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Are economic rents good for development? Evidence from the manufacturing sector

Daniel Schwab [a], Eric Werker [b,*]

[a] College of the Holy Cross, Worcester, MA 01610, United States
[b] Segal Graduate School, Beedie School of Business at Simon Fraser University, 500 Granville St, Vancouver, British Columbia V6C 1W6, Canada

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ABSTRACT

Are rents, or excess profits, good for development? Rents could induce firms to lobby or bribe governments to preserve the status quo; on the other hand, rents may promote growth by giving firms the needed funds to make investments in fixed capital or research and development. To test this question empirically, we use a panel of manufacturing data at the industry-country-year level, and measure rents by the mark-up ratio. We find that the relationship between rents and growth is strongly negative, with the results being primarily driven by the poorer countries (or those with worse institutions) in the sample. This result holds when we instrument for mark-up using the average mark-up in other industries in the country. Even in industries with high external financing needs and countries with less developed financial sectors, precisely the places where excess profits could be used to drive growth, we find that rents are especially harmful. Consistent with the rent-seeking mechanism we highlight, we find that high rents are associated with a slower reduction in tariffs. We also test for the most likely alternative mechanism, that higher rents cause slower growth through the channel of allowing managerial slack. We find that controlling for management has little impact on our estimate of the impact of mark-up on productivity growth.

> "Without development thereis no profit, without profit no development." Joseph Schumpeter, The Theory of Economic Development (1934)

1. Introduction

Are rents, or excess profits, good for development?

We seek to answer this question by examining panel data at the industry level and applying analytical methods from the competition-and-growth literature (see Aghion & Griffith, 2005) to a larger group of countries along the development spectrum. Economic theory supports both sides of the argument, thereby offering conflicting advice for competition policy and anticorruption efforts. Surprisingly, there has been little statistical research in the last decade and a half since data availability has improved to increase the sample size by two orders of magnitude from earlier studies (e.g. Ades & Di Tella, 1999) and the theoretical debate has become more complex.

On the one hand, rents seem to be a compelling feature of successful economic development. "Schumpeterian rents" (Galunic & Rodan, 1998) can incentivize innovation and thus bring about the economic development Schumpeter was talking about, as the economy becomes more sophisticated and productive. "Without profit," Schumpeter (1934) noted, "there would be no accumulation of wealth."

A different view of rents and development can be found in North, Wallis, and Weingast (2009). North and co-authors argue that most societies in history–including today's developing economies (North, Wallis, Webb, & Weingast, 2007)–are "natural states" in which a dominant coalition of elites carve up the economy into protected rents that can be collectively enforced. As these natural states become more consolidated, elites have an interest to promote specialization and trade in order to increase the amount of rents at play (p. 49). By this mechanism, rents go part and parcel with political stability, and their presence is required if the economy is to develop.[1]

* Corresponding author.
E-mail addresses: dschwab@holycross.edu (D. Schwab), ewerker@sfu.ca (E. Werker).

[1] Introducing an edited volume that applies North et al. (2009) to today's developing countries, North, Wallis, Webb, and Weingast (2012) recognize that some rents might generate a drag on growth while others enable it, but they do not find a pattern across the case studies analyzed (p. 20).

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A third idea can be found in the voluminous access-to-finance literature. Financial sector development is a key correlate of economic development (Levine, 1997). Countries with less developed financial sectors grow slower, all things equal. In those countries, retained earnings are an important source of capital for new investment. It thus seems logical that an economy or industry that enjoys higher profits or rents should be able to fund a faster expansion.

Taken together, these three conceptualizations highlight the crucial role for rents in economic development: as an incentive for innovation, a glue to keep elite interest in stability and expansion, and a source of capital for investment. Yet in spite of this logic there is a case to question the notion that high profits are good for economic development.

The strongest challenge to this notion is the flip side to North et al. (2009). Business interests can capture the state (e.g. Stigler, 1971), or vice versa (e.g. Shleifer & Vishny, 2002). Rents, rather than being used to promote growth, can be used to sustain the status quo, which is often one of limited competition. They can lead to corruption, since bureaucrats who preside over high-rent sectors will be able to extract more from the private sector (Ades & Di Tella, 1999). Rent-seeking activities exhibit increasing returns to scale, thus making rents self-sustaining, and because they are anti-innovation provide a further drag on growth (Murphy, Shleifer, & Vishny, 1993). Rent-seeking can draw talent from the productive sector (Acemoglu, 1995) and be destructive to entrepreneurship in particular (Baland & Francois, 2000).

The other first-order challenge to the view that rents are good for development is the flip side to Schumpeter. Rather than being an incentive for innovation, high profits may be a lack of incentive to do much at all–or, as Hicks (1935) said, "the best of all monopoly profits is a quiet life" (p. 8). If managers are not profit-maximizing and are lazily enjoying the rents from limited competition (e.g. Hart, 1983), then higher rents can lead to slower growth rather than more investment. Only when firms are at risk of losing their business are managers forced to innovate.

The impact of rents may vary depending on a country's wealth, as we illustrate with a simple example. Suppose a group of firms is locked in Cournot competition, and profits can be used for two purposes: socially beneficial research and development (via Schumpeterian creative destruction), and wasteful bribes to prevent new entrants (Stigler's "public choice" view). Credit constraints may limit the firms' spending (see Levine, 1997), but they can retain profits from previous periods. Firms spend on both categories, so factors that make bribery more attractive reduce the resources available for R&D, and vice versa.

There are two relevant differences between developing countries and wealthy countries. First, poor countries have weaker institutions (North & Thomas, 1973), so bribery is more common (Svensson, 2005). Second, financial systems are less well-developed, meaning that it is more difficult to access credit (Djankov, McLiesh, & Shleifer, 2007). The susceptibility of public officials to bribery means that resources will be drawn away from R&D, which suggests that rents are more damaging in poor countries. On the other hand, profits alleviate financial constraints more often in poor countries, so rents may provide the necessary funds for innovation. Thus, the relative impact of rents in poor countries may be more or less harmful, in addition to the ambiguity surrounding the overall impact of rents.

We address the question empirically, using the Lerner index as a measure of rents, following Nickell (1996), Aghion, Howitt, Griffith, Blundell, and Bloom (2005), and Aghion, Braun, and Fedderke (2008). The Lerner index (Lerner, 1934), also called a price-cost margin, is equal to the difference between price and marginal cost divided by price. Under perfect competition, price should equal marginal cost giving a value of zero for the index. The greater the degree of monopoly pricing, the higher the index. In practice, marginal cost data are unavailable for large panel data applications, so mark-up is approximated using a variant of profits over revenues (Domowitz, Hubbard, & Petersen, 1986; Aghion et al., 2005). Since firm-level data in less-developed economies is spotty and unavailable in time series for most countries, we follow Aghion et al. (2008) and use industry-level value-added data from the United Nations Industrial Development Organization (UNIDO, 2013). UNIDO's INDSTAT data are available for 18 manufacturing sectors in over 100 countries between 1964 and 2009 (country coverage is described in Appendix A). The mark-up ratio we calculate is a measure of both rents and (lack of) competition (Clarke & Davies, 1982), and we do not make an attempt to separate these two concepts.

We supplement the UNIDO data on the mark-up ratio with other industry and national-level variables and empirically examine the questions laid out above. In contrast to the simple example of Cournot competition, where the relationship between profits and growth is unclear, our results are decidedly unambiguous. First, we find that observed rents are higher in less developed countries–virtually any indicator of underdevelopment is associated with a higher average mark-up in the manufacturing sectors. Second, we find that the relationship between rents and growth is strongly negative, with the results being primarily driven by the poorer countries (or those with worse institutions) in the sample. This result, that higher excess profits are correlated with slower growth in developing countries, is robust to a series of modifications to the specification including instrumenting for mark-up using the average mark-up in other industries in the country.

We then ask whether how the impact of mark-up changes with financial sector development (as measured by the level of domestic credit to firms relative to GDP) and the degree of external finance required by the industry (taken from Rajan & Zingales, 1998). If access-to-finance constraints are binding, then rents may be especially helpful to finance innovation in sectors that require external finance but in markets with weak financial sector development. In fact, we find that the effect of rents on growth is especially harmful in these situations. In other words, far from being a way to finance investment out of retained earnings, rents seem to be the key to limiting competition.

To be sure, there is potential for endogeneity in our specifications, as evidenced by an old literature seeking to predict mark-up, but most of the potential critiques work against our findings. If better-performing firms also acquire market share, then we should see a positive relationship between mark-up and growth (Demsetz, 1973). Or, similarly, if firms innovate in order to increase profits (Aghion & Howitt, 1992), then there should be a positive relationship between mark-up and growth. Since state-owned enterprises are important in developing countries, and they are on average less profitable and efficient than private firms (Boardman & Vining, 1989; Megginson & Netter, 2001), then we should see a more positive relationship (negative times negative) between mark-up and growth in developing economies. If firms in poor countries over-report costs or under-report profits, we should see less profit rather than more profits in developing economies. If high-growth industries are more profitable, then we should see a positive relationship between mark-up and growth. Some remaining critiques are dealt with by our use of multiple fixed effects specifications and instrumentation strategies.

At the level of the industry, our best measure of protection from "new entrants" is the level of tariffs. We look at the effect of mark-up on the change in tariffs, which of course have been on a secular decline over the period of the sample. As expected, the higher the

Lerner, the slower the reduction in the tariff rate. We also use data from Bloom, Genakos, Sadun, and Van Reenen (2012) to test for the most likely alternative mechanism, that higher rents cause slower growth through the channel of allowing managerial slack. We find that controlling for management has little impact on our estimate of the impact of mark-up on productivity growth, although we lose some of the explanatory power of their data by collapsing from the firm level to the country-sector level.

Our findings are consistent with the earlier political economy literature that finds a destructive effect of rents (Ades & Di Tella, 1999; Baland & Francois, 2000). They are also consistent with the few IO papers that examine the link between competition and growth in developing countries. Carlin, Schaffer, and Seabright (2004) look at firms in transition economies and find that monopolies innovate less than firms facing competition, and Gorodnichenko, Svejnar, and Terrell (2010) find that foreign competition stimulates innovation. The measures of innovation used in these papers roughly correspond to our own understanding of innovation, rather than being inventions per se: new plants, new products, new technologies, or getting quality accreditation. A literature on corporate governance as well as financial sector development explores the political economy factors that influence the development of the business environment and financial markets finds largely similar results to ours with respect to the influence of rents (Morck, Wolfenzon, & Yeung, 2005; Braun & Larrain, 2005).[2] In the development studies field, our results align with Jarvis (2005)'s study of the coffee sector in Brazil, in which efforts to restrict coffee exports were accompanied by more rent-seeking and less growth. Our comprehensive results are probably most similar to Diwan, Keefer, and Schiffbauer (2016)'s study of crony capitalism in Egypt, which was seen by many as a cause of the country's troubles; they find that firms connected to the Mubarak regime benefitted from trade protection and subsidies, and the sectors they were in had less competition and growth.

Not all research has found that rent-seeking is associated with less growth or competition. In Suharto's Indonesia, using plant-level data from the manufacturing sector, bribes were associated with higher productivity growth (Vial & Hanoteau, 2010). In post-Communist countries, more competitive industries were associated with higher bribes (Diaby & Sylwester, 2015). These findings point to the possibility that corruption can be used to "grease the wheels" even in competitive industries and high-productivity firms.

In spite of the broad consistency of our findings with these earlier papers, the paper makes a unique contribution. Unlike the political economy literature, we explore the manufacturing sectors, and in so doing can use industry-level measures and increase the sample size from earlier studies by nearly two orders of magnitude. We undertake a comparative analysis on the level of economic development and also examine mechanisms. Unlike the business literature, the focus of our paper is not on firm profitability but instead on the drag on growth from industry-level profitability, and on reforms. The insight is that what may be good for the players in one industry may not be good for the economy at large. And unlike the IO literature, we focus on the channel of rent seeking, finding that mechanism to be first order in poorer countries.

The rest of the paper is organized as follows. Section 2 describes the data and empirical specifications. Section 3 contains the main results establishing the link between rents and growth at the level of the industry. Section 4 evaluates the mechanisms of rent-seeking and managerial slack. Section 5 concludes.

[2] Braun and Raddatz (2008) is the first paper, to our knowledge, that uses markups in cross-country analysis.

2. Data and empirical specifications

2.1. Rents, competition, and mark-up

We are interested in the relationship between the business environment and productivity growth, and there are two related concepts that we can use to describe the business environment. The first is rents, or excess profits, defined as a fraction:

$$\text{rents} = \frac{\text{revenue} - \text{total cost}}{\text{revenue}}. \tag{1}$$

The second is the competitiveness of the market, which is commonly measured by the Lerner index, or mark-up ratio:

$$\text{Lerner} = \frac{\text{price} - \text{marginal cost}}{\text{price}}. \tag{2}$$

A high value for either rents or Lerner index suggests that a firm or industry enjoys a relatively profitable and uncompetitive market. In practice, we do not observe the data that would be required to measure either rents or the Lerner index exactly, but we can use available data to calculate a good approximation. Following the definition used by Aghion et al. (2008), call the mark-up:

$$\text{mark-up} = \frac{\text{revenue} - \text{total variable cost}}{\text{revenue}}. \tag{3}$$

This measure of mark-up is similar to the Lerner index, but with total variable cost in place of marginal cost. If marginal cost were constant, then mark-up would be equal to the Lerner index. The difference between the measure of mark-up and a theoretical measure of rents is that rents should include the cost of capital, but data limitations restrict our ability to precisely measure the cost of capital.[3] We have access to data on capital expenditures for about three-quarters of the sample, but not to capital stocks or industry-specific depreciation rates and financing costs. We do use sector fixed effects throughout, so to the extent that capital costs as a fraction of output are similar across countries within the same manufacturing sector, we can address this concern. Also, as a robustness check, we estimate capital costs using various proxies for the missing data, as well as the perpetual inventory method to estimate capital stocks, and the main results hold. The variables in the main dataset are value added, which is equal to revenue minus cost of materials, and the wage bill, which is equal to total variable cost minus the cost of materials, so we calculate mark-up as follows:

$$\text{mark-up} = \frac{\text{value added} - \text{wage bill}}{\text{revenue}}. \tag{4}$$

With the data that we have, we cannot distinguish between competitiveness and rents, so we remain agnostic as to which one drives the results and use the two terms interchangeably throughout the paper.

Productivity is defined by

$$\text{productivity} = \frac{\text{value added}}{\text{employees}}, \tag{5}$$

and

$$\text{productivity growth in year } t = \frac{\text{productivity in } t - \text{ productivity in } t-1}{\text{productivity in } t-1} \tag{6}$$

Our reliance on average labor productivity growth has at least three consequences worth mentioning explicitly. One, given that it is defined as the change in value added per worker, rather than

[3] The lack of reliable data on capital stocks prevents us from calculating total factor productivity or to estimate the relationship between mark-up and productivity using the method of Roeger (1995).

the change in the number of units created per worker, it can conflate increases in pricing with increases in quality-adjusted output. On the plus side, this measure will often capture improvements in quality; for example, a change from low-quality coffee beans to gourmet beans will show up as a change in our definition of productivity growth, which a quantity-based measure would miss. This also means that true productivity gains may be statistically indistinguishable from changes in market power: if the producer gains in market power, for example, it can charge more for the same items (Foster, Haltiwanger, & Syverson, 2008).

As Hall (1990) pointed out, when there are increasing returns and market power, measured productivity will appear pro-cyclical. Rising demand will lead to higher margins rather than simply new firm entry. This may be attenuated due to the finding that aggregate labor productivity is less pro-cyclical than within-plant productivity (Baily, Bartelsman, & Haltiwanger, 2001). Even so, the empirical specification allows us to use various combinations of fixed effects to control for most forms of cyclical changes in productivity, which we discuss in the next subsection. Should our measures fail to control for some demand-induced change in productivity growth that is driven by increased market power, it would imply a positive relation between rents and growth, and thus lead us to understate our findings.

Two, given that labor productivity does not include a measure of capital efficiency (which total factor productivity does include), it cannot separate out non-efficiency enhancing increases in mechanization or capital-to-labor from true increases in overall productivity. For example, new restrictions on firing workers might lead to a preference at the margin for plant and equipment over new hires. However, we think the first-order effect here – that firms may divert resources away from capital investment in favor of wasteful rent-seeking – means that most of the time we should see increased capital per worker as a positive outcome for the question that we are researching. Thus we choose change in value added rather than some more complex measure of productivity as a robustness check for our dependent variable; from a developing-country, political-economy perspective it is this more basic measure of industry health that is likely to be influenced by development-retarding rent-seeking.

Three, we do not have the data on hours worked (let alone effort – see Hall (1990)), so this measure cannot capture the effects of changes in shifts or effort per worker, perhaps induced by business cycles; average productivity thus may be imperfectly correlated with some unobservable measure of latent productivity.

2.2. Empirical specifications

In the primary specification of the model, we assume that productivity growth from time $t-1$ to t is a function of mark-up in period $t-1$, log GDP per capita in period $t-1$, and fixed effects for year, country, and sector, with or without an interaction term of mark-up times log GDP per capita. That is, we assume

$$P_{ijt} = \beta_1 M_{ijt-1} + \beta_2 Y_{it-1} + \beta_3 M_{ijt-1} * Y_{it-1} + u_{ijt}, \quad (7)$$

where i indexes countries, j indexes manufacturing sectors, and t indexes time. M refers to mark-up and Y refers to log GDP per capita. We assume that the error term is uncorrelated with the independent variables, so

$$E[M_{ijt}u_{ijt}] = E[G_{it}u_{ijt}] = E[M_{ijt}Y_{it}u_{ijt}] = 0. \quad (8)$$

Value added in time $t-1$ is part of the definition of productivity growth from $t-1$ to t and mark-up in $t-1$, so it appears on both sides of (7), and it is reasonable to be concerned that a mechanical spurious negative correlation between mark-up and productivity growth could arise, as pointed out in Aghion et al. (2008). In our main specification, we instrument for mark-up in period $t-1$ with mark-up in the same country-sector in period $t-2$. This solves the problem as long as the autocorrelation of the error term is sufficiently short-lived; more persistent autocorrelation will be discussed in the robustness section below. We include the interaction of log GDP per capita and mark-up to test whether mark-up has a systematically different effect on productivity growth in poor and rich countries. This is complementary to existing research that has shown that mark-up hampers productivity growth.

Fixed effects for country allow us to control for omitted variables that are constant within a country over time, such as geography. Sector fixed effects control for the possibility that our results are being driven by different compositions of sectors being produced in rich and poor countries, and time fixed effects allow for arbitrary time trends in the data.

We allow for heteroskedasticity in the error term as well as correlation in the error term within a country by clustering standard errors at the country level (we also tried clustering at the country-sector level and found the standard errors were lower, so we cluster at the country level throughout). In other variations we consider different fixed effects specifications and replace productivity growth with growth in value added and log GDP with political stability.

2.3. Data

The INDSTAT2 2013 ISIC Revision 3, published by the Statistics Unit of the United Nations Industrial Development Organization (UNIDO) is our main source for manufacturing data. It covers over 160 countries from 1963 to 2010, categorized according to the 2-digit level of the International Standard Industrial Classification. UNIDO collects the data from a variety of sources, including national publications, published and unpublished international sources, and statisticians employed by UNIDO. Informal manufacturing is often excluded from these sources, so this paper should be regarded as addressing only formal manufacturing.

There are 23 manufacturing categories in the original data, but there are four pairs of categories that are only reported separately starting in the 1980s, and we combine those sectors back together for continuity. We also exclude the recycling sector due to lack of coverage, leaving us with 18 sectors. To give a sense of the fineness of the data, three examples of sectors are textiles, chemicals, and wood products excluding furniture. Manufacturing contributes more to GDP in some countries than others: at the 10th percentile among our observations, it makes up 7 percent of the economy, whereas at the 90th percentile it makes up 29 percent.

The data we use are employees, value added, output, and wages, which are converted to constant (year 2000) US dollar values by multiplying by the US real GDP and dividing by the nominal US GDP, as reported in the World Development Indicators (WDI).[4] We use these data to calculate productivity and mark-up as described above. Some countries are missing data required to calculate mark-up; there are 49 countries with sufficient data to calculate mark-up in 1963, between 74 and 91 from 1970 to 2008, and fewer for 2009 and 2010. Appendix Table A1 lists the countries in the sample and the number of years where they have sufficient data to be included in the preferred specification.

The case of Latvia is instructive to describe the UNIDO data in action. As Latvia conducted dramatic reforms upon its emergence from the Soviet Union in an effort to join the European Union (see Di Tella, Abdelal, & Kindred, 2012), the average mark-up in

[4] It is not immediately clear whether local currency or US dollars are more appropriate, since some goods are traded on the global market while others are consumed domestically. In the body of the text we use US dollars, but the main results hold when using local currency (results available upon request).

its manufacturing sectors fell steadily, from 30% in 1994 to just 13% in 2009. At first productivity growth was high as jobs were slashed, but quickly it fell as the economy struggled to reach a new equilibrium. In the five years from 1995, productivity growth averaged 3% while the mark-up averaged 26%. In the five years from 2002, during which Latvia was one of the fastest growing countries in Europe, productivity growth averaged 19% while the mark-up averaged 19%. In 2008, during the global financial crisis which took a particularly bad toll on Latvia, productivity growth fell to −21% and margins were tight at 14%.

The WDI, published by the World Bank and updated in 2013, is the source for GDP per capita (in 2000 USD), and for domestic credit as a percentage of GDP, which we take as a proxy for the development of the financial sector. Both variables are available from 1963 to 2010.

We are interested in why the impact of rents varies with the income level of a country. One strong possibility is that income level is capturing the strength of institutions, and to test that hypothesis we consider data on institutions from Freedom House, 2017, Polity IV (Marshall & Jaggers, 2017), and the PRS Group, Inc. (2012). For the Freedom House data (which starts in 1972), we use the average of the political rights and civil liberties variables. For the Polity dataset (starting in 1800), we use the Polity 2 variable, which characterizes countries as autocracies, democracies, or somewhere in between. Finally, the International Country Risk Guide starts in 1984, and calculates the political risk rating of countries as a composite of corruption, bureaucratic quality, and other variables. All three of these measures of institutional quality are rescaled from zero to one so that positive numbers are good.

2.4. Summary statistics

In Table 1 we provide summary statistics for some of the most important variables. Mark-up, productivity growth, and value added growth are all Winsorized, meaning that any values outside of the 1st to 99th percentiles are replaced by the 1st or 99th percentile values, to limit the impact of outliers.

The average mark-up is 24.5% in the poorer countries in the sample and 21.2% in the richer countries. Of course, this excludes capital deductions (which would probably amplify the difference) and it accords with the prediction from the discussion that profits are higher in poor countries. Average productivity growth of 5.6% in poor countries is slightly higher than the 4.4% in rich countries, catch-up growth consistent with Rodrik (2013) which finds absolute convergence in manufacturing productivity across countries. An average of 13.9 of the 18 sectors are being actively produced in the poor countries in the year 2000 compared with 16.5 in the rich countries, consistent with Hidalgo and Hausmann (2009) in which richer countries produce more products. Variables capturing institutional quality, wealth, and productivity fall in normal ranges and vary across the two samples as would be expected.

2.5. Correlates of mark-up

One important empirical finding is that mark-up is higher in less developed countries. We generally measure development by income per capita, but it may be better approximated with other measures of political and bureaucratic development. In this subsection we show that virtually any measure of underdevelopment is correlated with a higher mark-up. Our variables are not cherry-picked. This correlation is consistent and robust.

To give a sense of the types of environment where we observe high mark-up, Table 2 presents some correlates of mark-up. All of the variables are coded so that high numbers are better, and in every column, the dependent variable is mark-up (observed at the country-year level). The are no country fixed effects, meaning we are pooling observations across countries; the signs on the coefficients are the same when country fixed effects are included, but not significant (not reported). In the first column, the only independent variables are real log GDP per capita and fixed effects for year, and we see that poor countries tend to have higher mark-up. In the other columns, we control for GDP per capita so that we are not just picking up the income effect on high mark-up. Column 2 examines whether countries that are more likely to have military involvement in politics have higher mark-up; the correlation between military in politics is stronger than that for GDP. Columns 3–6 show that low mark-up is correlated with overall political stability (column 3), good bureaucracy (column 4), a low start-up cost for new businesses (column 5),[5] and low corruption (column 6)–most of which are better predictors of mark-up than income per capita. These findings would not be surprising to investors in emerging markets who assign a larger political risk premium to riskier jurisdictions, effectively increasing the discount rate on the investment and requiring a higher rate of return.

Table 1
Summary statistics.

	Poor		Rich	
	mean	sd	mean	sd
Average mark-up	0.245	0.086	0.212	0.069
Productivity growth	0.056	0.308	0.044	0.195
Poltical stability	−0.128	0.142	0.122	0.129
Polity	−0.142	0.335	0.197	0.277
Freedom House	−0.163	0.258	0.209	0.245
Log GDP per capita	6.77	0.93	9.34	0.75
Log productivity	9.23	1.04	10.47	0.87
Sectors produced in 2000 (out of 18)	13.92	5.53	16.48	1.60

Note: See text for sources.

3. Results

3.1. Primary specification

The results for the primary (IV) specification and two comparison OLS regressions are in Table 3.

The dependent variable in every column is productivity growth from period $t-1$ to t. The first three regressions are IV, with mark-up in period $t-2$ instrumenting for mark-up in period $t-1$, [6] as discussed above, and the fourth and fifth columns are OLS.

In the first model, mark-up is the only independent variable, along with fixed effects for country, sector, and year. In the second, real log GDP per capita is included as a control, to ensure that mark-up is not just a proxy for low GDP. Mark-up is strongly significant and is negatively correlated with productivity growth. The third column is our primary specification, where the interaction term of (log GDP per capita x mark-up) is also included, which is statistically significant and positive. This provides evidence for one of the main claims in the paper: a lack of competition is most harmful in poor countries.

[5] From the Doing Business indicators (Djankov, Porta, de Silanes, & Shleifer, 2002).

[6] The first stage (not reported) is very strong, which is an indication that mark-up is persistent over time, and there is no concern about a weak instrument. The first stage statistic that we consider is the Kleibergen and Paap (2006) F statistic, which is the appropriate first stage statistic in the presence of heteroskedasticity and clustered standard errors (Baum, Schaffer, & Stillman, 2010). If the interaction term is omitted, meaning that mark-up in period $t-1$ is instrumented with mark-up in period $t-2$, the F value is over 2,000. With the interaction term, where the instruments are mark-up in period $t-2$ and mark-up in period $t-2$ x log GDP per capita in $t-1$ and the endogenous variables are the same variables but measured one period forward, the F value is about 35, still well above the rule of thumb of 10.

Table 2
Correlates of Mark-up.

	(1)	(2)	(3)	(4)	(5)	(6)
Log GDP per capita	−0.011	0.001	0.002	0.002	−0.011	−0.004
	(−2.60)	(0.18)	(0.27)	(0.35)	(−1.75)	(−0.73)
Military out of politics		−0.075				
		(−3.74)				
Political stability			−0.198			
			(−2.70)			
Bureaucracy quality				−0.025		
				(−3.20)		
Low start−up cost					−0.237	
					(−1.36)	
Lack of corruption						−0.074
						(−2.70)
Observations	3126	1604	1603	1604	493	1604
R-squared	0.083	0.152	0.118	0.130	0.102	0.100

Note: t-values in parentheses. All variables are coded so that high values are good. There are year fixed effects. The unit of observation is a country-year, and the dependent variable is average mark-up. Standard errors are robust and clustered at the country level. See text for sources.

Table 3
The effect of rents on productivity growth.

	(1) IV	(2) IV	(3) IV	(4) OLS $t-1$	(5) OLS $t-2$
Mark-up	−0.319	−0.322	−0.314	−0.498	−0.240
	(−9.10)	(−9.17)	(−9.60)	(−13.12)	(−10.11)
Log GDP per capita		−0.033	−0.031	−0.034	−0.031
		(−1.44)	(−1.39)	(−1.52)	(−1.35)
Mark-up x log GDP			0.058	0.099	0.037
			(2.71)	(4.00)	(2.53)
Country FE	Yes	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	35031	35031	35031	35031	35031
R-squared	0.115	0.116	0.119	0.124	0.094

Note: t-values in parentheses. Productivity growth is measured from time $t-1$ to t. In the first three columns, we instrument for mark-up in $t-1$ with mark-up in $t-2$. In the fourth and fifth columns, we use OLS with mark-up measured in period $t-1$ or $t-2$. The unit of observation is a country-sector-year. Standard errors are robust and clustered at the country level.

The interaction term here has been demeaned, or defined as (mark-up − mean mark-up) × (log GDP − mean log GDP), implying that the coefficients reported on mark-up and log GDP in the third column can be interpreted as the effect of those respective variables when the other variable is at its mean (Balli & Sørensen, 2013). If we did not demean the interaction term, then the reported coefficient on mark-up would be the effect of a change in mark-up when log GDP was zero, which (thankfully) does not happen often. Notice that the reported coefficients on log GDP and mark-up are similar moving from column (2) to column (3).

The magnitude of the effect is large, but not implausibly so. The standard deviation of mark-up is about 0.12, with a mean of 0.23. Based on the results in the third column, we predict a one standard deviation decrease in mark-up would increase productivity growth in a poor country (25th percentile of wealth) by 4.7 percentage points, and in a rich country (75th percentile of wealth) by 2.9 percentage points. This is substantial compared to the mean of productivity growth, 5.2 percent, but we regard it as believable because the standard deviation is quite high, at 26 percent.

The fourth column presents an OLS regression where mark-up is measured in period $t-1$. The magnitude of the coefficient on mark-up is approximately 50 percent larger than in our primary specification in column 3, an indication that it is important to instrument with lagged mark-up to avoid measurement error problems. The fifth column is also OLS, with mark-up in period $t-2$. Here the results are similar to our primary specification, but the coefficients are somewhat closer to zero, as expected.

To give a visual representation of the relationship between mark-up and productivity growth, we split the full sample into ten bins based on GDP per capita, with equal numbers of observations in each bin. For each bin, we regress productivity growth on mark-up with log GDP per capita and country, year, and sector fixed effects as controls. Each regression generates one point in Fig. 1, where the x-axis is the average value of log GDP per capita in the bin, and the y-axis is the coefficient on mark-up for that bin. We have included a line of best fit, but the question of whether the relationship is linear is of secondary importance. What is most important for our argument is that the relationship is upward sloping: this says that mark-up is most damaging when GDP is low.

3.2. Robustness

Next we present a variety of tests to gauge the robustness of the main results. Additional tests can be found in Appendix B (online).

3.2.1. Instrumenting for mark-up

The primary challenge to our claim is that some unobserved variable is causing both high mark-up and low productivity growth. A perfect instrument would affect productivity growth only through the channel of mark-up and vary exogenously in a way that is orthogonal to any unobserved variables. Finding such an instrument is difficult for a data set that covers so many countries and years, as Aghion et al. (2008) find. They write that the opening of the economy to trade, the degree of tradability of the

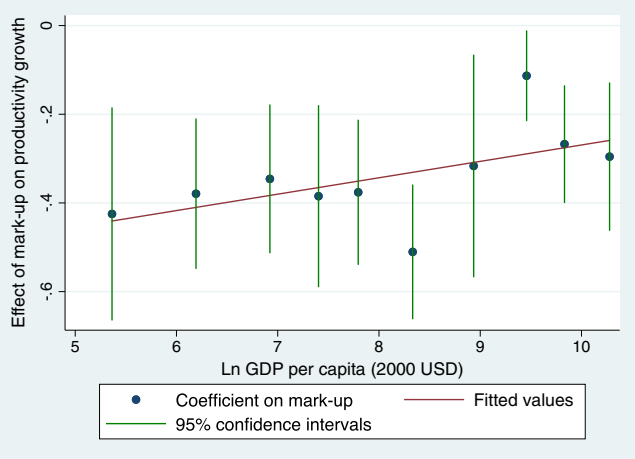

Fig. 1. Marginal effect of mark-up on productivity growth by income. This plot shows the effect of mark-up on productivity growth in 10 different bins of GDP. Please see text for details.

industry, and the level of tariffs all were weak instruments. We can add terms of trade shocks to the list of variables that generate weak first stages. Their most successful instrument is import penetration, which is relevant in one of their two datasets, but not in the UNIDO dataset that their paper and ours both use, and it also suffers from some endogeneity concerns.

We propose another instrument for mark-up, which we acknowledge is also not perfect. It is the simple average of mark-up in the other sectors in the same country-year, a similar strategy to that used in Brander, Du, and Hellmann (2014) which follows Berry, Levinsohn, and Pakes (1995) and others that instrument with local market conditions.[7] Structurally, this instrument is coherent in that the return to capital should be more or less equal across industries within a country-year. The main appeal of this as an instrument is that it allows us to rule out correlated intra-sector measurement error as well as at least some of the possible omitted variables that could be driving our results, namely those that are specific to a particular sector. For example, we can rule out a story where new inventions both cause high productivity growth and allow for entry, driving down mark-ups and generating a negative relationship between mark-up and productivity growth. If that were the case, and the effect was limited to the sector that had the invention, then our instrumentation strategy would yield no relationship because the predicted mark-up would not be affected by the invention. Similarly, our IV results indicate that our results cannot be driven by a productivity shock that is specific to a sector, where a negative shock decreases output and raises prices relative to costs.[8] The instrument has another desirable quality in that any instrumental-variables procedure can only identify the impact of the mark-up that co-varies with the instrument (Angrist, 2004). In this case, the average mark-up in other sectors is likely to capture the mark-up that is driven by the overall business environment, which comes close to the phenomenon that we are trying to uncover in the first place–whether rents for business are good for growth.

Our results with the instrument are presented in Tables 4. The first two columns are the first stage results, with the dependent variables of mark-up and mark-up x log GDP. Mark-up in other sectors is highly predictive of mark-up and mark-up in other sectors x [Table 4 here] log GDP is highly predictive of mark-up x log GDP. The Kleibergen-Paap F statistic is 24.6, above the rule of thumb of 10. The next two are the IV results, with and without an interaction term. In the regression with no interaction term (column 3), the coefficient on mark-up is similar to our main specification that simply instruments with lagged mark-up (−0.37 compared to −0.32). With the interaction term included (column 4), the coefficient on mark-up x log GDP is still significant, but it is much higher than in our main specification (0.34 compared to 0.058). We take this as further evidence that mark-up is most damaging in poor countries, but we should be cautious when making statements about the magnitude of the difference.

3.2.2. Including mark-up as a quadratic term

There is an important strand of the competition-and-growth literature that demonstrates an inverted-U relationship between competition and growth started by Aghion et al. (2005).[9] In Table 5, we include mark-up squared in the main specification to allow for a non-linear relationship. With or without the interaction of log GDP per capita and mark-up, we find a positive and significant coefficient on mark-up squared,[10] but the other coefficients of interest are not greatly impacted. The non-linearity is less dramatic in our paper than in Aghion et al. (2005) who find in a study of U.K. firms that there are large portions of the sample where mark-up is harmful and large portions where it is helpful. In our data, the predicted effect of mark-up is negative for 97 percent of observations, based on column 2 of Table 5; the estimated marginal effect of mark-up on productivity growth at the 25th and 75th percentiles of mark-up are −0.558 and −0.349. The main focus of this paper is how the impact of mark-up depends on poverty, so in the interest of simplicity we do include mark-up only linearly in our regressions outside Table 5.

3.2.3. Cost of capital

Because the mark-up measure that we use does not subtract the cost of capital, we undertake two exercises to estimate the cost of capital and subtract it from the numerator of the mark-up. The first measures the cost of capital using estimates, and the second simply proxies for it by using a five-year average of capital expenditures.

The cost of capital for each observation is the per-unit cost of capital multiplied by the capital stock. While the data do have capital expenditures for many observations, they do not provide local interest rates, depreciation rates, or any measures of capital stock. To estimate the per-unit cost of capital for each industry-country-year, we add to the risk free rate a country risk premium as well as an industry premium. For the risk-free rate we take the U.S. 3-month Treasury bill rate. For the country risk premium and industry we use NYU professor Aswath Damodaran's data.[11] Though we could not find sector-specific rates of depreciation we use the estimate of 5.9% across all manufacturing sectors (Nadiri & Prucha, 1996). After subtracting out the double-counted risk-free rate as well as U.S. inflation, for any given industry-country-year we therefore have an estimate of the per-unit cost of capital. The measure is not perfect by any stretch, but serves to generate a plausible first pass for any observation.

To estimate the capital stock, we use the perpetual inventory measure. However, there are not enough observations–particularly in lower-income countries–to do this with each country-industry.

[7] It would also be reasonable to use the weighted average of mark-up in other sectors in the same country-year, or the average of mark-up in other countries in the same sector-year, but these options give a weak first stage.

[8] We thank Sam Peltzman for suggesting this possibility.

[9] We thank an anonymous referee for suggesting that we investigate this topic

[10] Aghion et al. (2008) use the same dataset with a slightly different empirical approach, and they also find the positive coefficient on mark-up squared

[11] Tables available respectively at http://people.stern.nyu.edu/adamodar/New_Home_Page/datafile/ctryprem.html, "rating-based default spread," and http://people.stern.nyu.edu/adamodar/New_Home_Page/datafile/wacc.html, "cost of capital," with industries matched to the UNIDO codes, accessed June 2016.

Table 4
Instrumenting for mark-up with mark-up in other sectors.

	(1) 1st: mark-up	(2) 1st: inter.	(3) IV	(4) IV
Mark-up			−0.369	−0.519
			(−2.54)	(−3.02)
Mark-up x log GDP				0.335
				(2.96)
Mark-up other sectors	0.662	0.207		
	(16.77)	(2.83)		
Other mark-up x GDP	0.076	0.550		
	(3.40)	(9.56)		
Log GDP per capita	−0.005	−0.008	−0.034	−0.027
	(−0.83)	(−0.66)	(−1.47)	(−1.25)
Observations	35031	35031	35031	35031
R-squared	0.471	0.285	0.118	0.103

Note: t-values in parentheses. The first two columns present the first stage, and the second two present IV results. In the first two columns, the dependent variables are mark-up and mark-up x log GDP respectively, and in the other two it is productivity growth from $t-1$ to t. In the third and fourth columns, simple average of mark-up in the other sectors of the same country ("other mark-up") and other mark-up x log GDP are used as instruments for mark-up and mark-up x log GDP, all in $t-1$. The third and fourth columns should be compared to the second and third columns of Table 3. In every column there are fixed effects for country, year, and sector. The unit of observation is a country-sector-year. Standard errors are robust and clustered at the country level.

Table 5
The effect of rents on productivity growth: including mark-up squared.

	(1)	(2)	(3)
Mark-up	−0.314	−0.785	−0.751
	(−9.60)	(−8.04)	(−7.57)
Mark-up x log GDP	0.058	0.071	
	(2.71)	(3.44)	
Log GDP per capita	−0.031	−0.030	−0.032
	(−1.39)	(−1.36)	(−1.43)
Mark-up squared		0.760	0.691
		(5.72)	(5.35)
Observations	35031	35031	35031
R-squared	0.119	0.128	0.123

Note: t-values in parentheses. Column 1 is identical to Table 3, column 3, the main specification. Columns 2 and 3 include a term for mark-up squared. There are fixed effects for country, year, and sector. The unit of observation is a country-sector-year. Standard errors are robust and clustered at the country level.

Table 6
The effect of rents on productivity growth: cost of capital.

	(1) Cap. stock	(2) 5-year avg. cap. exp.
Mark-up	−0.295	−0.203
	(−9.91)	(−5.29)
Mark-up x log GDP	0.036	0.076
	(2.06)	(3.78)
Log GDP per capita	−0.040	−0.041
	(−1.42)	(−1.70)
Observations	31955	18451
R-squared	0.109	0.119

Note: t-values in parentheses. The dependent variable is productivity growth, and in both columns mark-up is calculated to include the cost of capital. In the first column, we predict capital stocks and then calculate the foregone interest and depreciation. In the second column, we estimate the true cost of capital to be a five-year rolling average of capital expenditures. In both columns there are fixed effects for country, year, and sector. The unit of observation is a country-sector-year. Standard errors are robust and clustered at the country level.

So rather than limiting our sample to those observations where we can calculate a capital stock, we estimate capital stock based on U. S. data. First, we estimate capital stocks in the United States using 20 years of observations. Then we calculate a predicted capital intensity for every industry based on U.S. data, expressed as the ratio of estimated capital stock to revenues (effectively the inverse of an asset turnover ratio). We then multiply this predicted capital stock by the estimated per-unit cost of capital and subtract that from the numerator of our Lerner index, thereby better approaching the correct price–cost margin.

As the first column of Table 6 demonstrates, the effect on the coefficients of interest is not significant. There is some attenuation bias, but it is minimal and both the effect of the mark-up ratio as well as its interaction with GDP per capita remain significant at the 1% and 5% level respectively. The second column uses the more simple average capital expenditure for the previous five years as a proxy for the cost of capital. The sample size is just over half because observations are lost due to the demands imposed by the 5-year lag as well as the fact that not all observations have capital expenditures. That said, the results again hold.

3.3. Credit constraints

The evidence thus far indicates that high rents slow productivity growth, particularly in poor countries, but perhaps we can find characteristics where they are beneficial. We are looking for a case where rents could alleviate credit constraints, which is most likely in a sector where external financing is important and in a country where the financial system is unlikely to provide loans in the absence of rents. Our measure for the external finance required in a sector is from Rajan and Zingales (1998), who use US firm-level data to calculate the amount of external finance they require; due to data constraints, this value is treated as invariant over time. Their argument (and ours) is that dependence on finance by US firms captures technological differences between sectors. Our proxy for financial market development is domestic credit as a fraction of GDP, from the WDI (2013), which varies at the country-year level.

Column 1 of Table 7 gives the results of our primary specification where we also include the interaction of mark-up and the external finance required by a sector. If rents can be useful, we would expect mark-up to be least damaging where external finance is important, but in fact we see the opposite, a strongly significant negative result. Similarly, in column 2, the interaction between domestic credit and mark-up is positive and significant at the 10% level.[12] If rents can alleviate credit constraints, then rents should be least harmful in those countries where credit is difficult to find, which again is the opposite of what we find.

[12] When controlling for convergence as in table 12, the results on the first interaction are unchanged, but the second interaction becomes insignificant ($t \approx 1.4$), so the results regarding domestic credit should be regarded as suggestive rather than conclusive (regression results available upon request).

Table 7
Mark-up and credit constraints.

	(1)	(2)
Mark-up	−0.347	−0.306
	(−8.74)	(−8.93)
Mark-up x external finance	−0.168	
	(−3.93)	
Credit/GDP		−0.043
		(−2.64)
Mark-up x credit/GDP		0.108
		(1.71)
Log GDP per capita	−0.023	−0.007
	(−0.99)	(−0.30)
Observations	33271	33271
R-squared	0.118	0.117

Note: t-values in parentheses. The dependent variable, productivity growth, is measured from time $t-1$ to t. We instrument for mark-up in $t-1$ with mark-up in $t-2$. The unit of observation is a country-sector-year. External finance required is from Rajan and Zingales (1998) and domestic credit/GDP is from WDI (2013). Note that external finance varies at the sector level, and is absorbed by the sector fixed effects, while credit/GDP varies at the country-year level. There are fixed effects for country, year, and sector. Standard errors are robust and clustered at the country level.

We interpret this as evidence that barriers to entry raise the returns for incumbents to keep a sector uncompetitive. This is consistent with the mechanism we propose, where firms divert more resources towards rent-seeking when it is easier to keep entrants out. It may also provide one explanation for the surprising finding (Singh, 1997) that firms in emerging markets were less likely to use retained earnings to finance growth than developed-country firms: when retained earnings are high, they do not need to grow at all.

4. Mechanism

The focus in the previous sections was that anticompetitive practices damage productivity growth. Here we argue that this occurs through the political economy channel. We test through the only potential country-sector variable on rent seeking for which we have data, tariffs, and find that tariffs fall more slowly in sectors with higher rents. Tariffs are not a direct measure of barriers to entry of other domestic firms, but they do affect the cost of imported substitutes.

We also test for an alternate channel broadly consistent with the main specification in the previous section, which is lazy management. That is, firms in high-rent sectors can rest on their laurels. Although our data coverage is limited, we do not find support for the hypothesis that slower growth is due to weaker management. Finally, we show that countries' mark-up is more harmful when institutions are weak, which suggests that institutional quality is the reason that poor countries are more affected by mark-up.

4.1. Tariffs

Using a broad competition and growth framework, Aghion and Howitt, 2007 model the effects of international competition on innovation, which in our paper corresponds to productivity growth. In their model, reducing trade barriers encourages firms to innovate, in the same way that firms with more domestic competition are forced to innovate more (so long as they are close enough to the production frontier that they do not simply give up). Aghion, Fedderke, Howitt, and Viegi (2013) test the predictions of the model using data from South African industries, and find that liberalization increases productivity growth, and that once the level of liberalization in the industry is controlled for, the negative effect of Lerner on productivity growth increases. The authors note that the inference is that sectors not impacted by trade liberalization (or a fall in tariffs in our context) have a larger pricing power impact.

Table 8
Mark-up and change in tariffs.

	(1) OLS	(2) OLS	(3) IV	(4) IV
Mark-up	0.178	0.188	2.774	2.782
	(2.87)	(2.99)	(2.16)	(2.18)
Log GDP per capita	−0.213	−0.213	−0.251	−0.252
	(−0.97)	(−0.97)	(−1.08)	(−1.09)
Level of tariffs		−0.041		−0.079
		(−1.89)		(−1.90)
Observations	5082	5082	5082	5082

Note: t-values in parentheses. The dependent variable is percentage change in tariff levels. Tariff data is from United Nations Conference on Trade and Development (2013). In columns 3 and 4, we instrument for mark-up with the simple average of mark-up in other sectors for the same country and year. There are fixed effects for country, year, and sector, and the unit of observation is a country-sector-year. Standard errors are robust and clustered at the country level.

In our paper, we conduct a different test that is closer to this paper's focus on whether rents are used for rent-seeking or investment. We examine whether in country-industries with higher mark-ups tariffs fall faster or slower. Tariff rates have been falling on average (by approximately 4.4% per year), but they are less likely to fall when firms are earning higher profits. This should be a surprise, since the main argument in favor of tariffs is to protect fledgling industries that need help in getting established. Tariff data come from the TRAINS database, published by the United Nations Conference on Trade and Development (2013), and are collapsed to the two-digit sector level by a weighted average. They cover from 1988 to 2008.

The dependent variable is the percentage change in the tariff rate, so that a change from 100% to 50% is considered the same as the change from 20% to 10%. The most extreme 5% of values on both ends for the change in tariff rate are replaced with the 5th and 95th percentile values to reduce the influence of outliers, and the results of these regressions are in Table 8.

Columns 1 and 2 present OLS results. Based on the results in column 1, we predict that a one standard deviation decrease in mark-up (i.e., a reduction of 0.12, compared to a mean of 0.23), would lead to an annual decrease in the tariff rate by an additional 1.8 percent. Over the 20-year sample for which we have tariff data, that would correspond to a cumulative decrease in tariffs of about 30 percent.

In columns 2 and 4, the level of the tariffs is also included as a control to rule out the possibility that sectors with high profits have low tariffs already, making further reductions unlikely. Adding this control has a very small effect on the other coefficients, so we conclude this is not what is happening.

To address reverse causality, where tariffs impact profitability, we use the same instrumentation strategy as above, and instrument for mark-up with the simple average of mark-up in other sectors in the same country and same year. We note that while tariffs are similar across industries within a country, there is still substantial variation within a country's tariff levels, so the instrument should not be regarded as essentially a country-year variable.[13] These results are displayed in columns 3 and 4. Given the straight-

[13] For example, the mean absolute difference in tariff levels between two observations in the same country-year is 4.7 percentage points, compared to a mean absolute difference of 6.9 percentage points for two observations in the same sector-year.

forward instrumentation strategy, it is reassuring that the coefficients on mark-up are also positive and significant, but the magnitudes are large enough that we should interpret the results as suggestive but not conclusive: that the point estimates increase so much may be an indication that common shocks are a concern. Taken at face value, a one standard deviation decrease in mark-up produces a 43% annual reduction in tariffs, meaning tariffs would rapidly disappear in any sector with low mark-up.

In all four regressions, including an interaction term for GDP and mark-up generates a coefficient that is very small and not close to significant (these results are not reported). We suspect this may be driven by nontariff barriers being the protectionist vehicle of choice in poor countries (Michalopoulos, 1999), or the sample could just be too small.

4.2. Management

Here we consider an alternate mechanism through which rents could depress productivity growth: lazy or satisficing managers, as in Hart (1983). We consider the hypothesis that mark-up is driving the slower productivity growth of profitable firms by controlling for management style and considering the effect on the measured coefficient on mark-up. Bloom et al. (2012) conducted telephone surveys of senior managers of over 10,000 firms in 20 countries between 2002 and 2010, asking open-ended questions and coding them from 1 (worst) to 5. We first collapse their firm-level data to the country-sector level data. The mean number of observations for a country-sector combination with at least one observation is under three, so we ignore temporal variation and further collapse their data to a single management score for each country-sector.

As our dependent variable, we use productivity growth from 2002 to 2009. The independent variable of interest is mark-up in 2002, instrumented with mark-up in 2001, as in our main specification. The results, reported in Table 9, do not give any indication that mark-up is merely a proxy for management style, as the estimated coefficient on mark-up does not change substantially when we include management as a control.

This is not to say that our results call into question the results in Bloom et al. (2012) that management practices have a substantial impact on total factor productivity or that uncompetitive markets foster weak management. Rather, in arguing that mark-up is detrimental beyond its effect through the management channel, our paper should be viewed as complementary.

4.3. Institutions

Our main results indicate that poor countries experience more of the negative impacts of mark-ups than rich countries do. We cannot provide conclusive evidence for why that is the case, but one plausible explanation is that poor countries have weaker institutions. Perhaps a profitable firm can take advantage of weak institutions to use bribes to hold its position, while stronger institutions constrain a profitable firm, causing more of the profits to be redirected back to research and development.

Table 9
Management, mark-up and productivity growth.

	(1)	(2)	(3)
Mark-up	−2.758		−2.799
	(−2.30)		(−2.40)
Management score		0.077	0.099
		(0.99)	(1.50)
Observations	97	97	97
R-squared	0.583	0.485	0.588

Note: t-values in parentheses. Management scores, from 1 (worst) to 5, from Bloom et al. (2012). The dependent variable is productivity growth from 2002 to 2009. Mark-up is measured in 2002 (instrumented with mark-up in 2001). There are fixed effects for country and sector, and the unit of observation is a country-year. Standard errors are robust and clustered at the country level.

Table 10
The effect of rents on growth in productivity growth: alternate measures of institutions

	(1)	(2)	(3)	(4)
Mark-up	−0.314	−0.347	−0.305	−0.343
	(−9.60)	(−10.05)	(−9.33)	(−7.58)
Mark-up x log GDP	0.058			
	(2.71)			
Log GDP per capita	−0.031			
	(−1.39)			
Mark-up x FH		0.034		
		(2.02)		
Freedom House		−0.005		
		(−0.18)		
Mark-up x Polity			0.011	
			(2.80)	
Polity			0.041	
			(2.11)	
Mark-up x political stability				0.004
				(1.34)
Poltical stability				0.006
				(0.10)
Observations	35031	32680	36452	17789
R-squared	0.119	0.123	0.120	0.131

Note: t-values in parentheses. The dependent variable, productivity growth, is measured from time $t-1$ to t. We instrument for mark-up in $t-1$ with mark-up in $t-2$. The unit of observation is a country-sector-year. See text for a description of the data sources. Institutional variables are rescaled to a zero to one range, and high values for all are desirable. There are fixed effects for country, year, and sector. Standard errors are robust and clustered at the country level.

In this section, we consider the effects of using three different measures of institutions in place of log GDP per capita: those from Freedom House, Polity IV, and ICRG discussed above (the variable from ICRG is labeled political stability). Table 10 shows that the interaction terms of Polity times mark-up and Freedom House times mark-up are both positive and significant, while the interaction term of political stability times mark-up is positive but not statistically significant. Taken collectively, these results are therefore consistent with the hypothesis that institutional quality is a relevant factor that determines how impactful mark-up is on productivity growth.

5. Conclusion

This paper has attempted to test the question of whether rents are good for development–brought to the forefront of scholarship and policy by North et al. (2009)–by using a rich dataset on manufacturing sectors and applying the methods from the competition-and-growth literature of Aghion and co-authors. While the results cannot be seen as incontrovertible due to the challenges of endogeneity and the presence of a strong alternative hypothesis (management) that cannot be discounted completely, the evidence all points in one direction.

Rents, as measured by a high-markup which is also an indication of low competition, seem to slow growth in productivity or output. The effect is strongest in poor countries and those with weaker institutions. Higher rents are associated with a slower removal of tariffs, indicative of the channel we propose: firms rent-seek to prevent competition and maintain their high margins. This investment in rent-seeking may be in lieu of investment in innovation or new productive assets, which slows the overall growth of the sector. In industries in which high profits should be essential in generating growth, those sectors that would

otherwise need external finance but in a country with weak financial markets, the negative impact of rents on growth is especially strong. We do not find evidence (although our data availability is limited) to support the alternative hypothesis most consistent with the data, that sectors with higher rents have inefficient managers.

These results should inform the discussion among development practitioners regarding how to engage with rent-seeking behaviour in developing countries. Until recently, the conventional approach was dominated by an anti-corruption focus. As former World Bank governance expert Brian Levy (2014) has argued, rather than lead with this anti-corruption focus, it makes more sense to "work with the grain" for a number of reasons, starting with North et al. (2009)'s finding that elites use rents to maintain political stability and building on Francis Fukuyama, 2014 argument that clientelism is "an early form of democratic accountability." Given the modest setting of this paper, we do not presume to answer this debate. However, the strength and the consistency of the findings do have relevance to practitioners working both for developing-country governments as well as bilateral and multilateral agencies in the fields of private sector development and market regulation.

Developing countries exhibit substantial variation at enforcing competition rules, with less developed countries devoting fewer resources to enforcement (Waked, 2010; Waked, 2011). This paper has found that this lack of competition is associated with slower productivity growth. While we cannot say for sure whether policy-driven efforts to promote competition will lead to higher growth in the same way that the average "natural" drivers of competition seem to, we have no reason to doubt that it might. Moreover, political economy analysis can complement antitrust efforts. The findings point to the potential benefits from promoting competition and paying attention to rents at the level of the industry in lower and middle-income countries.

Declarations of interest

None.

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Appendix A

Table A1
Country coverage

Country	Years	First	Last	Country	Years	First	Last
Albania	9	2001	2009	Denmark	38	1964	2008
Algeria	23	1968	1995	Dominican Republic	20	1964	1983
Argentina	13	1985	2001	Ecuador	44	1964	2007
Armenia	3	2005	2007	Egypt, Arab Rep.	24	1965	1988
Australia	26	1964	1989	El Salvador	4	1994	1997
Austria	40	1964	2008	Eritrea	17	1993	2009
Azerbaijan	8	2002	2009	Estonia	14	1996	2009
Bahamas, The	9	1978	1997	Ethiopia	18	1991	2008
Bangladesh	24	1968	1991	Fiji	29	1971	2008
Barbados	21	1976	1996	Finland	43	1964	2008
Belarus	4	2006	2009	France	28	1978	2008
Belgium	39	1964	2008	Gabon	3	1992	1994
Belize	1	1991	1991	Gambia, The	6	1976	1981
Benin	6	1975	1980	Georgia	4	2006	2009
Bolivia	28	1971	2000	Germany	10	1999	2008
Botswana	18	1982	2009	Greece	8	1993	2006
Brazil	4	1993	2009	Guatemala	15	1973	1987
Bulgaria	15	1992	2009	Hong Kong	21	1981	2009
Burkina Faso	8	1975	1982	Hungary	41	1964	2008
Burundi	12	1972	1990	Iceland	7	1989	1995
Cameroon	10	1971	1983	India	25	1980	2008
Canada	46	1964	2009	Indonesia	35	1971	2008
Central Afr. Rep.	13	1974	1992	Iran, Islamic Rep.	40	1966	2008
Chile	44	1964	2007	Ireland	38	1971	2008
China	8	1981	2006	Israel	36	1964	2002
Colombia	46	1964	2009	Italy	41	1968	2008
Costa Rica	19	1984	2002	Jamaica	19	1967	1991
Cuba	11	1978	1988	Japan	41	1964	2009
Cyprus	34	1976	2009	Jordan	30	1976	2009
Czech Republic	11	1996	2006	Kenya	38	1972	2009
Kuwait	9	1996	2009	Romania	19	1991	2009
Kyrgyz Republic	9	2001	2009	Russian Federation	12	1994	2009
Latvia	16	1994	2009	Senegal	17	1975	2001

(*continued on next page*)

Table A1 (*continued*)

Country	Years	First	Last	Country	Years	First	Last
Lesotho	3	1982	1984	Singapore	22	1964	2007
Lithuania	9	2001	2009	Slovak Republic	13	1994	2006
Luxembourg	20	1986	2008	Slovenia	19	1991	2009
Macao SAR, China	13	1983	1995	South Africa	21	1980	2008
Macedonia, FYR	16	1991	2009	Spain	45	1964	2008
Madagascar	22	1968	2005	Sri Lanka	11	1993	2009
Malawi	33	1968	2008	Suriname	7	1997	2003
Malaysia	38	1969	2009	Swaziland	15	1980	1994
Malta	34	1971	2007	Sweden	45	1964	2008
New Zealand	15	1978	2008	Syrian Arab Rep.	1	1994	1994
Nicaragua	19	1966	1984	Tanzania	10	1995	2009
Niger	3	1999	2001	Thailand	5	1969	1990
Nigeria	22	1964	1995	Trinidad & Tobago	21	1967	2002
Norway	44	1964	2007	Tunisia	26	1964	2001
Oman	16	1994	2009	Turkey	42	1964	2008
Pakistan	27	1964	1990	Ukraine	2	2002	2003
Panama	28	1964	2000	United Kingdom	38	1969	2006
Papua New Guinea	23	1964	1986	United States	39	1964	2007
Philippines	32	1964	1998	Uruguay	11	1977	1987
Poland	10	1991	2008	Venezuela, RB	23	1974	1996
Portugal	9	2000	2008	Vietnam	1	2007	2007
Puerto Rico	10	1990	1999	Yemen, Rep.	4	1999	2005
Qatar	3	2001	2003	Zambia	12	1964	1981

Appendix B. Supplementary data

Supplementary data associated with this article can be found, in the online version, at https://doi.org/10.1016/j.worlddev.2018.07.014.

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