

# Competition and Productivity: Evidence from Peruvian Municipalities

Marc Schiffbauer<sup>a</sup>   James Sampi<sup>b</sup>   Javier Coronado<sup>c</sup>

## Abstract\*

This paper uses new data on the removal of local, sector-specific barriers to firm entry across 1,800 municipalities in Peru to estimate the impact on firm productivity and markups. New legislation in 2013 strengthened the national competition authority's mandate to enforce the elimination of local entry barriers, providing a quasi-experimental setting to identify the impact of competition on productivity within the controlled institutional environment of a single country. We find that the elimination of local entry barriers boosted productivity, pointing to the critical role of subnational market entry barriers in explaining countries' laggard productivity performances despite liberalized national regulatory environments.

**JEL Classification codes:** O33, O47, D72, D24

**Keywords:** competition, TFP, productivity, structural reforms, Peru.

<sup>a</sup> Marc Schiffbauer. The World Bank. Email: [mschiffbauer@worldbank.org](mailto:mschiffbauer@worldbank.org)

<sup>b</sup> James Sampi Bravo. The World Bank and Vrije Universiteit Amsterdam, School of Business and Economics. Email: [jsampibravo@worldbank.org](mailto:jsampibravo@worldbank.org)

<sup>c</sup> Javier Coronado. Garrigues, Pontificia Universidad Católica del Perú and Universidad de Piura: [Javier.coronado@garrigues.com](mailto:Javier.coronado@garrigues.com)

\* The findings expressed in this paper are those of the authors and do not necessarily represent the views of the World Bank or its member countries. We would like to thank David McKenzie, Jorge Araujo, Norman Loayza, Ana Fernandes, Leonardo Iacovone, Tanja Goodwin, Seidu Dauda, and Martha Martinez Licetti for very valuable feedback.

## 1. Introduction

It is commonly accepted that markets with lower barriers to entry are likely to be more competitive than markets with higher barriers to entry, all else equal. Competition in the market is taken as an important driver for productivity as it is thought to foster incentives for firms to innovate, adopt cost-reducing production techniques, or embark on organizational changes. This paper presents empirical evidence of the impact of a large-scale elimination of subnational entry barriers to local markets on firms' productivity growth in Peru.

We use a new data set that captures the effects of a policy change aimed at removing subnational barriers to market entry across 1,800 municipalities in Peru. The data were collected after legal reforms in 2013 and 2014 had strengthened the mandate of Peru's competition authority over the implementation of business regulations by subnational government bodies. We match this data with firm census panel data from 2007-17 to estimate the impact on changes in firms' productivity and markups. We find that the elimination of barriers to entry caused an increase in firm productivity which we regard as evidence of the positive impact of enhanced competition on market efficiencies.

The recent Peruvian experience offers an unusual setting to identify the impact of more competition from the elimination of barriers to entry on firm productivity. First, economic growth is driven by the private sector with little participation of the state in economic activities. Second, most economic policies that may affect local product markets are applied nationwide with very few or no region-, sector-, and industry-specific policies. Third, its large number of municipalities have far-reaching discretion to control critical aspects of local market access. For instance, each municipality defines its own regulatory code to grant operating licenses, issue permits, define sector-specific technical standards, and set regulatory fees and charges. Fourth, the Peruvian

competition authority, Indecopi (Instituto Nacional de Defensa de la Competencia y de Protección de la Propiedad Intelectual), has a legal mandate to issue decisions against regional and local market access regulations that are inconsistent with the national legal framework or considered to lack economic rational. To the best of our knowledge, this is a distinctive feature in that Indecopi, through its Commission for the Elimination of Bureaucratic Barriers (CEB) operating as an independent decision body, can eliminate specific local regulations that are illegal or irrational barriers to market entry. In practice, although the CEB was created in 2007, it is not until the 2013 legal reform that the CEB has the mandate to repeal local government regulations.

Specifically, in 2013, Peru underwent a major reform of its competition authority's mandate to challenge local regulations affecting businesses. The reform allowed Indecopi to perform ex-officio investigations and publicly declare a local regulation as illegal or irrational. Thereafter, firms may denounce a municipality that keeps imposing the barrier which triggers a fast-track sanctioning procedure by Indecopi. This mechanism, along with fines that increased by 400 percent, forced the effective removal of local regulatory barriers that were declared as illegal or irrational either by means of sanctioning procedures or by a preemptive action of local governments to avoid the costly sanctions on individual local public officials.

The change in the legal mandate of Indecopi in the 2013 law unfolded a quasi-experiment as the decisions by the competition authority to remove barriers to entry can be considered as independent from local market conditions and sector or firm characteristics of individual municipalities. We argue that this setting allows to identify the causal effect of the elimination of barriers to entry on firm productivity and markups.

We use administrative data issued by Indecopi on eliminated barriers to local market entry and competition in 2013 and 2014. The data provide information on subnational jurisdictions, the

type of barrier, and the economic activity to which the barrier applies. We match it with firm census data for 2007-17. We follow the integrated approach of De Loecker and Warzynski (2012) which valid under imperfect competition to derive firms' productivity and markups and estimate how they are impacted by the subnational reforms in a difference-in-differences specification. We find that firms operating in municipalities and sectors that eliminated barriers to entry experienced a significant increase in (revenue) total factor productivity (TFP) growth but not in markups, relative to comparable firms not located in reform-municipalities. Given that revenue productivity can rise because of increases in physical productivity or increases in firm-specific prices due to higher local product quality—which raises markups and offsets their decline from lower local rents in reform municipalities-sectors—the results suggest that physical productivity improved.

We provide a range of evidence supporting a causal interpretation of the findings. First, the results are robust to different strategies to identify the control group, for example, by imposing that treated and control firms operate in the same municipality but in reform versus non-reform sectors, or by combining the quasi-experimental setting with a propensity score matching procedure. Second, we cannot reject that the treated and control firms followed parallel productivity trends in pre-reform years. Third, the results are robust to the inclusion of additional municipality-specific time trends, showing that the elimination of local entry barriers was independent of municipalities' TFP trends. Fourth, as expected, the reform impact is strongest when illegal licenses or technical requirements were eliminated that allowed local public officials to restrict the entry of competitors to local incumbents. Fifth, municipalities pre-reform average firm productivity levels, encapsulating the effect of other municipal productivity determinants, do not predict the elimination of local entry barriers. Finally, the results are robust when we estimate the reform impact separately for 2013 and 2014 and when we estimate the reform impact on all

firms in a reform-municipality independent of their economic activity. The latter also allows to make some inference on the importance of productivity spillovers across sectors.

The next section reviews the literature on competition and productivity. Section 3 discusses the empirical identification and describes how the legal reform in 2013 expands the competition authority's mandate over municipalities. Section 4 describes the underlying data. Section 5 presents the empirical strategy and section 6 summarizes the results. The final section concludes.

## **2. Literature Review**

The theoretical literature on endogenous growth offers contrasting views on competition and productivity. The standard model of endogenous technological change predicts that an increase in product market competition can discourage innovations by reducing their durability (i.e., creative destruction) and their expected future profits ("rent dissipation effect").

Aghion et al. (2001) show that a straightforward extension of the basic framework predicts a positive relationship between market competition and growth. They consider an oligopolistic intermediate sector where innovation enables a firm to break away from competition for a certain period. The authors predict that firms operating in sectors with neck-on-neck competition are forced to constantly enhance their cost-effectiveness to make (temporary) profits. In contrast, firms operating in sectors where some firms have an exogenous cost advantage, for example due to regulatory barriers shielding them from competition, have less incentives to innovate (or reduce managerial slack). Parente and Prescott (1999) and Aghion et al. (2009) apply this mechanism to an "escape entry effect", whereby the threat of potential entrants augments incumbents' incentives to innovate. Hellwig and Irmen (2001) further show that the prospects of profits from inframarginal rents, rather than oligopoly rents, are sufficient for competition to promote productivity.

Does the empirical evidence support these predictions? Syverson (2011) points out that empirical research on the link between competition and productivity has been relatively scant, except for reforms related to trade liberalization. One reason is the lack of adequate data. Several cross-country studies look at the effect of regulation on growth through channels such as changes in mark-ups, entry, exit, or turnover rates (Cole et al., 2005; Barseghyan, 2008). As highlighted by Durlauf et al. (2009), Hauk and Wacziarg (2009), and others, cross-country growth regressions are, however, a limited tool to draw inference due to the difficulty to measure differences in competition across countries with different unobservable institutional environments. Moreover, the small low-frequency sample sizes relative to the seemingly open-ended list of growth correlates make it quasi impossible to obtain robust results or address the problem of endogeneity.

At the microeconomic level, the empirical relation between competition and productivity is also difficult to identify either due to the lack of appropriate disaggregated measures of product-market regulation or due to the lack of a valid control group (of firms) not affected by competition reforms. Several contributions thus focus on case studies for a single homogenous good for which detailed information on prices and market structure characteristics are available. Syverson (2004) studies the market for ready-mix concrete where competition is highly localized due to high transportation costs so that regions with different densities generate an exogenous variation in the number of competitors. Collard-Wexler and De Loecker (2015) exploit changes in market structures due to the introduction of a new technology for producing steel to analyze the impact on firm productivity. Bloom et al. (2015) use variations in government interventions in the health sector to study the impact of competition on hospital management quality.

These studies find that competition accelerates the reallocation of resources to more productive firms or encourages the adoption of more efficient technologies or practices. More

recently, Backus (2020) analyzes the ready-mix concrete industry to differentiate between these different channels. He develops a model for establishments' exit decision to distinguish between the impact of competition on productivity through resource reallocation or reducing managerial slack and finds that the latter dominates.

We differ from these studies in focusing on the elimination of local entry barriers with rich geographical variation rather than an individual sector with appealing empirical characteristics to overcome the limitations of aggregate country or sector data. This allows providing more generalizable firm-level evidence based on a wider range of economic activities and affected firms—the elimination of local entry barriers affected 13 large 2-digit sectors such as retail trade, transport, and food, one-fourth of all municipalities, and 16 percent of all formal firms in Peru.

Several contributions analyze the impact of regional- or industry-specific regulatory changes on firm or industry productivity. Buccirossi et al. (2013), for example, use product market regulation indexes for 22 industries across 12 OECD countries and measure a positive effect on productivity growth. Aghion et al. (2005) use the delicensing across Indian states to show that the threat of entry encourages firms to innovate (see also Sakakibara and Porter, 2001, Ospina and Schiffbauer, 2010). Measures of regulatory changes at the sector-level or across larger subnational regions, however, may suffer from selection effects since policy makers may favor specific sectors or regions with higher productivity growth prospects.

Several recent papers rely on exogenous sector-specific regulatory changes to overcome selection effects. Aghion et al. (2009) exploit changes in sector regulations from reforms driven by the European Union (EU) Single Market Program to show that the threat of entry encourages firm-level innovation in U.K. sectors close to the technology frontier but discourages it in laggard sectors. Bartelsman et al. (2013) develop a stylized model in which firms face adjustment frictions

and idiosyncratic regulatory distortions. They calibrate model variations in market distortions to match observed moments from firm-level data such as the firm size-productivity covariance and find that differences in regulatory distortions can lead to substantial differences in aggregate productivity. Bloom et al. (2016) and Iacovone et al. (2015) exploit the entry of China in the WTO and the subsequent rise of competition from China to estimate that manufacturing firms in EU countries and Mexico are more likely to innovate when they sell products directly competing with Chinese exports. We contribute to this literature by estimating the productivity impact of a legal reform that led to exogenous changes in local entry barriers across many sectors and municipalities within the controlled institutional environment of a single country.

This paper is also related to a literature studying the impact of subnational regulatory barriers on prices. Starting with Benham (1971), who finds that prices of eyeglasses are higher in U.S. states that prohibit advertising by optometrists, a series of studies find that removing advertisement restrictions across U.S. states has led to a decline in prices for consumers while the impact on product quality is ambiguous (Kwoka, 1984; Milyo, J. Waldfogel, 1999; Bagwell et al., 2007). Similarly, studies find that the removal of restrictions to provide professional services tends to reduce prices. Wing and Marier (2014) and Kleiner et al. (2016), for example, find that reducing occupational licensing restrictions for medical services lowers prices without affecting quality. A literature survey by Pagliero (2019) concludes that removing occupational licenses tends to reduce prices while the impact on service quality is difficult to measure and more ambiguous.

At first glance, the findings may contrast with our result that removing local entry barriers in Peru does not change markups which is consistent with no price effects. While we do not look directly at advertisement, we argue that removing restrictions to local market access can lead to higher quality (branding) which raises markups, offsetting the negative markups effect from

lowering local rents. Against this backdrop, the ambiguous relation between advertisement and product quality make it less clear if the price effects can be generalized to the removal of local entry barriers such as restrictions to issue operating licenses. We also note that the studies on the impact of occupational licensing on prices focus mostly on medical services which are not included in our data. Apart from the different nature of reforms, the studies focus on price and quality differences across U.S. states. But it is reasonable to assume that product quality is more heterogeneous across a large number of small municipalities in a developing country such as Peru. The higher technical efficiency from the new threat of entry is thus more likely to not only spur cost efficiency in Peru, but also local product quality which has been shown to raise markups.

The recent literature documenting how demand factors such as product quality affect measured firm productivity is relevant for this study. Foster, Haltiwanger, and Syverson (2008) use U.S. data with information on producer-level quantities and prices to distinguish physical productivity from revenue productivity which is derived by deflating firms' revenues with industry-level prices. They show that higher product quality leads to a positive correlation between firms' revenue productivity and markups, driving a wedge between physical and revenue productivity. Markups thus arise from market power either due to rents or from higher product quality, e.g., through branding, which has been documented to be ubiquitous (De Loecker and Goldberg, 2014). The impact of eliminating local entry barriers on markups is thus ambiguous since it not only reduces local rents but is also expected to raise local product quality. Against this background, De Loecker and Warzynski (2012) apply the control function approach of Akerberg (2015) to develop an integrated framework to estimate firm (revenue) TFP and markups. We follow their approach to estimate how firms' (revenue) TFP and markups are impacted by the local reforms, making it possible to infer the impact on physical productivity.

### **3. Empirical identification**

Peru has implemented a comprehensive competition policy framework at the national level in the past 20 years and has one of the most liberal trade regimes in the world. But many instances of barriers to market entry and competition have emerged at the local level (World Bank, 2017). The legal framework gives local governments the authority and discretion to regulate local market entry as Peru decentralized the regulation and enforcement of issuing operating licenses, permits, defining local technical standards, and conducting inspections. As a result, each of the more than 1,800 municipalities issues its own specific code of business regulations, the *Texto Unico de Procedimientos Administrativos* (TUPA). In principle, procedures in the TUPAs should be consistent with national legislation but in practice this has been the exception rather than the rule.

The decentralized system effectively allowed local public officials to limit market access to protect local incumbents. Several municipalities outright refused to issue operating licenses to new firms, protecting local incumbents. The municipality of Chilca, for example, refused to receive the applications of selected firms for the construction permit of a new building without providing a reason. The practice was declared illegal and removed in 2014.

Against this background, Peru put in place a worldwide unique ex-post control mechanism governed by the competition authority, Indecopi, to investigate, eliminate, and sanction illegal or irrational barriers to market access and competition imposed by local governments through its independent decision body CEB. But the CEB lacked an adequate legal mandate: it could only declare bureaucratic barriers as illegal upon an individual firm's complaint and its decision would apply only to the complaining firm, leaving the barrier still binding to all other firms for the same local market. Moreover, firms could not refer to previous rulings and had to submit their own complaint to the CEB to enforce national law.

In mid-2013, Peru enacted a new legal framework to change the policy design to boost Indecopi's enforcement power, leading to the effective elimination of illegal or irrational barriers to entry imposed by local governments. First, the new legal framework directly listed several entry barriers as illegal that were commonly imposed by municipalities, such as additional local procedures resulting in the withholding of business licenses. The CEB could then directly start a sanctioning process against a municipality once it verified that it kept restricting market entry by imposing any of these listed illegal barriers. Second, the CEB could itself start ex-officio investigations against municipality regulations and publicly declare a barrier to be illegal or irrational. After publishing the decision, any firm could bring a case against a municipality not complying with the published CEB's decision, triggering a fast-track sanctioning process. Third, fines were increased by 400 percent through a bylaw in 2014, including against the individual local major and public official imposing an illegal or irrational bureaucratic barrier.<sup>1</sup>

This new framework had several important effects. There were few yearly individual case rulings by the CEB before 2013 while after the reform the number of rulings against entry barriers in municipalities increased dramatically across the country. Likewise, municipalities were forced to effectively remove barriers to entry through sanctioning procedures or pushed to preemptively eliminate illegal or irrational barriers to escape the threat of large sanctions that could have substantial economic consequences for local public officials. As a result, the number of decisions on regulatory barriers to competition surged by more than 3-fold from 2011/12 to 2013/14 (recall that before 2013 decisions would also still leave the local barrier in place for all other firms).

---

<sup>1</sup> For more details see Law No. 30230 in 2013 or Coronado et al. (2014). The web appendix for this article provides a detailed description of the chronology of competition policy reforms in Peru.

The change of the law in 2013 declaring all regulatory local entry barriers of a certain type as illegal and allowing Indecopi to investigate and publicly declare as illegal additional regulatory entry barriers, was exogenous to the conditions of individual municipalities, creating a quasi-experimental setting to analyze the effects of removing barriers to entry at the local level. The resulting illegal regulations were often removed through sanctioning procedures by Indecopi and in several cases also preemptively removed by municipalities to avoid sanctioning.<sup>2</sup> Notably, municipalities had strong incentives to preventively remove regulation declared as illegal to escape the large new fines of up to the equivalent of US\$27,500 that were now directly charged upon individual majors and local public officials. Consistent with the initiated public sanctioning procedures and the credible threat of enforcing the higher new fines, once a sector-specific entry barriers was eliminated in one municipality, it was in almost all cases removed shortly after also in the other affected municipalities.

Given that the legal change affected sectors in some municipalities but not others, our key identification assumption is that illegal entry barriers were not systematically eliminated in those sector-municipality pairs that had a higher productivity growth potential after 2013 for reasons other than the removal of the illegal barriers. We argue that the cases in which our identification assumption would be violated are unlikely and inconsistent with the empirical evidence.

First, one could argue that the CEB strategically targeted sector-municipality pairs that had a higher productivity growth potential. As mentioned, however, the CEB operated as an independent decision body consisting of a small technical team of lawyers shielded from political

---

<sup>2</sup> Indecopi (2014) evaluates the first 252 cases of eliminated local barriers in 2014 which shows that the higher sanctions under the new legislation had been imposed in the majority of cases.

influence by law and in practice. The process and reasons by which it initiated ex-officio investigations was not related to regional policy considerations, in particular not to sector-specific growth policies of a few of Peru's more than 1,800 municipalities. The large number of sector-municipality combinations would have made such a strategy also difficult to implement. Moreover, our results are inconsistent with the argument that reform sector-municipalities had a higher productivity growth potential for reasons other than the elimination of the illegal entry barriers: treated and control reform sector-municipalities had similar pre-reform productivity trends (Sections 6.3 and 6.4) and similar productivity levels (Sections 6.2 and 6.5).

Second, the CEB may still have picked specific sector-municipality pairs such as the worst abusers 'red tape' municipalities or cases with high demonstration potential. For our results to be biased upwards, however, one would also need to argue that such 'red tape' municipalities had a higher productivity growth potential in the reform sector for reasons other than the eliminated illegal regulation, which seems implausible.<sup>3</sup> Moreover, our finding that reform sector-municipalities did not have lower TFP levels and had similar pre-reform productivity trends attenuates this concern.<sup>4</sup>

Third, the deregulating municipalities may have implemented other simultaneous policies, unrelated to the elimination of the regulatory entry barriers, that boosted firm productivity such as

---

<sup>3</sup> If they had a lower productivity growth potential it would bias the results against our finding that reform municipality-sectors experienced high productivity growth after the legal reform in 2013, in which case our results are lower-bound estimates.

<sup>4</sup> The 2013 legal shock may also have happened to spuriously coincide with reform-sector specific productivity shocks in treated but not in untreated municipalities, but this is entirely implausible.

investments in infrastructure or institutions. But this systematic overlap with the timing of the national legal reform seems implausible. Moreover, we find significant post-reform productivity growth differences between firms located in the same municipality but in reform versus non-reform sectors (Section 6.2), implying that the internal municipal political equilibrium would have needed to shift simultaneously towards other productivity-enhancing, sector-specific policies precisely in the sectors that were subject to the deregulation.

Fourth, the municipalities that preemptively deregulated may have also been more likely to sought to exploit their underlying greater potential for productivity growth in the specific reform sectors. But these municipalities could have deregulated their sectors with high local growth potential at any time and it is unclear why they would have chosen precisely 2013-14. Moreover, such pre-reform differences in municipalities' growth potential are again inconsistent with our empirical results. Instead, we find that reform sector-municipalities had similar pre-reform TFP trends (Sections 6.3 and 6.4) and TFP levels (Sections 6.2 and 6.5) which suggest similar productivity growth prospects for the treated and the control firms operating in the same sector and province but in non-reform municipalities.

Fifth, the municipalities that preemptively deregulated to avoid the large, personalized fines may have had higher expected reform effects. Despite the high fines, municipal violators surely varied in their beliefs about when Indecopi would turn its attention to them. For example, the municipalities with the largest markets and highest inefficiencies in a specific reform-sector would be more vulnerable to Indecopi's actions and thus more likely to reform first. We highlight, however, that this scenario does not violate our empirical identification assumption as long as these municipalities did not have a higher productivity growth potential after 2013 for reasons other than the elimination of the high-impact illegal entry barriers. As pointed out above, the latter is

inconsistent with the empirical results. While the sequencing does not violate the identification, it may imply that our estimates are upper bounds in the sense that eliminations of entry barriers in smaller local markets in later years are expected to have smaller TFP effects. While this sequencing is plausible, we note that once a sector-specific entry barrier was eliminated in one municipality, it was typically closely surveilled by Indecopi in the rest of the country and in most cases was enforced or preemptively removed also in the other affected municipalities. Moreover, we do not find support for different pre-reform TFP or markup trends between treated and control firms which would indicate greater inefficiencies or larger markets in reform sector-municipalities.

Consequently, the 2013 reform has two critical key features for a successful empirical identification of the causal effect of the removal of barriers to local market entry on firm performances. First, the reform introduced for the first time in Peru a legal mechanism that allowed for an effective elimination of regulatory entry barriers across all firms in a municipality and sector. Second, the selection of regulation that triggered the effective elimination of entry barriers is likely to be independent of local market characteristics, economic sectors, and other institutional changes at the municipality level at the time.

## **4. Data**

### **4.1 Firm census data**

The analysis is based on firm-level data from the Annual Economic Survey (EEA) collected by Peru's National Institute of Statistics (INEI) for the 2007-17 period. The EEA draws its sample from a directory of formal firms, based on administrative tax records. The survey stratifies the sampling frame by economic activity and firm size proxied by annual sales to ensure that it is representative of firms in all regions in agriculture, manufacturing, utilities, construction,

trade, transport, communication, and other services. For each stratum and year, firms with annual sales above 1,700 tax units (US\$2 million) are surveyed each year (the threshold is adjusted each year for inflation). This allows to construct a firm panel which is representative at the sector and region level and accounts for 90 percent of total annual sales in Peru.

The data include information related to firms' location, sector classification (ISIC Rev4), age, sales, value added, material input costs, number of employees, total labor costs, and book values of tangible fixed assets. This detailed information allows to compute firm TFP and markups (Table 1). While the vast majority of firms in Peru are single-establishment firms, INEI also provides information at the establishment level for a subset of these variables for three economic activities which are more likely to include multi-establishment firms: agriculture, manufacturing, and hotels and restaurants. But only one of the 13 two-digit sectors with subnational reforms in the Indecopi data, namely food processing, is among these activities (Table 2). That is, we do not observe eliminated barriers in other manufacturing sectors, agriculture, hotels and restaurants, and financial services (INEI does also not provide panel information for the latter three sectors). Since we only compare firms within the same 2-digit sector in our baseline specification, our results do not depend on these sectors. We thus argue that the potential presence of multi-establishment firms is not critical for the purpose of our study.<sup>5</sup>

---

<sup>5</sup> While multi-establishment firms should be the exception, their presence would imply a bias against our results. If a treated multi-establishment firm has a subsidiary not located in a reform municipality, we underestimate the treatment effect since only part of the firms' accounting data are affected by the reform. If a multi-establishment control firm has a subsidiary in a reform municipality, we again underestimate the effect as the subsidiary's data is part of the control group.

*[Table 1 here]*

The data do not provide output quantities (i.e., prices) for individual firms. We thus deflate all monetary production function variables using constant price indices at the most disaggregated level available (2-digit). Foster, Haltiwanger, and Syverson (2008) refer to the resulting measure as revenue TFP. Revenue productivity can increase because of increases in the unobserved physical productivity or increases in firm-specific prices from higher markups.

Table 1 shows that the estimated markup for Peru based on the translog specification in Table 1 (i.e., 1.61 or 0.476 in logs) is very close to the average markup of 1.64 reported for Peru in 2016 by de Loecker and Warzynski (2018, Table 1 and Figure A.4). Table A.2 in the web appendix for this article reports average TFP and markups for broad economic sectors, showing that variations in the estimates across sectors are consistent with ranges reported in the literature.<sup>6</sup>

#### **4.2 Subnational data on eliminating entry barriers**

We use a novel data set for Peru that captures the elimination of all administrative entry barriers across the large number of municipalities as a response to legislative changes in 2013 and 2014 that strengthened the mandate of the national competition authority, Indecopi, to enforce the elimination of entry barriers that are inconsistent with national legislation. Indecopi published the elimination of 723 subnational regulatory barriers for businesses in states, provinces, and municipalities in 2013 (149 eliminated barriers) and 2014 (593 eliminated barriers).<sup>7</sup> The bulk of

---

<sup>6</sup> The range of the sector estimates is consistent with the range of aggregate markups reported for developing and developed countries in de Loecker and Eeckhout (2018) and Akcigit et al. (2021).

<sup>7</sup> The full list of 2,190 eliminated procedures also includes regulatory changes that apply only to consumers but not to firms. We restrict the analysis to eliminated barriers that impact firms.

the eliminated barriers in 2013/14—553 or 75 percent—were in municipalities. Out of these 553 regulatory barriers eliminated in municipalities,<sup>8</sup> about two-thirds (342) were sector-specific in that they applied to a specific 2-digit economic activity. We use both, the municipality and sector dimensions, to identify the impact of eliminating barriers to local entry on firm performance.

Among the 342 eliminated sector-specific local regulatory barriers, we can distinguish between the impact of three different types of reforms: the removal of illegal licenses requirements to operate in a sector or municipality (133 sector-specific barriers), illegal technical requirements to enter local markets such as sector-specific specifications for firms' transport vehicles or buildings (142), and the removal of illegal fees for administrative procedures (67). Local entry barriers from withholding the issuance of operating licenses and imposing illegal technical requirements were the most common types, while excessive local fees were less common.

We expect that the effects of withholding operating licenses or excessive technical requirements have the strongest impact on firm performance as they directly restrict firm entry. Other than cases of simply refusing the issuance of operating license for a specific activity without providing a reason, municipalities, for example, were able to effectively prevent the entry of potential rivals for local incumbents in the transport sector by arbitrarily imposing requirements for the size of vehicle fleets to obtain a local operating license; in the retail sector, municipalities requested additional sanitary or environmental licenses beyond what is foreseen in the law or they withheld operating licenses based on arbitrary restrictions on the minimum distance to existing retail stores. In contrast, administrative fees are expected to be less effective to deter high-

---

<sup>8</sup> Indecopi did not publish the share of the 553 eliminated regulatory barriers that municipalities removed preemptively to escape the high potential sanctions on local public officials.

productivity entrants given that these additional local charges were typically small (in the range of hundreds of dollars).

### 4.3 Matched firm census-reform data set

We match the data on eliminated subnational barriers in 2013 and 2014 with firm census panel data from 2007-17, using the 6-digit geographic codes, providing the location of firms at the municipality level. Figure A.3 in the web appendix of this article shows the provinces with data for formal firms from INEI (left map). Business activity in Peru is concentrated in the coastal area which is 11 percent of the land mass but accounts for 60 percent of the population and GDP.<sup>9</sup> The provinces without formal firms lack major settlements and are in the Amazon (West) and the high Andean regions (center).<sup>10</sup> The right map of Figure A.3 shows the provinces with formal firm-level data which host municipalities that eliminated regulatory barriers to local market entry. These provinces are concentrated on the coast and in the states (*departamentos*) with the largest populations and business activity. They include provinces in the agglomerations of the major cities such as Lima, Trujillo, Piura, and Tacna on the coast, Huancayo, Cusco, and Arequipa in the Andes, and Iquitos in the Amazon region. Within each of these provinces, some municipalities eliminated local entry barriers while others did not.

The matched firm census-reform data include 69 municipalities that removed entry barriers in at least one of 13 different 2-digit sectors in 2014, and 392 municipalities with data on formal firms that did not. We observe on average 84 (64) non-reform municipalities located in the same state (province) as treated municipalities. Moreover, 819 out of a total of 5,283 firms in the

---

<sup>9</sup> Most remaining GDP is produced in the Andes in mining which was not subject to the reforms.

<sup>10</sup> Many of them lack density as two-thirds of municipalities have fewer than 5,000 inhabitants.

matched panel in 2014 operated in reform sectors and municipalities. Given the detailed geographic and sector definitions, on average, 5.4 firms operated in reform sector-municipalities. In the control group, on average, 10.7 (13.9) firms operated in reform sectors but in non-reform municipalities that are located in the same state (province) as reform municipalities (see Table A.1 in the web appendix for this article). Thus, despite our narrow definition of the control group consisting of firms operating in the same sector and in comparable locations as treated firms (see below), we have a healthy variation in our sample.

Table A.3 in the web appendix for this article further reports the summary statistics for productivity and markups among firms operating in reform municipalities and sectors and all other firms. It shows that firms in reform and non-reform sector-municipalities had comparable TFP and markup levels and growth rates before 2013, consistent with the identification assumption that reforms can be regarded as exogenous to pre-reform sector and municipality characteristics.

## **5. Empirical Specification**

### **5.1 Deriving firms' TFP and markups**

We follow the integrated control function approach of De Loecker and Warzynski (2012) to estimate the unbiased measures for the output elasticities of inputs, allowing to compute (revenue) productivity and markups. The approach is critical for our purpose since it controls for unobserved firm-level productivity shocks which corrects for the potential endogeneity bias in estimating the production function coefficients; it further accounts for heterogeneous technologies and input demands across firms within the same industry, is valid under imperfect competition, and allows to infer the reform impact on physical TFP. In the following, we highlight critical

aspects of the approach. The detailed methodology and results of the production function estimation is presented in the web appendix for this article.

We restrict the analysis to production functions with a scalar Hicks-neutral productivity term and common technology parameters ( $\beta_s$ ) across the set of firms ( $I$ ). This allows to use control function methods such as Akerberg, Caves, and Frazer (2015) to obtain consistent estimates of  $\beta_s$ . We can thus define a firm's translog production function as:

$$y_{ist} = \beta_{ls}l_{ist} + \beta_{ks}k_{ist} + \beta_{lls}l_{ist}^2 + \beta_{kks}k_{ist}^2 + \beta_{lks}l_{ist}k_{ist} + \omega_{ist} + \epsilon_{ist}, \quad (1)$$

where  $y_{ist}$ ,  $k_{ist}$ ,  $l_{ist}$  are the logs of real value added, capital, and labor of firm  $i$  in sector  $s$  and year  $t$ ,  $\omega_{ist}$  is an unobserved contemporaneous productivity shock, and  $\epsilon_{ist}$  the error term. We estimate (1) separately for each sector  $s$  to account for heterogeneity in production technology across economic activities. The translog specification further allows technologies to differ across firms within the same sector in that the output elasticities of inputs are firm specific.

To obtain consistent parameter estimates in (1), we follow the two-stage procedure Akerberg, Caves, and Frazer (2015). In the first stage, we use the control function  $m_{ist} = f(k_{ist}, \omega_{ist}, a_{ist})$ , assuming that a firm's demand for material inputs in  $t$  ( $m_{ist}$ ) is a function of its capital stock in  $t$ , an unobservable contemporaneous firm productivity shock, and other firm-specific attributes ( $a_{ist}$ ) such as a firm's age and its location in a reform municipality and sector (our treatment variable). We can then proxy for the unobserved contemporaneous productivity shock by estimating the inverted control function:  $\omega_{ist} = h(m_{ist}, k_{it}, a_{ist})$  to obtain estimates of  $\epsilon_{ist}$ ,  $\omega_{ist}(\beta)$ , and a firm's expected output  $\hat{\varphi}_{ist}(\beta)$ .

In the second stage, the production function parameters  $\beta$  are identified from the law of motion for productivity, in which current productivity is a function of past productivity and the firm attributes:  $\omega_{ist} = g_t(\omega_{ist-1}, a_{ist}) + \varphi_{ist}$ . By nonparametrically estimating the function  $g_t$ ,

the approach recovers the innovation to a firm’s productivity term given  $\beta$ ,  $\varphi_{ist}(\beta)$ , which is assumed to be uncorrelated with the second order polynomials of lagged employment and the predetermined capital stock. This allows to estimate the production function parameters  $\beta$  using a generalized method of moments (GMM) approach. The procedure results in a reasonable range for the estimated output elasticities of labor and capital; the reform dummy has a significant impact in all specifications. The results of the production function estimations by sectors are reported in Table A.14 in the web appendix.

Plugging the estimated production function parameters into the expression for a firm’s productivity  $\omega_{ist}(\beta)$  provides an unbiased estimate of firm TFP. De Loecker and Warzynski (2012) further show that a firm’s markup  $\mu_{it}$  can be defined as the price-marginal cost fraction:

$$\mu_{it} = \theta_{it}^X \left( \frac{P_{it}^X X_{it}}{P_{it} Q_{it}} \right)^{-1} \quad (2)$$

where the output elasticity on an input  $X_{it}$  is denoted by  $\theta_{it}^X$ . We obtain a firm’s markup by plugging into (2) the estimated unbiased output elasticity for the labor input ( $\theta_{it}^L$ ) and the share of expenditures of total labor cost in total output ( $P_{it} Q_{it}$ ). When computing firm TFP and markups, we apply the authors’ correction for unobserved variation in firm-level prices that are uncorrelated with firms’ input choices and are thus captured by the error term  $\epsilon_{ist}$ , by replacing firms’ physical output with the estimated residual from the first stage of the procedure.

## 5.2 Empirical strategy to identify subnational reforms impacts

The matched subnational reform and firm census panel data allow us to estimate the effects of eliminating subnational barriers to market entry on firms’ productivity in a difference-in-differences setting. We thus define a reform dummy variable which is equal to 1 in 2013 or 2014 and the years after if a firm is located in a reform municipality and sector (treatment group), and

zero otherwise (control group). The census data are available from 2007-17 so that we can track productivity and markups of firms impacted by one of the subnational reforms for up to 5 years before and 3 years after the changes in the national legislation strengthening the mandate of Peru’s competition authority over municipalities.

We use the joint sector-municipality-year changes to identify the impact of competition on firm productivity. The treatment group thus consists only of firms that are located in a reform-municipality and operate in a reform-sector. We restrict the comparison to control firms that operate in the same 2-digit sector and in the same province, but in reform- versus non-reform municipalities using the following difference-in-differences specification:

$$Y_{impst} = \beta_0 + \beta_1 R_{mpst} + X_{impst} + F_i + F_p * F_t + F_s * F_t + \varepsilon_{impst} \quad (3)$$

where  $i$  stands for a firm,  $m$  for the municipality in which the firm is located,  $p$  for its province,  $s$  stands for its 2-digit sector, and  $t$  for the year.  $Y_{impst}$  measures TFP or markups as defined above.  $R_{mpst}$  is the reform indicator; it is a dummy variable which is equal to 1 in the years of and after a reform for all firms located in a municipality and 2-digit sector for which at least one regulatory barrier has been eliminated, and 0 otherwise.  $X_{impst}$  includes a firm’s age and  $F_i$ ,  $F_p$ ,  $F_s$ , and  $F_t$  are vectors controlling for firm, province, 2-digit sector, and year fixed effects, and  $\varepsilon$  is the error term. The standard errors are clustered at the sector-municipality level.<sup>11</sup>

---

<sup>11</sup> We thus cluster the standard errors at the treatment level reflecting that the eliminated entry barriers are sector-municipality specific. This allows for correlation in the standard errors across firms operating in the same sector or municipality. It also allows standard errors to be serially correlated given that the reforms remain in place until the end of the sample period.

The firm fixed effects control for unobserved differences across firms that influence their performance.<sup>12</sup> The province-year fixed effects ( $F_p * F_t$ ) control for all fixed and time-varying country and province-specific factors such as regional infrastructure investments, regional policy changes, and macroeconomic trends. The sector-year fixed effects ( $F_s * F_t$ ) control for all additional fixed and time-varying sector-specific factors such as regulation or infrastructure investments. The estimation thus removes any potential TFP or markup effects of policy changes that affects all firms within the same province and sector. Given that Peru's provincial governments often drive local infrastructure projects, this addresses the potential endogeneity concern that the elimination of local regulatory entry barriers may be correlated with contemporaneous local infrastructure investments which may also affect the productivity of firms in these provinces.  $\beta_1$  hence measures if firms operating in the same sector and province but in reform versus non-reform municipalities experienced higher subsequent TFP or markup growth.

Specification (3) is restrictive in that it provides an additional insurance against reversed causality due to selection effects beyond our identification strategy. That is, even if Peru's national competition authority's investigations of entry barriers were guided by regional development and sector-specific motives—which in Section 3 we argue was not the case—this would not affect our estimate of the reform impact ( $\beta_1$ ). Even if Indecopi intended to promote a specific sector in a specific province, this effect would have been absorbed by the province-year and sector-year dummies.<sup>13</sup> For selection issues to arise, one would need to argue that Indecopi had the intention

---

<sup>12</sup> The firm fixed effects also account for other fixed municipality or sector attributes.

<sup>13</sup> Large cities such as Lima, Trujillo, and Arequipa are provinces consisting of several self-governed municipalities.

to target and the capacity to identify specific municipalities within some of Peru's 196 provinces that have imposed entry barriers for specific sectors for which they had a higher growth potential. Consistent with the validity of this quasi-experimental setting, we cannot reject that treatment and control firms followed parallel trends before the reform (see Section 6.3).

As an additional robustness test, we further refine the control group by combining the sector-location-year difference-in-differences estimator with a propensity score matching (PSM) approach. This allows us to restrict the comparison of the treated firms to control firms that not only operate in the same sector and a comparable location, but also had comparable productivity (markups) levels and other comparable firm characteristics in the pre-reform years. For our purpose, we need to adjust the standard PSM approach to fit the clustered structure of our data in that we need an exact match for the year and firms' 2-digit sector to compare TFP performances in the same time period and to control firms operating in the same sector. The estimation approach is described in detail in the web appendix to this article.

## **6. Results**

### **6.1 Impact of eliminating sector-specific local entry barriers**

We use only the joint sector-municipality-year changes to identify the impact of competition on firm productivity, following the difference-in-differences specification in equation (3). The treatment group consist of firms located in reform-municipalities and reform-sectors and is compared to control firms operating in the same sector and in the same province, but in non-reform municipalities. The results are reported in Table 2.

We find that eliminating regulatory barriers to enter local markets for a specific economic activity raises these firms' TFP relative to firms operating in the same 2-digit sector and in the

same province, but in a non-reform municipality (column 1). The TFP effect is statistically significant and of comparable magnitude for the translog and Cobb-Douglas production function specifications (columns 1 and 3). And it is economically significant. Eliminating an illegal entry barrier to local markets has raised the TFP of firms operating in these local markets by 11 percent in post reform years relative to same-sector firms in municipalities that did not change their local market entry regulation (column 1).

Next, we check if the type of regulatory barrier matters for productivity growth. Therefore, we include the three different types of reform dummies in the same regression which allows to compare their relative effects on firm TFP (columns 1 and 4). We find that eliminating sector-specific illegal licenses procedures or technical requirements that allow local officials to restrict the issuance of operating licenses and thus limit the entry of firms into that local market, had the strongest TFP effect (columns 2 and 4).<sup>14</sup> The results suggest that maintaining a credible threat of market entry from potential rivals is critical to foster firm productivity growth.

As expected, the reduction of local administrative fees exceeding the amounts defined in national law did not have a significant impact on TFP growth. The reduction in fees was in almost all cases small, in the range of hundreds of dollars. The additional local fees did thus not present effective barriers to market entry and hence to firms' TFP growth.

---

<sup>14</sup> We also estimate the individual TFP impact of each type of regulation separately by removing municipalities that implemented any of the other two reform types from the control group. We find that eliminating illegal licenses procedures or technical requirements raised TFP in the translog and Cobb-Douglas specifications. The results are presented in Table A.4 in the web appendix.

Notably, we do not find significant effects of eliminating regulatory entry barriers on firms' markups (columns 5-8).<sup>15</sup> The reform dummy is only significant at the 10 percent level in one specification. But the effect is explained entirely by the reduction of the administrative fees. While these do not present an effective market entry barrier affecting TFP growth, our finding suggests that the lower fees tended to raise markups of local incumbents. Since revenue TFP is a positive function of markups and physical TFP, we can infer that the impact of the local competition reforms on revenue TFP stems from a rise in firms' physical TFP rather than increasing markups.

We highlight that the absence of average markup effects does not imply that the reforms did not diminish firms' rent-seeking opportunities. In fact, the elimination of regulatory entry barriers is thought to have reduced the rents of local incumbents in reform municipalities-sectors putting downward pressure on their markups and forcing them to enhance their cost effectiveness which raises TFP. But the elimination of entry barriers has an ambiguous effect on prices and thus markups. Low firm productivity and rents often manifest in lower quality products and services. The elimination of barriers to local market access and the associated (threat of) local market entry is thus expected to raise not only technical efficiency in reform municipalities, but also local product quality which has been shown to raise firms' markups (Foster, Haltiwanger, and Syverson, 2008; De Loecker and Goldberg, 2014). Specifically, local entry barriers protected local incumbents which is expected to have prevented to some degree the availability of national-level quality services and brands in local markets. The higher product quality can itself also be the result

---

<sup>15</sup> The total number of observations is somewhat lower since computing markups requires additional information on compensation of employees which is not available for all firms (Table 1). Using logs further removes the few observations with negative markups in the translog case.

of the rising higher technical efficiency of firms in reform municipalities and sectors. The absence of changes in markups is thus consistent with the two offsetting effects—the reduction in markups from lower local rents is compensated for by higher local product quality.<sup>16</sup>

*[Table 2 here]*

## **6.2 Robustness to different strategies identifying the control group**

First, we alter the strategy to identify the control group by imposing that treated and control firms operate in the same municipality but in reform versus non-reform sectors. We thus exclusively focus on reform-municipalities and estimate if treated firms experienced higher post-reform productivity growth relative to control firms operating in the same municipality but in a sector that was not affected by the reform. Specifically, we change our baseline difference-in-differences estimation in (3) to replace the province-year dummies by municipality-year dummies ( $F_m * F_t$ ) and drop the sector-year dummies ( $F_s * F_t$ ). This alternative specification controls for the effect of any unobserved fixed and time-varying factors including changes in local regulation affecting all firms in a municipality. Identifying the reform impact requires that the municipal reforms can be regarded as exogenous to other contemporaneous changes affecting sectors' local

---

<sup>16</sup> While these effects are established in the literature, firm-product data that would allow to test for product quality effects are not available in Peru. Still, we obtained data on service quality by states for one of the main 2014 reform sectors from the Ministry of Transport and Communication. They show that growth of installed mobile antennas per capita surged by 44 percent from 2013-14 and another 26 and 15 percent in 2015 and 2016, consistent with higher mobile service quality in locations where the installation of new antennas was prohibited before the reform.

growth potential such as sector-specific local public investments (recall that we always control for fixed differences in sector characteristics by including firm fixed effects in all specifications).

We find that eliminating local barriers to market entry led to a significantly larger increase in TFP for reform-sector firms relative to firms operating in the same reform-municipality but in non-reform sectors (Table 3, columns 1 and 3). The TFP effects are statistically significant and of comparable magnitudes as in the baseline specification in Table 2. The results are driven by the elimination of effective local entry barriers, especially illegal licenses procedures (Table 3, columns 2 and 4). As before, the relatively small reduction of local administrative fees did not have a significant impact on firms' TFP growth. We also tend to find limited reform effects on firms' markups—the markup effect is only significant in the translog specification in which the reform dummy of eliminating illegal licensing procedures is significant at the 10 percent level (column 6). Overall, this alternative strategy to identify the control group corroborates the finding of a significant reform impact on firm productivity.

*[Table 3 here]*

Comparing the results in Tables 2 and 3 also allows to make some inference on the importance of sectoral productivity spillovers. While the direct estimation of such sector spillovers is not feasible given the lack of detailed recent 4-digit sector input-output tables in Peru, we can indirectly assess if sector productivity spillovers in reform municipalities are dominant. That is, if the productivity spillovers were complete due to a pass-through of the cost efficiency along the value chain, we should not observe a productivity impact on treated firms relative to control firms that operate in different sectors but in the same reform municipality. Instead, we find significant differences in the impact of eliminating regulatory entry barriers on TFP between reform-sector versus other firms in the same municipality. This suggests that, while positive spillovers to local

firms in different sectors may exist, they are significantly smaller than the productivity gains for firms in the deregulated sectors. We discuss the implications of our findings on the importance of sectoral or geographical productivity spillovers in more detail in the web appendix to this article.

Second, we further refine the control group by combining the difference-in-differences specification for sector-specific reforms with a propensity score matching approach adjusted for the clustered structure of our data as defined in equations (A.4) and (A.5) in the web appendix to this article. The matching approach allows to compare the performance of treated firms not only with control firms in the same sector and with a similar location, but also with similar pre-reform performance indicators that may be correlated with subsequent productivity growth. We therefore include firms' pre-reform productivity and markup levels, their investments, and their size—measured by employment and total fixed assets—as additional variables in the matching procedure given that firm size and investment have been associated with higher firm productivity and may thus signal a higher potential for subsequent productivity growth in post-reform years. The first stage of the matching procedure shows that all pre-reform observable firm performance indicators are correlated with the probability that a firm is operating in a reform municipality and sector. Moreover, the balancing tests reject an unbalanced sample of treatment and control firms.

Corroborating our baseline results in Table 2, we find positive and significant productivity effects for both TFP measures when restricting the comparison to same-sector firms in a similar location and with comparable pre-reform productivity, employment, total fixed assets, and investment levels. As before, the effect is driven by the elimination of illegal practices to restrict access to operating licenses to enter local markets. We do not find a positive significant reform impact on firms' markups. If anything, local firms' markups tended to decline after the elimination

of licensing barriers to entry. The results are reported in Tables A.8 and A.9 and discussed in more detail in the web appendix to this article.

### **6.3 Test of parallel trend assumption**

In the following, we test for parallel pre-reform trends in TFP and markups between firms in reform versus non-reform sector-municipalities. If the elimination of local entry barriers was exogenous to other potential determinants of firm TFP and markups at the municipality-level, firms in the control group should follow the same pre-reform trends as the treated firms. We should thus reject diverging trends in TFP and markups in the data before the reform years 2013/14, implying that the lead effects for the treated firms in our sample should be jointly insignificant. The parallel trend test results for the different estimation specifications are reported in Table 4.

We find that all lead effects of the impact of eliminating regulatory barriers on productivity and markups are jointly insignificant in all estimation specifications. We thus cannot reject the hypothesis that firms in reform-sectors and municipalities had the same productivity and markups trends from 2008-12 than firms operating in the same sector and the same province, but in non-reform municipalities. When analyzing the relative impact of the different types of regulatory reforms, we also cannot reject the parallel trend assumption for the impact of any of the different regulatory types and estimation specifications, apart from the impact of eliminating illegal technical requirements on TFP in the case of the Cobb-Douglas production function. For all other specifications, the leads of the reform dummies are jointly insignificant.

We further test the parallel trend assumption for the alternative strategy to identify the control group by imposing that treated and control firms operate in the same municipality but in reform versus non-reform sectors. We find that all lead effects of the impact of eliminating regulatory barriers on productivity and markups are jointly insignificant in all estimation

specifications and for all regulatory reform types (lower panels of Table 4). Overall, the results suggest that we cannot reject that the treatment and control firms followed parallel productivity and markup trends in the years before the reform.

*[Table 4 here]*

Figure 1 illustrates the estimated difference in TFP between treated and control firms relative to the TFP difference between both groups in 2012, the year before the reform corresponding to the baseline specification in Table 2. It shows only marginal changes in the TFP difference between treated and control firms before 2013—the TFP of treated relative to control firms tends to decline slightly from 2009-12, but the estimated pre-reform coefficients are not statistically different from each other. The relative TFP of treated firms starts to rise in the reform years with a sharp increase in 2014; thereafter it remains at a higher level with further increases in 2016 and 2017. The estimated yearly differences in TFP between treated and control firms relative to 2012 are significant in all post-reform years after 2013.<sup>17</sup> The dynamics are comparable for the alternative strategy to identify control firms in that treated firms and control firms operate in the same municipality but in reform and non-reform sectors (see Figure A.4 in the web appendix).

The results suggests that it takes about one year for the removal of local entry barriers to translate into significantly higher firm productivity levels. The lagged productivity effect is reasonable as it should take some time until firms' initiatives to reduce managerial slack and

---

<sup>17</sup> Figure 1 presents the estimates and confidence intervals of the differences between the estimated means in each post-reform year relative to the estimated means in the last pre-reform year 2012. The estimated treatment effects in 2014, 2016, and 2017 are also significantly different from the treatment and control group difference in 2008.

optimize cost structures as a response to the higher threat of entry of competitors, or actual local market entry, materialize in productivity increases. The potential (additional) productivity impact from innovation may take even longer than the available 3-4 post reform years in our sample.

*[Figure 1 here]*

#### **6.4 Controlling for municipality-specific time trends**

An alternative way to check for the robustness of the difference-in-differences identification assumption is to add municipality-specific time trends to the regressors in (3), allowing treated and control firms to follow different local productivity trends.<sup>18</sup> This makes it possible to test if sector-specific local entry barriers were more likely to be eliminated in municipalities where TFP was increasing anyways. In this case, the TFP effect in reform municipalities would be captured by the municipality-specific time trend and the estimated reform indicator coefficient would not be significantly different from zero.

We find that this is not the case. The baseline results in Table 2 are robust to the inclusion of additional municipality-specific time trends. The increase in firm TFP due to the elimination of sector-municipality entry barriers remains significant at the 1 percent level and declines only slightly from 10.9 to 10.5 percent for the translog specification and from 12.5 to 12.4 percent for the Cobb-Douglas. Consistent with the findings of the parallel trend tests, the results reject the hypothesis that the reform impact may be driven by specific TFP trends in some of the reform municipalities. The detailed results are reported in Table A.7 in the web appendix for this article.

---

<sup>18</sup> See, for example, Besley and Burgess (2004) on including geographical time trends to test for the robustness of the common trend assumption in a difference-in differences setting.

## 6.5 Additional robustness checks

We conduct additional robustness checks to support the argument that the process of eliminating subnational illegal practices that stifle market entry and competition is exogenous to the predicted productivity performance of firms in reform municipalities.

First, we test if municipalities pre-reform average productivity levels predict the elimination of sector-specific local entry barriers in 2013/14. If other time-varying factors at the municipal level explain the subsequent productivity growth in reform-municipalities, we should expect a significant correlation between the probability that a municipality was prompted to eliminate an entry barrier and that municipality's initial TFP level (capturing the effect of other municipal productivity determinants). Table A.10 in the web appendix to this article shows that this is not the case. The probability that a municipality eliminated a sector-specific entry barrier is not correlated with its pre-reform average productivity level.

Second, we find that the results are robust when estimating the impact of eliminating barriers to local market entry separately for 2013 and 2014 (see Table A.13 in the web appendix). Third, we also tend to find significant productivity impacts when estimating the impact of eliminating all subnational barriers to entry independent of their sector (see Tables A.11 and A.12 in the web appendix). This allows to include reforms applying to all local firms raising the total number of municipal reforms by one-third (see Section 4.2). The result implies that either removing the sector-specific barriers, or the subset of barriers applying to all firms, have a strong enough impact to drive the average productivity growth among all firms in reform-municipalities.

## 7. Conclusion

We use a new data set for Peru to estimate the impact of strengthening the mandate of Peru's competition authority to enforce the elimination of illegal barriers to local market entry on firm productivity and markups. We argue that the specific legislative changes empowering the competition authority in 2013/14 to enforce national legislation on subnational jurisdictions provides a quasi-experimental setting allowing to identify the impact of competition on firm productivity within the controlled institutional environment of a single country.

We find that the elimination of subnational barriers to entry, especially through the elimination of illegal procedures in withholding the issuance of local operating licenses, had a strong positive impact on subsequent productivity growth among firms operating in reform municipalities and sectors. We provide a wide range of evidence supporting a causal interpretation of this finding.

The results support the theoretical prediction of models linking barriers to market entry to lower average firm productivities as in Parente and Prescott (1999) and Aghion et al. (2001). They highlight the critical role of subnational barriers to market entry in attenuating competition and reducing firm productivity in developing countries. Subnational entry barriers can thus undermine the productivity and growth impact of seemingly best practice national regulatory frameworks. Our findings further suggest that strengthening the mandate of state institutions enforcing competition is critical to raise productivity.

## References

- Akerberg, Daniel A., Kevin Caves, and Garth Frazer, "Identification Properties of Recent Production Function Estimators," *Econometrica* 83:6 (2015), 2411-2451.
- Aghion, Philippe, Robin Burgess, Stephen Redding, and Fabrizio Zilibotti, "Entry Liberalization and Inequality in Industrial Performance," *Journal of the European Economic Association* 3: 2/3 (2005), 291-302.
- Aghion, Phillipe, Richard Blundell, Rachel Griffith, Peter Howitt, and Sussane Prantl, "The Effects of Entry on Incumbent Innovation and Productivity," *Review of Economics and Statistics* 91:1 (2009), 20–32.
- Aghion, Phillipe, Christopher Harris, Peter Howitt, and John Vickers, "Competition, Imitation and Growth with Step-by-Step Innovation," *The Review of Economic Studies* 68:3 (2001), 467–92.
- Akcigit, Ufuk, Wenjie Chen, Federico Diez, Romain Duval, Philipp Engler, Jiayue Fan, Chiara Maggi, "Rising Corporate Market Power: Emerging Policy Issues," International Monetary Fund Working Papers Series WP-2021-001 (2021).
- Backus, Matthew, "Why is Productivity Correlated with Competition?," *Econometrica* 88:6 (2020), 2415-2444.
- Bagwell, Kyle, "Chapter 28 The Economic Analysis of Advertising," *Handbook of Industrial Organization* 3 (2007), 1701-1844.
- Barseghyan, Levon, "Entry Costs and Cross-Country Differences in Productivity and Output," *Journal of Economic Growth* 13:2 (2008), 145-167.

- Bartelsman, Eric, John Haltiwanger, and Stefano Scarpetta, "Cross-Country Differences in Productivity: The Role of Allocation and Selection," *American Economic Review* 103:1 (2013), 305-334.
- Benham, Lee, "The Effect of Advertising on the Price of Eyeglasses," *The Journal of Law and Economics* 15:2 (1972), 337-352.
- Besley, Timothy, and Robin Burgess, "Can Labor Regulation Hinder Economic Performance? Evidence from India," *The Quarterly Journal of Economics* 119:1 (2004), 91-134.
- Bloom, Nicholas, Mirko Draca, and John Van Reenen, "Trade Induced Technical Change? The Impact of Chinese Imports on Innovation, IT and Productivity," *The Review of Economic Studies* 83:1 (2016), 87-117.
- Bloom, Nicholas, Carol Propper, Stephan Seiler, and John Van Reenen, "The Impact of Competition on Management Quality: Evidence from Public Hospitals," *The Review of Economic Studies* 82:2 (2015), 457-489.
- Buccirosi, Paolo, Lorenzo Ciari, Tomaso Duso, Giancarlo Spagnolo, and Cristiana Vitale, "Competition Policy and Productivity Growth: An Empirical Assessment," *Review of Economics and Statistics* 95:4 (2013), 1324-1336.
- Cole, Harold L., Lee E. Ohanian, Alvaro Riascos and James A. Schmitz Jr., "Latin America in the Rearview Mirror," *Journal of Monetary Economics* 52:1 (2005), 69-107.
- Collard-Wexler, Allan, and Jan De Loecker, "Reallocation and Technology: Evidence from the US Steel Industry," *American Economic Review* 105:1 (2015), 131-171.
- Coronado, Javier, Santiago Davila, Jose A. Tirado, Hebert Tassano, Roxana B. Arellano, Delia A. Farje, Ray A. Meloni and Luis A. Leon, "Propuestas para Mejorar la Competitividad del

- País,” INDECOPI Gerencia de Estudios Económicos Working Papers Series WP-2014-11 (2014).
- Diwan, Ishac, Philip Keefer and Marc Schiffbauer, “Pyramid Capitalism: Political Connections, Regulation, and Firm Productivity in Egypt,” *The Review of International Organizations* 15:1 (2018), 211-246.
- De Loecker, Jan and Pinelopi K. Goldberg, “Firm Performance in a Global Market,” *Annual Review of Economics* 6:1 (2014), 201-227.
- De Loecker, Jan and Frederic Warzynski, “Markups and Firm-Level Export Status,” *American Economic Review* 102:6 (2012), 2437-2471.
- De Loecker, Jan and Jan Eeckhout, “Global Market Power,” NBER Working Papers Series WP-24768 (2018).
- Durlauf, Steven N., Paul A. Johnson, and Jonathan R.W. Temple, "The Methods of Growth Econometrics," In *Palgrave Handbook of Econometrics*, edited by Palgrave Macmillan, 1119-1179, London (2009).
- Foster, Lucia, John Haltiwanger, and Chad Syverson, “Reallocation, Firm Turnover, and Efficiency: Selection on Productivity or Profitability?,” *American Economic Review* 98:1 (2008), 394-425.
- Hauk, William R., and Romain Wacziarg, "A Monte Carlo Study of Growth Regressions," *Journal of Economic Growth* 14 (2009), 103-147.
- Hellwig, Martin, and Andreas Irmen, "Endogenous Technical Change in a Competitive Economy," *Journal of Economic Theory* 101:1 (2001), 1-39.

- Iacovone, Leonardo, Mariana Pereira-Lopez and Marc Schiffbauer, "ICT Use, Competitive Pressures and Firm Performance in Mexico," *The World Bank Economic Review* 30:1 (2017), 109-118.
- INDECOPI, "Anuario de Estadísticas Institucionales 2014," Lima, Peru (2014), <https://indecopi.gob.pe/documents/20182/175196/2014/9bde3a77-27a7-46e1-b869-4e03ab34d76b>
- Kleiner, Morris M., Allison Marier, Kyoung W. Park, and Coady Wing, "Relaxing Occupational Licensing Requirements: Analyzing Wages and Prices for a Medical Service," *The Journal of Law and Economics* 59:2 (2016), 261–291.
- Kwoka, John E. Jr., "Advertising and the Price and Quality of Optometric Services," *American Economic Review* 74:1 (1984), 211-216.
- Milyo, Jeffrey and Joel Waldfogel, "The Effect of Price Advertising on Prices: Evidence in the Wake of 44 Liquormart," *American Economic Review* 89:5 (1999), 1081-1096.
- Ospina, Sandra, and Marc Schiffbauer, "Competition and Firm Productivity: Evidence from Firm-Level Data," International Monetary Fund Working Papers Series WP-2010-067 (2010).
- Pagliari, Mario, "Occupational Licensing in the EU: Protecting Consumers or Limiting Competition?," *Review of Industrial Organization* 55 (2019), 137–153.
- Parente, Stephen L., and Edward C. Prescott, "Monopoly Rights: A Barrier to Riches," *American Economic Review* 89:5 (1999), 1216-1233.
- Sakakibara, Mariko and Michael E. Porter, "Competing at Home to Win Abroad: Evidence from Japanese Industry", *Review of Economics and Statistics* 83:2 (2001), 310-322.
- Syverson, Chad, "Market Structure and Productivity: A Concrete Example," *Journal of Political*

*Economy* 112:6 (2004), 1181-1222.

Syverson, Chad, “What Determines Productivity?,” *Journal of Economic Literature* 49:2 (2011), 326-365.

Wing, Coady and Allison Marier, “Effects of Occupational Regulations on the Cost of Dental Services: Evidence from Dental Insurance Claims,” *Journal of Health Economics* 34 (2014), 131-143.

World Bank , “Peru Systematic Country Diagnostic”, The World Bank, Washington D.C (2017).

**Table 1. Summary statistics firm census data (pooled across 2007-16)**

	<b>Number of observations</b>	<b>Average</b>	<b>Standard Deviation</b>
In Total Factor Productivity Translog	62,877	13.9	1.09
In Total Factor Productivity Cobb-Douglas	62,877	9.23	1.85
In Markups Translog	54,276	.476	1.13
In Markups Cobb-Douglas	62,514	.134	1.11
In Real Value Added	62,877	14.9	1.67
In Employees	62,877	3.75	1.65
In Real Stock of Capital	62,877	14.3	2.25
In Real Intermediate Input Cost	62,877	14.6	2.13
In Real compensation of employees	62,514	13.9	1.90
In Age	62,877	16.7	14.4

*Source:* INEI.

**Table 2. Impact of eliminating sector-specific local entry barriers**

*Control group of firms in the same sector and province as treated firms but in non-reform municipalities*

	ln(TFP-TL)		ln(TFP-CD)		ln(Markups-TL)		ln(Markups-CD)	
Reform-any	.109** (4.01)		.125** (3.33)		.053* (1.76)		.039 (1.25)	
Reform-license	.045 (1.60)		.095** (2.40)		.040 (1.20)		.024 (0.66)	
Reform-technic	.078** (2.18)		.070* (1.70)		.019 (0.82)		-.009 (-0.31)	
Reform-fees	.065 (1.29)		-.029 (-0.33)		.093* (1.94)		.141** (2.03)	
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector*Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province*Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs total	60,053	60,053	60,053	60,053	51,561	51,561	59,681	59,681

*Note:* Based on firm census data from INEI and eliminated entry barriers from Indecopi; reform-dummy is 1 in reform-years and the years after for firms operating in a reform sector and municipality, and 0 otherwise. All regressions control for firm fixed effects, firm age, province\*year, and sector\*year fixed effects as defined in (3). Standard errors are clustered at the sector-municipality level, t-statistics in parenthesis; \*,\*\* significance at 10, 5 percent level. The parallel trend test results corresponding to each regression are reported in Table 6.

**Table 3. Impact on firms in reform-sectors relative to other firms in reform-municipality**  
*Control group of firms in same municipality as treated firms but in non-reform sectors*

	ln(TFP-TL)		ln(TFP-CD)		ln(Markups-TL)		ln(Markups-CD)	
Reform-any	.096**		.212**		.056**		-.003	
	(4.78)		(4.52)		(2.19)		(-0.12)	
Reform-license	.059**		.223**		.046*		-.009	
	(2.77)		(3.85)		(1.67)		(-0.29)	
Reform-technic	.097**		.035		.021		.011	
	(2.82)		(0.49)		(0.58)		(0.25)	
Reform-fees	.019		.017		.071		.041	
	(0.40)		(0.24)		(1.03)		(0.54)	
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality*	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE								
Obs total	59,204	59,204	59,204	59,204	50,801	50,801	58,843	58,843

*Note:* Based on firm census data from INEI and eliminated entry barriers from Indecopi; reform-dummy is 1 in reform-years and the years after for firms operating in a reform sector and municipality, and 0 otherwise. All regressions control for firm (and thus sector) fixed effects, firm age, and municipality\*year fixed effects. Standard errors are clustered at the sector-municipality level, t-statistics in parenthesis; \*,\*\* significance at 10, 5 percent level. The parallel trend test results corresponding to each regression are reported in Table 6.

**Table 4. Test of parallel trend assumption**

*Null hypothesis: treated and control firms have the same productivity and markups in years*

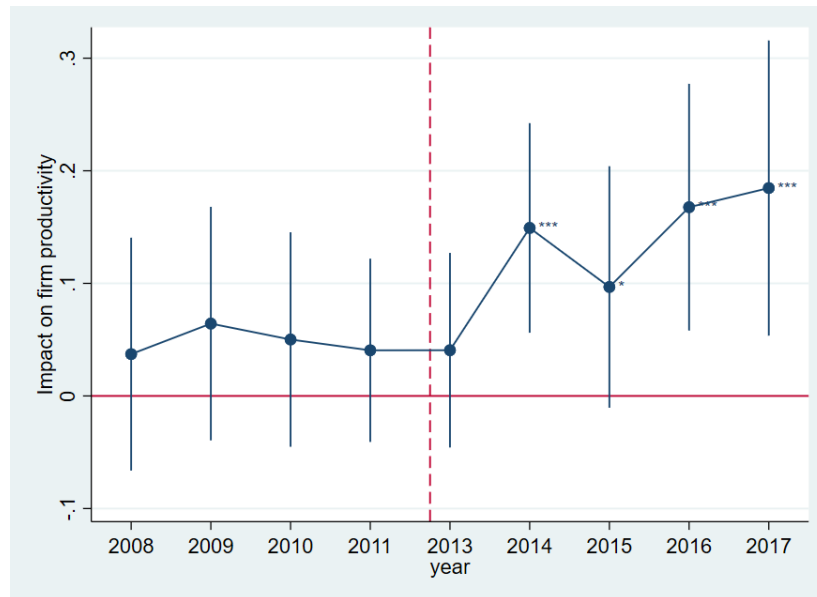
*before the reforms*

	ln(TFP-TL)				ln(TFP-CD)			
	all	license	technic	fees	all	license	technic	Fees
<i>Sector*Year &amp; Province*Year FE:</i>								
<u>2013/14: Prob&gt;F (F2.-F5.=0)</u>	0.476	0.346	0.157	0.632	0.466	0.295	0.045**	0.776
<i>Municipality*Year FE:</i>								
<u>2013/14: Prob&gt;F (F2.-F5.=0)</u>	0.311	0.984	0.133	0.704	0.579	0.889	0.113	0.885
	ln(Markups-TL)				ln(Markups-CD)			
	all	license	technic	fees	all	license	technic	Fees
<i>Sector*Year &amp; Province*Year FE:</i>								
<u>2013/14: Prob&gt;F (F2.-F5.=0)</u>	0.922	0.243	0.460	0.642	0.900	0.346	0.566	0.234
<i>Municipality*Year FE:</i>								
<u>2013/14: Prob&gt;F (F2.-F5.=0)</u>	0.524	0.592	0.372	0.937	0.542	0.426	0.488	0.196

*Note:* Reports the p-values of F-tests if impact of leads of reform dummies in Table 2 and Table 3 are jointly equal to zero for each of the corresponding regressions. Standard errors are clustered at the sector-municipality level, t-statistics in parenthesis; \*,\*\* significance at 10, 5 percent level.

**Figure 1. Impact of eliminating local entry barriers**

*Controlling for Province\*Year & Sector\*Year FEs*



*Note:* The figure shows the estimated reform impact in each year relative to 2012. It corresponds to the estimation specification in Table 2. 95% confidence intervals; \*, \*\*, \*\*\* significance at 10, 5, 1 percent level.