. Number of sites for naturalization interview tests were increased to make it more convenient for immigrants. |
| Spain | 1996 | Royal Decree 155/1996 - approving the implementation of regulations of Organic Law 7/1985 | Increase Rights | This amendment stated that foreigners with legal status have the rights to access education and other resources. Foreigners could obtain permanent residency after 6 years or 5 years if they have permanent job permit. |
| Spain | 2003 | Organic Law 14/2003 - amendment to Organic Law 8/2000 | Increase Rights, Increase Volume | This amendment increased rights to the family of legal foreigners, such as spouse could obtain his/her own residence permit when given work permit and children could obtain their own permit upon reaching adulthood. Each year government would review annual foreign worker quota. |
| Spain | 2009 | Organic Law 2/2009 - amendment to the Organic Law 4/2000 | Increase Rights | This amendment added article 2b which focused on integration of immigrants. Article 6 stated that foreign residents have rights to vote in municipal elections. Article 12 stated that foreigners have access to healthcare under the same condition as citizens. Article 38s stated that highly qualified residence would be able to obtain residence permit and EU blue card. |

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**Table D25 – continued from previous page**

| Country | Year | Title | Impacts | Brief Description |
|---|---|---|---|---|
| Taiwan | 1992 | Employment Service Act | Decrease Volume, Decrease Rights | The act was the first law in Taiwan to legalize hiring of certain foreign workers, strengthen the legal rights of employees, and impose sanctions on employers who hired illegal foreign laborers. Employment for foreign workers was limited to a maximum of two-year term and blue-collar foreign workers are prohibited to marry Taiwanese during employment. |
| Taiwan | 2014 | Amendments to the Regulations Governing Visiting, Residency, and Permanent Residency of Aliens | Extend Duration, Increase Rights | Adult children of foreign residents who grew up in Taiwan are able to apply for two three-year extensions of residency if they meet certain requirements. Foreign professionals who have completed their previous work assignments have up to six months of extended residency to seek new employment in Taiwan. Foreign students who graduated from Taiwan universities also have a six-month extension of residency. They qualify for employment without needing the two years of work experience as previously required. |
| Taiwan | 2015 | Global Recruiting Platform | Increase Volume | A Recruitment Policy Committee was established under the Executive Yuan that included representatives from ministries such as Economic Affairs, Education, Labour, Health and Welfare and National Immigration Agency. The platform aims to attract highly-skilled professionals from overseas to live and work in Taiwan. |
| UK | 1996 | Asylum and Immigration Act | Decrease rights | The act made it a criminal offence to employ anyone unless they had permission to live and work in the UK. |
| UK | 2006 | Immigration, Asylum and Nationality Act | Decrease Rights; Increase Rights | A five-tier points system for awarding entry visas was created. Those refused work or study visas had their rights of appeal limited. The act brought in on-the-spot fines of £2,000, payable by employers for each illegal employee, which could include parents taking on nannies without visas. |
| | | | | United States |

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**Table D25 – continued from previous page**

| Country | Year | Title | Impacts | Brief Description |
|---|---|---|---|---|
| 1990 | Immigration Act | Increase volume | Increased legal immigration ceilings. Created a diversity admissions category. Tripled the number of visas for priority workers and professionals with U.S. job offers. | |
| United States | 1998 | American Competitiveness and Workforce Improvement Act (ACWIA) | Increase volume | increased this annual cap of H1B visas from 65000 to 115,000 for Fiscal Year 1999 and 2000; and 107,500 in Fiscal Year 2001. The cap returned to 65,000 starting with Fiscal Year 2002. |
| United States | 2000 | American Competitiveness in the 21st Century Act of 2000 | Increase volume | The quota was increased to 195,000 H-1B visas in fiscal years 2001, 2002, and 2003 only. Nonprofit research institutions sponsoring workers for H-1B visas became exempt from the H-1B visa quotas. |
| United States | 2004 | H-1B Visa Reform Act of 2004 | Decrease volume | Reduction in the H-1B cap from 195,000 to 65,000 visas, but declaring exemptions for the first 20,000 applicants each year with graduate degrees. Additional restrictions and regulations for L-1 Visas (intra-company short visits). |
| United States | 2009 | Employ American Workers Act | Decrease volume | For employers who applied to sponsor a new H-1B and who had received funds under either the Troubled Asset Relief Program (TARP) or the Federal Reserve Act Section 13, the employers were required to attest that the additional H-1B worker would not displace any U.S. workers. |
| United States | 2015 | Rule about work authorization for certain H-4 holders | Increase rights | Allows certain spouses of H-1B workers to be eligible for work authorization. |

## D.2 Construction of a Database of Migration Reforms

### Collecting Reforms

In constructing a sample of reforms, our starting point was the work of Branstetter et al. (2006), who indexed global intellectual property reforms. The countries indexed in the final data are: Brazil, Canada, Chile, China, Germany, India, Italy, Japan, Mexico, the Philippines, Portugal, South Korea, Spain, Taiwan, and the United Kingdom. Countries were selected based on the presence of: (a) historical enactment of intellectual property legislation supportive of patenting; (b) multinational activity; and (c) significant migration flows. Ten of these countries coincide with the sample analyzed in Branstetter et al. (2006), who studied the impact of systematic reforms designed to strengthen and standardize intellectual property on MNEs' resulting foreign direct investments from 1982 to 1999. Relative to that study, we expanded the sample to five additional countries with the aim of including countries that are the source and destination of significant migration flows. For instance, Canada and the United Kingdom were in the top four most frequent destinations of OECD migration in 2010, while India, the Philippines, and the United Kingdom experienced the most net emigration in 2010 (Kerr et al., 2016). Additionally, several of the countries in the list are representative of high levels of net inventor immigration.

After identifying a list of countries, we turned to collecting reforms. During the period of 2017 through summer 2020, teams of research assistants and the authors identified migration policy reform events impacting high-skilled human capital migration of two types into a focal country: (a) return migrants and (b) foreign immigrants. Alongside identification, the team collected corresponding primary and secondary sources related to reforms. Collection occurred in three waves—the first in 2017, the second in Winter 2018 to Summer 2019, and the third in Summer 2020. The latter two focused on ensuring complete collection of reforms enacted in the period of 1990 to 2016. Where additional reforms were identified outside this period, they were included in the dataset. As a result, the database of reforms is primarily useful for analyses on the post-1990s era and is less reliable for reforms and initiatives prior to this point.

Starting from the second wave, we began collecting reforms, following a standardized heuristic with emphasis on ensuring completeness in the dataset. First, we conducted a search to collect any primary or secondary news sources related to the countries under review from websites that focused on information related to migration policies and programs of countries, including websites focused on assisting immigration and websites focused on the navigating migratory legislative policies of countries. Example websites include: LegislateOnline, (http://www.legislationline.org/); the Library of Congress, (https://www.loc.gov/law/help/migration-citizenship/); and that of the think tank Migration Policy (http://www.migrationpolicy.org). Website-based searches would also turn to legal codes of countries published online by their central governments; we searched explicitly for links and connections to the codified migration laws of a country (e.g., legal codes of all European Union countries are indexed on EU websites). After website searches, we searched academic repositories for articles with comprehensive explanation of migration policy reforms and initiatives. Finally, these searches were followed by a series of keyword-based searches implemented in the Wikipedia online encyclopedia (https://www.wikipedia.org/) and Google's web search engine focused on identifying articles, information, and primary sources related to migration policy reforms, migration policy initiatives, and high-skilled human capital immigration into and out of a country. Iteration between approaches occurred as necessary (for example, if Wikipedia revealed several individual laws or programs to search for, the researcher would spend time looking for primary sources for those laws or programs in legal code and government websites). Table D27 provides a list of example searches utilized in the search process.

#### Categorizing Reforms

To characterize the anticipated impacts of reforms, the authors qualitatively assessed each reform and the associated primary and secondary sources. Based on this analysis, reforms were coded according to whether the anticipated effects were positive (easing movement) or negative (restricting movement) based on how the reforms impacted legal migration frameworks of countries. Specifically, reforms were classified as positive or negative according to anticipated impact along three dimensions: (a) the rights of a migrant (either foreigners or returnees); (b) the expected volume of migrants post reform; and/or (c) the duration of stay or required time to achieve residency status criteria associated with admission to a country. Reforms identified as generating increases (alt. decreases) along any of these dimensions were then codified as having a positive (alt. negative) effect. While rare, some reform packages simultaneously enacted provisions exhibiting both positive and negative effects. For such reform events, we treat the event as an instance of both a positive reform and a negative reform. For example, in 2006, the U.K. enacted administrative regulations that increased the number of visas awarded, which increased work rights for migrants with accepted visas, but also decreased rights for those who encountered visa refusals (limitation of rights to appeal). As a result, this reform is coded both as a positive and negative reform event for the United Kingdom in 2006.

Table D28 considers the subsample of all reforms affecting business migration and presents counts summarizing reform distribution across countries by its expected impact (positive, negative, or both), by its importance in determining migration flows (major vs. minor), and by immigrant type affected (returning citizen vs. foreigner). Here we include only the reforms taking place during the years 1990 to 2016, which correspond to the period analyzed in this paper. Most countries in our sample have at least three reforms within the 26 years, while some countries (such as China, Japan, and South Korea) have six or more. A large majority of reforms—85%—target foreigners, while only 15% explicitly target returnee migrants. Reforms during the period leaned toward positive interventions, anticipated to increase migration, with 44 identified instances of anticipated positive effects, two identified instances where the outcome is ambiguous because the new legislation includes both positive and negative aspects, and only 12 with anticipated negative effects. In our complete dataset, we also collected reforms affecting student migrants or entrepreneurs. More details are available upon request.

**Table D26:** Criteria for the selection of the final sample

| Country code | OECD | Share migrants (always) | N. Patents | N. GCPs | N. subsidiaries | BFF | BCSS | Reason of difference with BFF |
|---|---|---|---|---|---|---|---|---|
| US | 1 | 5,2% | 129214 | 14668 | 23158 | 0 | 1 | More than 2000 subsid, more than 1% GMIs |
| JP | 1 | 2,4% | 44937 | 1420 | 2628 | 1 | 1 | |
| KR | 1 | 3,4% | 17855 | 738 | 1337 | 1 | 1 | |
| DE | 1 | 1,5% | 15772 | 3613 | 3222 | 0 | 1 | More than 2000 subsid, more than 1% GMIs |
| GB | 1 | 1,0% | 7183 | 2650 | 2226 | 0 | 1 | More than 2000 subsid, more than 1% GMIs |
| CA | 1 | 1,4% | 7033 | 2384 | 2408 | 0 | 1 | More than 2000 subsid, more than 1% GMIs |
| FR | 1 | 0,6% | 6607 | 1790 | 1712 | 0 | 0 | |
| IL | 1 | 0,7% | 3454 | 878 | 999 | 0 | 0 | |
| CH | 1 | 0,9% | 2989 | 1378 | 925 | 0 | 0 | |
| IT | 1 | 0,2% | 2925 | 709 | 1175 | 0 | 0 | |
| NL | 1 | 0,5% | 2814 | 927 | 795 | 0 | 0 | |
| SE | 1 | 0,6% | 2759 | 764 | 689 | 0 | 0 | |
| AU | 1 | 0,5% | 1623 | 552 | 761 | 0 | 0 | |
| AT | 1 | 0,5% | 1499 | 565 | 463 | 0 | 0 | |
| BE | 1 | 0,3% | 1481 | 749 | 491 | 0 | 0 | |
| FI | 1 | 0,3% | 1382 | 313 | 344 | 0 | 0 | |
| DK | 1 | 0,3% | 1159 | 334 | 378 | 0 | 0 | |
| ES | 1 | 0,2% | 929 | 337 | 474 | 1 | 1 | |
| IE | 1 | 0,5% | 689 | 403 | 219 | 0 | 0 | |
| NO | 1 | 0,2% | 587 | 186 | 262 | 0 | 0 | |
| NZ | 1 | 0,3% | 299 | 108 | 167 | 0 | 0 | |
| CZ | 1 | 0,1% | 296 | 151 | 113 | 0 | 0 | |
| MX | 1 | 0,0% | 284 | 136 | 143 | 1 | 1 | |
| PL | 1 | 0,1% | 273 | 130 | 145 | 0 | 0 | |
| HU | 1 | 0,1% | 185 | 99 | 75 | 0 | 0 | |
| TR | 1 | 0,1% | 154 | 68 | 94 | 0 | 0 | |
| GR | 1 | 0,0% | 100 | 49 | 57 | 0 | 0 | |
| LU | 1 | 0,5% | 98 | 77 | 34 | 0 | 0 | |
| PT | 1 | 0,1% | 87 | 38 | 67 | 1 | 1 | |
| CL | 1 | 0,3% | 50 | 17 | 41 | 1 | 1 | |
| SI | 1 | 0,3% | 45 | 14 | 32 | 0 | 0 | |
| SK | 1 | 0,0% | 42 | 29 | 30 | 0 | 0 | |
| EE | 1 | 0,1% | 37 | 19 | 21 | 0 | 0 | |
| CO | 1 | 0,1% | 36 | 14 | 27 | 1 | 0 | Less than 100 subsid and less than 0.2% GMIs |
| TU | 1 | 0 | 0 | 0 | 0 | 1 | 0 | Less than 100 subsid and less than 0.2% GMIs |
| VE | 1 | 0 | 0 | 0 | 0 | 1 | 0 | Less than 100 subsid and less than 0.2% GMIs |
| CN | 0 | 3,8% | 12163 | 2686 | 2488 | 1 | 1 | |
| TW | 0 | 2,0% | 10526 | 1164 | 1771 | 1 | 1 | |
| IN | 0 | 0,8% | 4232 | 2009 | 721 | 1 | 1 | |
| SG | 0 | 0,5% | 1131 | 512 | 286 | 0 | 0 | |
| HK | 0 | 0,8% | 561 | 209 | 255 | 0 | 0 | |
| RU | 0 | 0,1% | 537 | 263 | 216 | 0 | 0 | |
| BR | 0 | 0,1% | 413 | 194 | 196 | 1 | 1 | |
| SA | 0 | 0,7% | 372 | 94 | 36 | 0 | 0 | |
| MY | 0 | 0,2% | 314 | 151 | 105 | 0 | 0 | |
| ZA | 0 | 0,3% | 171 | 68 | 87 | 0 | 0 | |
| TH | 0 | 0,1% | 124 | 64 | 68 | 1 | 0 | Less than 100 subsid and less than 0.2% GMIs |
| RO | 0 | 0,1% | 108 | 62 | 43 | 0 | 0 | |
| AR | 0 | 0,1% | 90 | 56 | 56 | 1 | 0 | Less than 100 subsid and less than 0.2% GMIs |
| PH | 0 | 0,2% | 73 | 44 | 36 | 1 | 1 | |
| UA | 0 | 0,1% | 72 | 42 | 38 | 0 | 0 | |
| AE | 0 | 0,2% | 68 | 37 | 37 | 0 | 0 | |
| EG | 0 | 0,0% | 50 | 33 | 26 | 0 | 0 | |
| BG | 0 | 0,1% | 45 | 23 | 24 | 0 | 0 | |

Sample of countries with at least 20 subsidiaries patenting on average between 2010 and 2015. BFF indicates the list of , while BCSS indicates our list. OECD indicates whether the country is a member of the OECD.

**Table D27:** Example Keyword Terms Leveraged in Search

| Wikipedia | Google: HS HC | Google: Catch-All |
|---|---|---|
| 1. Migration in <Country> | 1. Entrepreneurship Immigration <Country> | 1. Move to <Country> |
| 2. History of Migration in <Country> | 2. Start a Business as an Immigrant <Country> | 2. Immigrate to <Country> |
| 3. Migration Policy <Country> | 3. STEM Incentives <Country> | 3. Immigration to <Country> <Nationality> Heritage |
| 4. <Nationality> Citizenship | 4. High Skill Migration <Country> | 4. Migration Policy <Country> |
| 5. Citizenship in <Country> | 5. Refugee Immigration <Country> | 5. History of Migration <Country> |

**Table D28:** Classification of Reforms

| | Positive vs Negative | | | Major vs Minor | | Migrants vs Returnees | |
|---|---|---|---|---|---|---|---|
| Countries | positive | negative | Both | major | minor | migrants | returnees |
| Brazil | 1 | 0 | 0 | 1 | 0 | 1 | 0 |
| Canada | 0 | 1 | 0 | 1 | 0 | 1 | 0 |
| Chile | 1 | 0 | 0 | 1 | 0 | 0 | 1 |
| China | 4 | 1 | 0 | 5 | 1 | 4 | 2 |
| Germany | 4 | 1 | 0 | 3 | 2 | 4 | 0 |
| India | 2 | 0 | 0 | 0 | 2 | 0 | 2 |
| Japan | 6 | 0 | 0 | 2 | 4 | 5 | 1 |
| Mexico | 3 | 1 | 0 | 1 | 3 | 4 | 0 |
| Philippines | 3 | 3 | 0 | 3 | 3 | 4 | 2 |
| Portugal | 2 | 1 | 0 | 2 | 1 | 3 | 0 |
| South Korea | 13 | 0 | 0 | 7 | 6 | 11 | 2 |
| Spain | 3 | 0 | 0 | 3 | 0 | 3 | 0 |
| Taiwan | 2 | 1 | 0 | 2 | 1 | 3 | 0 |
| United Kingdom | 0 | 1 | 1 | 2 | 0 | 2 | 0 |
| United States | 4 | 2 | 0 | 4 | 2 | 6 | 0 |
| TOTAL | 48 | 12 | 2 | 37 | 25 | 51 | 10 |

# E Estimation of Treatment Effects Give Frequently Repeated and Clustered Events

## E.1 A Generalized Estimator

In a classical difference-in-differences or event-based approach, the key term of interest is an indicator variable or series of relative event-time indicators that take the value 1 in the periods of and subsequent to treatment. The coefficient on this key term estimates the mean difference in the response in the period(s) surrounding treatment with emphasis on those subsequent to treatment.[28] This model is inflexible in the case of repeated treatment, and standard practice is to discard observations where repeated treatment occurs. This is not feasible in all situations, however, including those where treatment events are clustered at the level of the group among observations with few group categories or where treatment events are clustered in time, as in our data.

To accommodate, we relax the requirement that the time periods examined in the difference-in-differences estimator include only the singular enactment of an event; we treat the difference-in-differences estimator key term as a non-negative count of events enacted that can vary over time. Generalizing from the regressions in our analyses, we allow variations on models of the general form:

$$Y_{it} = f(\gamma_i + \gamma_t + \beta \; r_{it}; \; \epsilon_{it}),$$

where $Y_{it}$ represents the response variable in time $t$ for observation group $i$, $\gamma$ indexes time and group fixed effects, and $r_{it}$ is the count of treatment events implemented to date for group $i$ in

[28] Borusyak and Jaravel (2017) presents canonical equations that outline the generalized event-based estimator and which relate difference-in-differences specifications to event-study specifications by demonstrating that the estimator is a specific case of a more general event-study specification with dynamic treatment effects. Goodman-Bacon (2021) examines the case of difference-in-differences estimation conditional on variation in treatment timing. This author shows that the treatment effect estimated is a weighted average of the treatment effect of the component difference-in-differences estimates and proposes a test for the validity of such estimators.

time $t$, and $\epsilon_{it}$ is the standard error term.[29] When only a singular event is ever enacted for any given observation. This model is equivalent to classical difference-in-differences or event-based approaches that include fixed effects that subsume the independent effects of time and treatment.

In this model, the key coefficient of interest is $\beta$, and it is interpreted as the average per-period increase in the response conditional on an additional event. For simplicity, the measurement $r_{it}$ assigns equal weight to each consecutive reform of the same type and, as a result, imposes the restriction that the average treatment effects of a given reform event type must be equivalent across reform events.

A generalized version of this measure might estimate treatment effects independently, including linearly additive indicators for each level of consecutive treatment such that $r_{it} = \sum_j^J \sum_{t=0}^{T=t} \mathbb{1}(event_{it,j})$, where $j$ indexes the various levels of treatment and where coefficients are estimated for each level of $j$. To economize on statistical power and maintain simplicity, we impose the restriction of equivalence in effect across treatment levels in our analyses.

Causal inference given this estimator requires additional assumptions. The literature on causal inference in the presence of repeat events (e.g., Blackwell 2013) suggests two. First, it is necessary to assume that treatment events are linearly additive in their effects and exhibit independence otherwise, with no interaction across treatment levels. Second, it also must be assumed that treatment is orthogonal to the consequences of the treated unit's prior treatment history—i.e., future treatment and impacts on the response are not significantly determined by the prior sequence of past treatment.

## E.2 Simulation of Estimator Measurement Error

To evaluate whether this estimator accurately measures the corresponding causal treatment effect, we conducted computational simulations in which data based on parameters in our setting were simulated, and the model fit repeatedly across several simulations. Specifically, for each simu-

[29] In other words, $r_{it} = \sum_{t=0}^{T=t} \mathbb{1}(event_{it})$.

lation $s$, data were generated from the following process involving "Reform Events" across eight years ($y$) affecting 15 "Countries" ($c$) and 10 "Firms' ($f$) present within those countries (where other parameters were chosen to approximate sample means in the actual data observed where possible[30]):

1. **Simulate Country Treatment Pathways:** A treatment event pathway was assigned for each simulated country with random variation in the frequency of treatment events within a given country that was defined by random variation in the probability of treatment event occurrence across countries. This occurred in two steps:
   (a) **Assign Random Country-Level Probability of Per-Year Treatment From Uniform Distribution:** $p_{cs} \sim \mathcal{U}(0, 0.4)$
   (b) **Determine Treatment Pathway From Binomial Distribution:** $T_{cys} \sim \mathcal{B}(p_{cs})$
2. **Simulate One-Way Fixed Effects:**
   (a) **Simulate Assignee Fixed Effects:** $\gamma_{fs} \sim \mathcal{N}(\mu = 10, \sigma = 3)$
   (b) **Simulate Year Fixed Effects:** $\gamma_{ys} \sim \mathcal{N}(\mu = 0, \sigma = 3)$
   (c) **Simulate Country Fixed Effects:** $\gamma_{cs} \sim \mathcal{N}(\mu = 0, \sigma = 3)$
3. **Simulate Two-Way Fixed Effects:**
   (a) **Simulate Assignee-Year Fixed Effects:** $\gamma_{fys} \sim \mathcal{N}(\mu = 0, \sigma = 3)$
   (b) **Simulate Country-Year Fixed Effects:** $\gamma_{cys} \sim \mathcal{N}(\mu = 0, \sigma = 3)$
   (c) **Simulate Subsidiary (Assignee-Country) Fixed Effects:** $\gamma_{fcs} \sim \mathcal{N}(\mu = 0, \sigma = 3)$
4. **Simulate Random Noise:** $\epsilon_{fcys} \sim \mathcal{N}(\mu = 0, \sigma = 1)$
5. **Simulate Treatment Effect w/Random Variance Across the Year-Firm-Country Level:** $D_{fcys} \sim \mathcal{N}(\mu = 3, \sigma = 1)$

[30] While fixed effects are estimates from a consistent normal distribution, the results prove robust to estimating fixed effects based on by-variable mean and standard deviation point estimates from a regression on the data that includes only fixed-effect terms.

6. **Compute Linearly Additive Response Based on Differing Treatment Modes:**

   (a) **Treatment Affects Rate:** $y_{fcps} = \gamma_{fs} + \gamma_{cs} + \gamma_{ys} + \gamma_{fcs} + \gamma_{fys} + \gamma_{cys} + \sum_{t=0}^{T=t}(T_{cys}) \times D_{fcys} + \epsilon_{fcys}$

   (b) **Treatment Affects Level:** $y_{fcps} = \gamma_{fs} + \gamma_{cs} + \gamma_{ys} + \gamma_{fcs} + \gamma_{fys} + \gamma_{cys} + T_{cys} \times D_{fcys} + \epsilon_{fcys}$

For each of the 5,000 simulations, we then fit the following regressions:

$$y_{fcps} = \gamma_{fs} + \gamma_{cs} + \gamma_{ys} + \beta r_{cys} + \epsilon_{fcys} \qquad \text{Cumulative Estimator}$$

$$y_{fcps} = \gamma_{fs} + \gamma_{cs} + \gamma_{ys} + \beta T_{cys} + \epsilon_{fcys}, \qquad \text{Panel Estimator}$$

where the first equation corresponds to estimating the treatment effect on the cumulative count of events and the second equation corresponds to a panel estimator where the variable of interest takes the value 1 in periods where the event occurs and 0 otherwise. For the resulting key coefficient of interest ($\beta$), we calculated the variance of the resulting estimates and their mean squared error defined as the mean of the square of the differences between the estimate and the actual treatment effect (MSE $= \frac{1}{5{,}000}\sum(3-\beta)^2$).

Table E29 displays the resulting estimates. Readily apparent is that the panel estimator is best suited for contexts where treatment produces a single-period shock to the response and in such cases, it estimates closely the real average treatment effect. However, in the case of repeated events, the cumulative estimator most closely reflects the real average treatment effect. Additionally, when applied to the outcome derived from a model in which treatment influences the rate of the response, the cumulative estimator yields the lowest variance in the estimates as well as the lowest mean squared error across all specifications. Overall, we interpret this as strong evidence for the statistical validity of the cumulative estimator.

**Table E29:** Efficiency of Estimator

| Model | Estimator | $\mu(\beta)$ | Var.($\beta$) | MSE | $\frac{MSE}{TreatEffect}$ | $\frac{Var.(\beta)}{TreatEffect}$ |
|---|---|---|---|---|---|---|
| Rate | Cumulative | 3.006 | 0.349 | 0.349 | 0.116 | 0.116 |
| Rate | Panel | 1.475 | 0.794 | 3.120 | 1.040 | 0.265 |
| Level | Cumulative | 0.783 | 0.406 | 5.319 | 1.773 | 0.135 |
| Level | Panel | 2.984 | 0.688 | 0.688 | 0.229 | 0.229 |

*Notes:* This table provides the results from simulations designed to evaluate the efficiency of the "cumulative events" estimator.