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RENTS AND REGULATIONS IN THE DEVELOPING WORLD

by

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For Hazel, who kept me motivated, and Amanda, who kept me sane.

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ABSTRACT

Regulations may end up harming the very people they are intended to protect, and an unexpected windfall may lead to upheaval rather than prosperity. In three chapters, I discuss how rents and regulations can affect lives and welfare in the developing world.

In the first chapter, I examine employment protection legislation (EPL), which is intended to promote security for workers by placing restrictions on firing. Using India as a setting, I argue that EPL shifts jobs from the young to older workers. The identification strategy is based on Rajan and Zingales (1998), and relies on heterogeneity between manufacturing sectors. The impact of EPL is strongest in those manufacturing sectors where international evidence suggests employers most like to fire workers. Finally, I present suggestive evidence that the shift from young to old employment induced by EPL reduces total factor productivity of plants.

The second chapter, coauthored with Eric Werker, demonstrates how rents slow down productivity growth. The negative effect is strongest in poor countries, suggesting that high profits stymie economic development rather than enable it. Consistent with the rent-seeking mechanism of our theory, we find that high rents

are associated with a slower reduction in tariffs. We also provide evidence that a country's average mark-up in manufacturing is a strong negative predictor of future economic growth.

The third and final chapter, which is based on joint work with Faisal Ahmed and Eric Werker, examines foreign aid and oil rents in the Middle East. Aid from oil-rich autocrats created unearned rents for many developing countries in the 1970s. We provide a theoretical explanation for this phenomenon, whereby autocrats experiencing a windfall in unearned income may find it optimal to donate some of it to other countries in order to make their own state a less attractive prize to potential insurgents.

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Employment Protection and the Labor Informality
of the Youth: Evidence from India

1 Introduction

This paper explores unintended consequences of employment protection laws (EPL), which limit the ability of employers to fire workers. Nearly every country in the world has some form of EPL, which can be a requirement of advance notice, a severance payment, a prohibition on firing, or some combination of these (World Bank 2015). One common argument in favor of these laws is that they reduce income risk in the absence of perfectly functioning insurance markets (Pissarides 2001).

Although there may be benefits, EPL carries the risk of creating distortions in the labor market. For example, a worker may respond to increased job security by reducing her effort level (Ichino and Riphahn 2005). EPL may also introduce more subtle distortions, including a change in the age composition of the labor force (an “age distortion”). The central question of this paper is to ask whether EPL generates an age distortion, and if so, whether there are measurable implications for productivity.

I study these phenomena in India’s manufacturing sector. India is an excellent setting to study EPL because India’s EPL is among the strictest in the world (World Bank 2008), so the distortions it creates are large and important for the Indian economy. In addition, there is considerable variation in state-level EPL, meaning that it is possible to restrict attention to a single country, ensuring that much of the institutional environment is the same across the data.

India’s main labor law is the Industrial Disputes Act (IDA), which requires large plants to obtain government permission before the plants can fire a permanent worker. Contract workers in all plants and permanent workers in small plants (I discuss the size threshold below) can be fired freely. I define workers who are permanent workers in large plants as “formal”; these are the workers who are protected by EPL. One important feature of the law is that it does not apply to workers who have reached retirement age, which is generally

58 years old.

There are two reasons why EPL may lead to an older workforce. The first is that a forward-looking firm is reluctant to hire a young worker if it will not be able to fire her if she turns out to be unproductive, which I call the apprehension effect. Older workers are especially “safe” in India, because firms know there is no risk of an unproductive worker staying on past the retirement age. The second reason that EPL leads to an older workforce relates to what happens if a firm does hire a worker who turns out to be a poor worker. In the absence of EPL, the firm can fire the worker and hire someone else. New hires tend to be young, so this makes the labor force younger. On the other hand, if a firm is disallowed from firing because of EPL, then the worker will remain with the firm, so the labor force will be older. I call this the legacy effect.

First I demonstrate a correlation: in states with stronger EPL, older workers are more likely to be formal. The strength of a state’s EPL is calculated by aggregating the ratings of three earlier papers.¹ To argue that stronger EPL causes older workers to be in the formal sector, as opposed to the results being driven by an omitted variable or reverse causality, I consider heterogeneity across manufacturing sectors. The broad strategy is to argue that some manufacturing sectors are more impacted by EPL than others. If EPL causes the formal sector to be older, then we should observe more old formal sector workers in sector-state combinations that have strong EPL and are most affected by EPL. Rajan and Zingales (1998) use similar reasoning to argue that financial development causes economic growth, because countries with strong financial systems have especially strong manufacturing sectors in exactly the sectors that rely heavily on external finance.

Which manufacturing sectors in India are most affected by EPL? Sectors where employers would frequently fire workers if not for EPL are the sectors where EPL matters the most, but it is not possible to observe directly which sectors would have the most fir-

¹They are Besley and Burgess (2004), Bhattacharjea (2008), and OECD (2007).

ing. The United States is a useful proxy because it has very weak EPL.² There is a strong correlation between the rank of involuntary separation rates from one country to another (Micco and Pagés 2006), which provides some reassurance that the rank of separation rates is driven by characteristics of the sector rather than by idiosyncratic country-specific factors.³ Thus, I argue that EPL causes the shift in jobs from young to old because older workers are especially likely to be formal in strong-EPL states in manufacturing sectors where the involuntary separation rate in the United States is high.

The next question is whether the shift in jobs from young workers to older workers has a measurable effect on the total factor productivity (TFP) of large plants. If the age composition that a plant would choose in the absence of EPL is profit-maximizing, and EPL causes the plant to employ older workers, then EPL likely reduces plants' profits, and its TFP. To examine this question empirically, it is important to first identify which sector-states are most impacted by EPL. I define the "age shift" as the difference in the average ages of the formal sector and informal sectors. I interpret a high age shift, meaning an older formal sector, as evidence that a sector-state is strongly affected by EPL. This could be because the state has strict EPL, the sector has a high involuntary separation rate, or because the sector-state has strong enforcement of EPL.

Among plants large enough to be affected by EPL, there is a negative relationship between TFP and age shift. Smaller plants provide a useful placebo test, and there is no relationship between TFP and age shift for plants that are too small to be affected by EPL, which suggests that EPL reduces TFP. There are two suggestive arguments that the age distortion is one channel through which EPL reduces TFP. First, younger workers have higher education on average, and EPL does reduce the expected education of formal sector workers through the age distortion by 0.15 years. If education is a proxy for individual

²I also use the involuntary separation rate in a group of Latin American countries as a robustness check.

³Bassanini et al. (2009) also use the involuntary separation rate of the United States as a proxy for the separation rate of a manufacturing sector in the absence of EPL.

productivity, then this is evidence that that EPL reduces TFP through the age distortion. The magnitude is relatively modest effect, but a state with weak EPL compared to other Indian states still has very strong EPL, so a change to truly weak EPL may have a larger effect.

Second, I look at the relationship between EPL and TFP in old and new plants. New plants have no existing employees, so they are not affected by the legacy effect, but are affected by the apprehension effect. Older plants, on the other hand, are affected by both. There is a stronger negative relationship between EPL and TFP in old plants, which is consistent with the hypothesis that the age distortion is an important channel.

This paper connects two branches of the labor literature. The first is a body of literature which shows that EPL has negative effects on the employment prospects of the young.⁴ This literature has been concentrated in wealthy or middle-income countries, and my paper is the first to find this effect in India. The second strand of literature contends that EPL reduces TFP.⁵ With the exception of Ichino and Riphahn (2005), who show that EPL reduces TFP by creating moral hazard, these papers do not explore the mechanism. I bridge the gap between the two branches by arguing that EPL creates an age distortion, and that EPL reduces TFP by creating the age distortion.

The rest of the paper is organized as follows. Section 2 provides some background information on the relevant legal institutions of India. Section 3 presents a simple theoretical model. Section 4 outlines the data, the empirical strategy, and the results, and Section 5 concludes.

⁴Notable papers in this vein include: Montenegro and Pagés (2004) in Chile, Heckman and Pagés (2001) in Latin America, Skedinger (1995) in Sweden, Esping-Andersen (2002) and OECD (2004) in Europe.

⁵See, for example, Bassanini et al. (2009) in OECD, Okudaira et al. (2013) in Japan, Autor et al. (2007) in the United States, and Dougherty et al. (2013) in India.

2 Formality and EPL in India

In order to be protected under Chapter VB of the Industrial Disputes Act, a worker must meet several qualifications, but if a worker is protected, then it is very difficult for her employer to fire her (Ahsan and Pagés 2007). First, she must be a regular employee, as opposed to a contract worker, who are hired through outside firms rather than being hired directly. Contract workers have become more common during the past several decades, and may be fired by firms without government permission (Chaurey 2015). Employers cannot dodge EPL without consequence by relying entirely on contract workers, because if the principal employer exercises direct control over workers employed through an outside firm, those workers become regular workers and are entitled to the protections of the IDA (Kumar 2010). The courts can also convert contract workers to regular workers if they do the same tasks as regular workers.

Second, workers are only protected if they are in a plant that is sufficiently large. The employment threshold where Chapter VB applies varies by state, because India's constitution places labor laws on the concurrent list, meaning it is under the purview of both the central and state governments. Under the central act, plants with 100 workers or more must receive government permission to fire a regular worker or shut down the plant, but states can increase or decrease the threshold with permission from the central government. Uttar Pradesh has modified the threshold to 300, and West Bengal's threshold is 50. No state changed this threshold or made any other meaningful changes to the IDA during the span of my data (1998 - 2009), so my analysis relies on the interaction of time-invariant strictness of EPL and sector heterogeneity, rather than on intertemporal variation.

Most of the differences between states' EPL is not related to the IDA's employment thresholds. For example, Andhra Pradesh passed an amendment to the IDA in 1987 that extends the notice that must be given to a worker before the terms of her job can be changed,

which is a strengthening of the IDA; see Malik (2007) for more details.

Finally, EPL no longer applies once a worker has reached retirement age. If that age is not specified in the contract of a manufacturing worker, the default is 58 years old, and in practice that is almost always the age that applies.

3 Theory

This section will develop a model that illustrates how EPL impacts a firm's decision between an old and a young employee, and the implications for productivity. That main tradeoff is that a firm is willing to hire a slightly less productive older worker in order to preserve flexibility for the next period.

A firm chooses between a young and old employee in every period, but if it hires a young employee, it may be constrained by EPL to retain that employee in the following period. In some manufacturing sectors, the old have comparative advantage relative to the young. For example, old workers may be more productive than young workers in tasks requiring human capital that is built up over a career, while young workers are more effective at physical labor.⁶EPL is less important in manufacturing sectors where older workers are more productive, because firms can hire an old worker, employ her until retirement, and then hire another old worker.⁷

In order to keep the focus on the choice between old and young, I abstract from a number of important issues, such as how a firm chooses its number of employees (and therefore whether or not it crosses the employment threshold to be affected by EPL), how wages are affected by EPL, and the role of competition in the labor and product markets.

⁶It is not important whether young or old workers are more productive in a sector in an absolute sense; what is important is that the difference in productivity between young and old is different across sectors.

⁷Alternatively, the model could be designed so that sector heterogeneity is based on returns to experience within a particular firm, or so the preciseness of the signal of the young worker's ability varied across sectors.

In practice, large firms employ a mix of regular workers and contract workers. A given worker is likely to be more productive as a regular worker than a contract worker, because of the restrictions on the tasks that a contract worker can perform and on the control that managers can exert on a contract worker. In the interest of simplicity, I focus entirely on the regular workers for the purpose of the theory.

3.1 Firms

When firm m is created, I assume that its level of labor L_m is exogenously given, and fixed over time. The focus of this model is the formal sector, meaning that $L_m \geq 100$, because EPL does not impact smaller firms. The l -th worker in the firm in period t has productivity θ_{lt} , and the firm's output in time t is given by the production function:

$$Output_{mt} = \sum_{l=1}^{L_m} \theta_{lt}. \quad (1)$$

The firm's only cost is wages, so the firm's profit in a period is $\sum_{l=1}^{L_m} (\theta_{lt} - w_{lt})$, and its present discounted profit is

$$Profit_m = \sum_{t=0}^{\infty} \beta^t \sum_{l=1}^{L_m} (\theta_{lt} - w_{lt}) \quad (2)$$

Firms and workers are both immobile, in the sense that they cannot choose their manufacturing sector or their state. The wage-setting process will be described below.

3.2 Labor market and timing

In period 0, a risk-neutral firm is created, and it is matched with one young and one old worker for each of its L_m positions. I assume that a worker applies for a specific position at a firm, meaning that if a firm will have L_m employees, it chooses between one young and

one old applicant for each position.⁸ The employee works and receives a wage, and the firm realizes a profit that is difference between the wage and the worker's productivity.

In period 1, if the firm hired the old worker in period 0, then the worker retires and the firm is in an identical situation that it was in during period 0. It is again matched with one young and one old worker, employs one of them, and realizes a profit. If the firm hired the young worker in period 0, then with probability e , it will be forced to employ her again in period 1 (high e corresponds to strong EPL). In that case, she will retire after period 1, and the firm will once again be free to hire either a young or an old worker in period 2. If the firm is allowed to fire her, then it is free to choose between keeping her and hiring the young worker, meaning that it is the same position as a firm that hired an old worker in period 0. This process continues for an infinite number of periods.

3.3 Productivity and information

A young worker has productivity $\theta \sim U[\underline{\theta}, \underline{\theta} + 1]$, but the realization is not observed by the firm. The parameter $\underline{\theta} > 1/2$ represents the lowest possible productivity of a young worker, and is constant across all manufacturing sectors and firms. The firm knows the distributions of all of the variables, and knows that each draw of θ is independent. An old worker has productivity $\theta \sim U[\underline{\theta} - f, \underline{\theta} + 1 - f]$, where $-1/2 < f < 1/2$ is specific to the manufacturing sector and exogenous for the firm. The bounds on $\underline{\theta}$ and f are chosen to ensure an interior solution, and to guarantee positive productivities. A high value of f means that young workers in the sector are relatively productive, so firing is frequent, while a low value of f means that employers rarely want to fire workers.

I assume the firm observes the old worker's productivity perfectly, because the old employee has a verifiable track record.⁹

⁸If a firm could observe L_m young and L_m old workers and choose the best L_m workers, there would be economies of scale in the matching process.

⁹This assumption could easily be relaxed, since the firm is risk-neutral and the old worker will retire after

3.4 Wages

To describe how a worker's wage is determined, it is important to first describe her outside option. There are many more workers who want formal sector jobs than there are available positions, so I assume that a worker who passes up an opportunity at a formal job ends up in the informal sector. A worker with productivity θ in the formal sector has productivity of $\alpha\theta$ in the informal sector, where $0 \leq \alpha < 1$ is constant across workers, manufacturing sectors, and states. The informal sector is competitive, with no search frictions, and a worker can earn her expected productivity. The information structure and timing in the informal sector is the same as in the formal sector. A young worker's expected productivity in the formal sector is $\underline{\theta} + 1/2$, so her outside option is to work in the informal sector for a wage of $\alpha(\underline{\theta} + 1/2)$. Firms can observe an old worker's productivity in the formal sector, θ , so firms also know that her productivity in the informal sector is $\alpha\theta$.¹⁰

I assume that when a firm retains an employee, whether because it chooses to or because it has been forced to be EPL, the firm makes a new wage offer. This new wage takes into account the worker's productivity, which has now been revealed. EPL does place restrictions on pay reductions,¹¹ but in the interest of simplicity, I assume that EPL prevents firing, but does not impact wage setting.

When the formal sector firm has chosen which worker to hire, it has all of the bargaining power, and it makes a take-it-or-leave-it offer to the worker. If it chooses the young worker, it offers a wage of $\alpha(\underline{\theta} + 1/2)$, and if it chooses the old worker, it offers $\alpha\theta$. Hiring the young worker gives the firm an expected profit of $(1 - \alpha)(\underline{\theta} + 1/2)$, while hiring the old

the next period; the model is essentially unchanged if the old worker's expected productivity is distributed $\theta \sim U[\underline{\theta} - f, \underline{\theta} + 1 - f]$ instead of her actual productivity.

¹⁰Human capital is assumed to not be firm-specific, but the results would be the same if human capital were entirely firm specific and the outside option of all workers was some wage w , where $w < \underline{\theta}$.

¹¹If it did not, then EPL would never have any bite, as an employer could retain an employee at a wage of zero.

worker gives the firm a profit of $(1 - \alpha)\theta$. The firm's present discounted profit is

$$Profit_m = (1 - \alpha) \sum_{t=0}^{\infty} \beta^t \sum_{l=1}^{L_m} \theta_{lt} \quad (3)$$

3.5 A firm's decision

Each position is independent of the others, so the firm's problem is to maximize the profits from a single position within the firm. The profit for position l in firm m is,

$$Profit_{ml} = (1 - \alpha) \sum_{t=0}^{\infty} \beta^t \theta_{lt} \quad (4)$$

The only decision the firm makes is whether to hire the young worker or the old worker. Every period in which the firm has a choice between the workers is identical, so without loss of generality we can say the firm chooses a threshold $z - f + \underline{\theta}$, where it hires an old worker if the old worker has productivity of at least $z - f + \underline{\theta}$. In periods when it has the choice of employees, the firm chooses the young worker with probability z (it is more convenient to think of the firm as choosing the probability z rather than the threshold $z - f + \underline{\theta}$).¹²

The reason that a firm may hire an old worker with productivity below $\underline{\theta} + 1/2$ is to preserve flexibility for the next period. If the old worker has productivity just below $\underline{\theta} + 1/2$, then hiring the young worker does maximize expected productivity in the current period, but the firm risks being committed to a low-productivity employee in the next period.

I define the firm's present discounted profit (starting when the firm is not committed to a worker) as a function of z as $V(z)$. If the firm hires the young worker in period 0, her expected productivity is $\underline{\theta} + 1/2$ in period 0. With probability e , it must retain the employee, meaning the expected productivity is $(\underline{\theta} + 1/2 - f)$ in period 1, and then

¹²For example, if $\underline{\theta} = 3$, $f = .1$, and $z = .4$, then the old worker's productivity is distributed $U[2.9, 3.9]$, and the firm hires an old worker whose productivity is at least 3.3.

the worker retires and the firm is uncommitted in period 2, so it receives $V(z)$ as a continuation value, starting in period 2. If it is allowed to fire her in period 1 (which occurs with probability $1 - e$), then it has continuation value of $V(z)$ starting in period 1. Therefore, the discounted sum of expected productivities when hiring the young worker is $(\underline{\theta} + 1/2 + e[\beta(\underline{\theta} + 1/2 - f) + \beta^2V(z)] + (1 - e)\beta V(z))$.

If the firm hires the old worker, her productivity is distributed $U[z - f + \underline{\theta}, 1 - f + \underline{\theta}]$, so her expected productivity is $\underline{\theta} + ((z - f) + (1 - f))/2 = \underline{\theta} + (z + 1)/2 - f$. The firm then earns continuation value of $V(z)$ starting in period 1. We know that the firm is free to hire either worker in period 0, so the probability of hiring the young worker in period 0 is z .

The firm's profit as a function of z is

$$V(z) = z(\underline{\theta} + 1/2 + e[\beta(\underline{\theta} + 1/2 - f) + \beta^2V(z)] + (1 - e)\beta V(z)) + (1 - z)[(z + 1)/2 - f + \underline{\theta} + \beta V(z)] \quad (5)$$

Solving for $V(z)$

$$V(z) = (-1 + 2f - z - 2fz + z^2 - ez\beta + 2efz\beta)/(2(-1 + \beta)(1 + ez\beta)) + \underline{\theta}/(1 + \beta), \quad (6)$$

and maximizing over z yields the optimal choice of z :

$$z^* = \begin{cases} \frac{\sqrt{\beta e + 2\beta e f + 1} - 1}{\beta e} & e > 0 \\ 1/2 + f & e = 0 \end{cases} \quad (7)$$

Note the numerator and denominator are both zero of the top expression are equal to 0 when $e = 0$, and it can be verified via l'Hôpital's rule that this expression is smooth at $e = 0$. To gain some intuition behind this expression, consider the special case where $e = f = 0$.

Old and young workers are equally productive, and the firm is free to fire any worker, so the firm will just choose whichever worker has the higher productivity in every period, so the probability of hiring the young worker, z^* , is equal to $1/2$. Equation (7) indicates that $z^* = 1/2 + f = 1/2$, as expected.

3.6 Individual-level outcomes

The first question is how the likelihood of a young person being hired varies as EPL varies in strictness. For a young person to be hired, the firm needs to be free to choose and then actually choose the young. Based on equation (5), for every period in which the firm is free to choose, it will be committed to the old worker in ez^* periods, meaning that it is free in a fraction $1/(1 + ez^*)$ of the periods and committed in $ez^*/(1 + ez^*)$ of the periods. Since it chooses the young worker with probability z^* when it has a choice, it will hire a young person with probability $z^*/(1 + ez^*)$. The effect of strengthening EPL on the likelihood that a young person is hired,¹³ is:

$$\frac{\partial}{\partial e} \frac{z^*}{1 + ez^*} < 0 \quad (8)$$

See Appendix 1 for proof. In words, increasing EPL reduces the chance of a young person being hired, because with strong EPL a firm is less likely to be allowed to fire an old employee, and even when it is free to choose, it is reluctant to hire a young person because it knows it is unlikely to be allowed to fire.

Prediction 1: Fewer young workers work in the formal sector in states with strict EPL.

Next, I examine whether EPL has a stronger impact in those sectors where firms would like to fire more often. First, I verify that in a zero-EPL environment, the firing rate is increasing in f . If $e = 0$, then a firm chooses the young worker when the old worker's pro-

¹³Note that z^* is itself a function of e .

ductivity is below $\underline{\theta} + 1/2$, which happens with probability $1/2 + f$. A worker is fired when the young worker is hired in two consecutive periods, so a worker is fired with probability $(1/2 + f)^2$, which is increasing in f , the relative productivity of younger workers.

To test whether EPL has a stronger effect in sectors where firing is more attractive, I look at how the likelihood of hiring a young worker changes with a change in e and f :

$$\frac{\partial}{\partial e \partial f} \frac{z^*}{1 + ez^*} < 0 \quad (9)$$

See Appendix 1 for proof.

Prediction 2: EPL depresses a young worker's chance of being in the formal sector more strongly in manufacturing sectors where firms would like to fire workers.

3.7 Firm-level outcomes

The first two predictions will be tested using individual-level data, and there is an analogous prediction on the firm level. Firm profitability is a function of parameters e , f , and β and the choice variable z . I define $\phi(e, f, \beta)$ as the profits associated with the optimal choice of z . We can verify that firm profitability is decreasing in EPL, and especially in high-firing sectors:

$$\frac{\partial}{\partial e} \phi(e, f, \beta) < 0$$

and

$$\frac{\partial^2}{\partial e \partial f} \phi(e, f, \beta) < 0$$

See Appendix 1 for proof. The more profitable firms also have higher productivity per period, which generates the following prediction.

Prediction 3: EPL reduces a firm's productivity, especially in high-firing sectors.

Finally, a new firm and an older firm are impacted differently by EPL. More specifically, a new firm has no existing employees, so it is not impacted by the legacy effect, while an older firm may have a low-productivity employee that it cannot fire. The hiring decisions of both firms are distorted by the possibility that a worker must be retained, so both firms experience the apprehension effect.

A firm which is committed to a worker has expected productivity of $1/2 - q + \underline{\theta}$. A firm which is not has expected productivity of $(z + 1)/2 - q + \underline{\theta}$, and since $z > 0$, the uncommitted plant has a higher expected productivity.

Prediction 4: EPL reduces the productivity of older plants more than new plants.

4 Empirics

The model generates four predictions to be tested. Below, I will explain the data sources used, the strategy that I will use to test the predictions, and the results. Finally, I will present some alternate explanations for my results.

4.1 Data

The individual-level data is from the National Sample Survey (NSS) on Employment and Unemployment, which reports a manufacturing worker's age, regular or contract status, the size of the establishment she works in, as well as education, and sex. To classify the level of EPL in each state, I rely on three papers, Besley and Buress (2004), Bhattacharhea (2008), and OECD (2007). The Annual Survey of Industries provides plant-level data necessary to compute total factor productivity (TFP). Finally, the U.S. Displaced Worker Survey and Micco and Pagés (2006) provide measures of the job loss rate in the United States and Latin America, which serves as a proxy for the firing rate in India.

4.1.1 National Sample Survey

The National Sample Survey is carried out by the central government of India, and is a nationally representative survey covering a wide variety of topics. Questions regarding employment are not asked every year, and there have been only two rounds of the survey that ask the respondent how many people work in her place of business. This question is critical for my definition of formality, so I only use data from those years, 2009-2010 and 2011-2012. I pool the data and use year fixed effects to capture firm-invariant year effects. The 2009-2010 round uses the 2004 version of India's National Industrial Classification (NIC-2004), while the latter round uses NIC-2008. Details on the concordance are available upon request.

There is evidence of age heaping in the sample; for example, there are over five times as many self-reported 40-year olds as 41-year olds. To circumvent this issue, I treat all workers between 38 and 42 years old as 40, all workers between 43 and 47 as 45, and so on. Workers whose contracts do not specify otherwise may be fired after age 58, and there is indeed a large decline in formality from the 53 to 57 age category to the 58 to 62 age category. The forces acting on those workers older than 58 are likely different from those acting on younger workers, so I exclude everyone who is 58 and above. Similarly, I exclude everyone who is under 18 because of child labor laws that affect younger workers.

The definition of "formal" varies somewhat depending on the author and the context. I am most interested in which workers are protected by EPL, meaning they must be a regular worker and not a contract worker, and in a plant with at least 100 workers (this threshold is 50 in West Bengal and 300 in Uttar Pradesh). The NSS asks about the size of the establishment where a worker is employed as a categorical response question, and the workers in the NSS are not matched to the plants in the Annual Survey of Industries dataset described below. The highest category provided is 20 or more workers, so I have

a noisy measure of whether a worker is in a plant that is truly protected by EPL. I define a worker as formal if she is a regular worker in a plant with at least 20 workers, which is overcounting somewhat.

4.1.2 State-level EPL

In order to classify the strictness of EPL in each of 15 major states, I follow the approach of Gupta et al. (2009; GHK hereafter), who analyze three earlier studies that classify states. The first paper in the group is by Besley and Burgess (2004), and classifies states according to the text of amendments to the IDA that were passed by the state. They classify each amendment passed by a state in year t as pro-worker or pro-employer, and if a majority of amendments in t is pro-worker or pro-employer, then they classify the state as moving in pro-worker or pro-employer direction in year t . A state's level of EPL in year T is defined as the sum of all of the changes in year $1, \dots, T$, where the change in every year is limited to be $-1, 0$, or 1 .

The second paper is by Bhattacharjea (2008), who argues that Besley and Burgess code some amendments incorrectly, and that the way they aggregate amendments is arbitrary. For example, if one state passes two pro-worker amendments in the same year and another passes the same two amendments in different years, then the first state has an EPL of $+1$ while the second has an EPL of $+2$. Bhattacharjea presents his own classification of each amendment to the IDA, and also includes judicial rulings that impact how the IDA is applied.

OECD (2007) uses a survey to identify a range of state-level labor reforms, breaking away from the other two studies in terms of methodology. This study encompasses the IDA, as well as the Factories Act and the Contract Labour Act. The authors consider both the statutory laws and the enforcement mechanisms that are in place.

GHK convert each study's rating of each state into a simple strong, weak, or neutral

EPL (-1, +1, 0), so that there are three ratings for each state, and I use the same three ratings for each state. The only way that our methods diverge is how to collapse from three ratings per state to one. Where there are disagreements between ratings, GHK use a majority rules approach, meaning that every state has a score of -1, +1, or 0. One downside to this approach is that it discards information. For example, all three papers agree that Maharashtra has strong EPL, while only two of the three code Orissa as having strong EPL, but GHK regard them as having the same level of EPL. The minor change that I make to GHK's method is to preserve that information and add the three ratings to arrive at an overall ranking between -3 and 3. In the main specification I treat this as an interval scale, and in a robustness check I consider EPL as a categorical variable.

To see the ranking of each state, please see Appendix Table 1.

4.1.3 Firing rate

My identification strategy depends on an assumption about which manufacturing sectors in India would have a high rate of firing in the absence of EPL. The first proxy that I consider is involuntary job loss rate in the United States, which I calculate based on the U.S. Displaced Worker Survey (DWS). I pool data from 2004, 2006, 2008, and 2010 (the DWS is published every other year). Bassanini et al. (2009) use the same method to calculate a measure of involuntary separation rates in the United States.

The ideal measure would specifically refer to firing, but the DWS includes firing as part of "other," so firing is not directly observable. Job losses that are categorized in the DWS as something other than firing may also be informative about what the firing rate in India would be. For example, if a worker in the United States loses her job and the reason given is "Position abolished," it is possible that a more junior position was created to fill a similar role. The involuntary job loss rate in the DWS is not a perfect proxy for the firing rate in India, but as long as the discrepancy between the two is uncorrelated with the independent

variables of interest, the empirical strategy is unaffected.

The DWS provides data on 16 2-digit manufacturing sectors. The only manufacturing sector that is included in the NSS but not the DWS is tobacco, so I drop tobacco throughout the paper. This leaves 20 2-digit manufacturing sectors in the NSS, classified according to the NIC-2004, and I match manufacturing sectors. Where a DWS sector encompasses two NSS sectors, I apply the DWS firing rate to both NSS sectors. Please see Appendix Table 2 for more details.

Using US separation rates as a proxy for which manufacturing sectors are most affected by EPL is an established strategy in the literature, but one natural concern is whether conditions in the United States are informative about India. While Latin America is still considerably wealthier than India, it may provide some reassurance if results are similar using data from Latin American countries. Following Davis and Haltiwanger (1999), Micco and Pagés (2006) define job reallocation as the sum of job creation and job destruction. They calculate job reallocation for eight¹⁴ manufacturing sectors for 18 countries, across the Americas and Europe. This measure is not the same as firing, but it is similar, because industries with frequent firing will also have high rates of job destruction.

In order to construct a Latin American measure of firing for each of eight sectors, I take the average job reallocation rate for the seven Latin American countries with no missing data: Argentina, Brazil, Chile, Colombia, Mexico, Uruguay, and Venezuela. Micco and Pagés also present the correlation in ranks of the job reallocation rate to assess the similarity between different countries, and find that the correlation is quite high. Colombia is the poorest country of the group and therefore the closest in GDP per capita to India, so the correlation in the rank of job reallocation rates between Colombia and other countries is instructive. With the exception of Sweden, the Spearman rank correlation between Colom-

¹⁴Again, when matching sectors, I apply the firing rate of a broad category to its components. For example, Micco and Pagés present data on paper, printing, and publishing, and I assume that it applies to both sector 21 (paper and paper products) and sector 22 (publishing, printing, and recorded media) in NIC-2004.

bia job reallocation rates and those of each of the 16 other countries is at least 0.3, which is significantly different from 0 at the one percent level.¹⁵ This finding provides some reassurance that the rank of India's firing rates in the absence of EPL would be similar to the job loss measures in the United States and Latin America.

4.1.4 Plant-level data

The main data source to calculate TFP is the Annual Survey of Industries (ASI), from 1998-1999 to 2007-08. The ASI's sampling sector includes all plants that employ at least 20 workers without power, and at least 10 workers with power, and these plants are surveyed every several years. The census sector consists of the larger factories, but the threshold has varied between 50 and 200 over the course of the sample, meaning that factories with at least 200 have been surveyed every year. For more information regarding data issues within the ASI and the sampling procedure, see Bollard et al. (2013) and Harrison et al. (2011).

I use the Levinsohn-Petrin (2003) method to calculate TFP, which is an improvement on using OLS to calculate total factor productivity. It is similar in that it defines as total factor productivity the residual from a Cobb-Douglas regression. It has the advantage that it corrects for the endogeneity generated by the fact that plants can increase their labor in response to a positive productivity shock by using a plant's intermediate inputs as an instrument for the unobserved productivity shock.

To calculate TFP, I need data on output, labor, capital, fuel, and electricity. After dropping all plant-year observations with missing or negative values, I am left with 227,601 observations and 94,037 unique plants. The ASI provides sampling weights for all plants, which again I use throughout.

The ASI lists values in current rupees, so to adjust for inflation I apply industry-specific

¹⁵Sweden is the only country whose job reallocation ranks are not correlated with the others. Micco and Pagés discuss possible reasons why Sweden is an outlier in this regard but do not reach any conclusions.

deflators to value added using the Wholesale Price Index (WPI). I deflate intermediate inputs according to the WPI associated with that product's five-digit ASICCC code. Some inputs do not have an ASICCC code listed; for example, capital is deflated using the WPI for machinery and equipment. Dougherty et al. (2011) give a detailed description of the deflating process, and I follow their method.

4.2 Empirical strategy

The first section of the empirics uses individual-level outcomes from the NSS to argue that EPL shifts jobs from young to old workers. In the second section, I calculate how strongly EPL impacts a sector-state using individual-level outcomes, and then examine the relationship between plant-level TFP and the importance of EPL in a sector-state.

4.2.1 Individual-level outcomes

The first question is whether older workers are relatively more likely to be in the formal sector in states with stricter EPL (Prediction 1 in the theory). To test this relationship, leaving aside for now the question of causality, I consider regressions of the form

$$Formal_{ijk} = f(age_{ijkt}, EPL_k, \delta_{jk}, \delta_t, \xi_i), \quad (10)$$

where i indexes individuals, j indexes manufacturing sectors, k indexes states, and t indexes years (there are two years of data). δ_{jk} is a dummy variable that the worker is in sector j and state k , and ξ_i is a vector of characteristics of worker i . The dependent variable is equal to 1 if the worker is a regular employee in a plant that employs at least 20 people.

In the main specification, I use a linear probability model of the form

$$Formal_{ijk} = \beta_1 age_{ijk} + \beta_2 EPL_k + \beta_3 age_{ijk} * EPL_k + \delta_{jk} + \delta_t + \xi_i + \varepsilon_{ijk}, \quad (11)$$

where ξ_i includes dummy variables for every level of education and for gender. I conclude that the evidence is consistent with Prediction 1 if β_3 , the coefficient on the interaction term, is positive.

Note that I estimate how EPL shifts jobs from young to old (or vice versa), but I do not estimate whether EPL creates or destroys formal sector jobs. The reason for this is that there is no time variation in EPL_k in my data, and there is a long list of state-level variables other than EPL that could impact the level of formality. I use sector-state fixed effects, which capture any variable that affects the formality of everyone equally in a sector-state (and therefore in a sector or in a state, as well). The variable EPL_k is absorbed by the fixed effects, and is listed above only for clarity.

Shifting jobs from young to old may have important implications for the education of the workforce. Young workers are disproportionately high education, so the impact of shifting jobs from young to old also decreases the average education of the workforce.

This model imposes a linear structure on EPL , meaning that the change from $EPL = -2$ (strict EPL in two indices and neutral in one) to $EPL = -1$ (strict EPL in one index and neutral in two) has the same impact as the change from $EPL = 1$ to $EPL = 2$, a topic that will be addressed in more detail in the sensitivity analysis section.

I follow the common practice of estimating both types of models so that results do not depend solely on either approach, and results are similar in all cases.

I report standard errors that are clustered at the state level to address the possibility of correlated errors within a state and because the variation in EPL occurs at the state level and I have many observations per state (Bertrand et al., 2004). In an additional robustness check, I use the Donald and Lang (2007) estimator.

Some of the potential endogeneity problems can be solved by considering sector heterogeneity, where some manufacturing sectors are more impacted by EPL than others. I rely on the approach of Rajan and Zingales (1998), who test whether financial development

causes economic growth by showing that countries with strong financial systems have especially strong manufacturing sectors in exactly the sectors that rely heavily on external finance (measured as those sectors where American firms use more external finance).

I make a similar argument: if EPL causes the shift in formal jobs from young to old workers, we should see that differences in EPL generate larger formality boosts to old workers in manufacturing sectors in which Indian employers would like to fire workers. The United States has some of the weakest EPL in the world, so it serves as a useful benchmark for what would happen in the absence of EPL. I use measures of job losses in the United States and Latin America as proxies for the firing rate in India. Bassanini et al. (2009) uses the same variation to identify the causal effect of EPL in Europe, and Dougherty et al. (2013) employ the same strategy in India.

I test prediction 2 with the following regression:

$$\begin{aligned}
 Formal_{ijk} = & \beta_1 age_{ijk} + \beta_2 EPL_k + \delta_{jk} + \delta_t + \xi_i \\
 & \beta_3 * age_{ijk} * fire_j + \\
 & \beta_4 * age_{ijk} * EPL_k + \\
 & \beta_5 * fire_j * EPL_k + \\
 & \beta_6 * age_{ijk} * EPL_k * fire_j
 \end{aligned} \tag{12}$$

and conclude there is evidence that EPL causes the different age composition if $\beta_6 > 0$ (note that EPL_k is absorbed by the sector-state fixed effects, and is listed above only for clarity).

4.2.2 Plant-level outcomes

In the first part of the empirical section, I argue that EPL distorts who is employed in the formal sector. In the second part of the paper, I will provide evidence that suggests that this

distortion reduces productivity at the plant level.

The two main datasets (NSS and ASI) are not matched, meaning I do not observe the place of employment of a respondent to the NSS or the age profile of a plant in the ASI. I use the previous results to classify sector-states by how strongly plants within them are affected by EPL.

The first option would be to use the same strategy as above and classify the impact of EPL on a sector-state by the interaction of state-level EPL and sector-level firing rate. The direct effects of EPL and firing rate will be captured by state and sector fixed effects, which are important because of the large number of variables that are constant across the state or sector that may influence TFP. One weakness of using the interaction is that there is no variation that is truly at the sector-state level. For example, if enforcement is stronger in some sector-states than others, this will not be captured.

Instead, I infer the impactfulness of EPL by observing the distribution of ages in the formal and informal sectors in the sector-state. If the formal sector is relatively old, then I conclude that EPL is important, whether because of strict EPL in a high-firing sector, or because of particularly high enforcement.

I define the “age shift” as the difference in average age of the formal and informal sectors in the sector-state. This is based on NSS rather than ASI data, so age shift is a sector-state variable rather than a plant or firm variable.

$$\text{age shift}_{jk} \equiv \text{mean age of formal sector}_{jk} - \text{mean age of informal sector}_{jk} \quad (13)$$

The line of causation is that EPL causes a decrease in TFP, but I do not directly observe EPL at the sector-state level, because enforcement is difficult to measure. EPL also causes the age shift, which I can measure, and stronger enforcement will generate a larger age

shift.¹⁶ I look at the relationship between age shift and TFP, but I do not attribute any causal relationship between age shift and TFP; instead, age shift and TFP are both impacted by EPL. Throughout the section on plant-level outcomes, a central identifying assumption is that high age shift sector are more impacted by EPL.

First, I will test whether factories in sector-states with high age shift have a lower TFP, which I will take as evidence that indication that EPL does reduce TFP. Plants with fewer than 100 workers provide a useful placebo test, since the main employment laws do not apply to them. To control for the possibility that there is an unobserved sector- or state-level variable that influences TFP, I use sector and state fixed effects, and cluster at the state level. Breaking the sample into plants with more and fewer than 100 workers (and excluding West Bengal and Uttar Pradesh because they have a different threshold for the IDA), I test prediction 3 using this regression:

$$TFP_{i j k t} = \beta_1 * age\ shift_{j k} + \delta_j + \delta_k + \delta_t + \varepsilon_{j k}, \quad (14)$$

where i indexes plants, j indexes states, k indexes manufacturing sectors, t indexes years. I conclude that EPL harms TFP if $\beta_1 < 0$ for factories with more than 100 workers and is near zero for factories with fewer.

One possible concern is that larger plants are more productive, and some unobserved variable that is correlated with age shift causes plants to be larger. I control for the log of a plant's labor to address the possibility of economies of scale, and present results above and below 100 workers, with and without the control.

Using individual-level data allows me to conclude that EPL causes a distortion in who is employed in the formal sector. It is natural to think that this creates some negative impact on TFP, and in this portion of the paper I provide evidence that is consistent with

¹⁶In terms of the theory, more enforcement can be considered an increase in e , which leads to older formal sector workers.

this hypothesis.

There are two broad classes of mechanisms through which EPL reduces productivity. The first group is the mechanisms that do not vary in strength over a plant's lifetime, and the second group is the mechanisms that impact new and old plants differently.

By comparing new and old plants, all with at least 100 workers, I can test whether there is some mechanism operating whose strength varies over the life of a plant. Finding evidence that such a mechanism does impact TFP is consistent with the hypothesis that the distortion of who is employed has an effect on TFP. The overall strategy is to split the sample in new and old plants with at least 100 workers, and estimate the same regression above:

$$TFP_{ijkt} = \beta_1 * age\ shift_{jk} + \delta_j + \delta_k + \delta_t, \quad (15)$$

If the legacy and apprehension effects are both important for TFP, then I expect to see a negative relationship between TFP and age shift for both sets of plants, but a stronger negative relationship for the old plants.

The ASI includes data on a plant's first year of operation, so it is possible to calculate how long a plant has operated, but that leaves open the question of how to define "new" and "old." In the body of the paper, I will split the observations equally into new and old, which means that new plants are defined to be 17 years old or less. To show that the results are not dependent on the threshold chosen, in a robustness check I present the same regression with a wide variety of other thresholds.

4.3 Results

In Table 1 (tables are at the end of this document), I present the results from the regression of equation (11). The linear probability model is column 1, and the probit is in column 2.

The coefficient on the variable of interest, the interaction between EPL and age, is positive and significant at the five percent level in both models, and this result is consistent with Prediction 1, which states that more old workers in the formal sector in states with strict EPL.

To give a sense of the magnitude of of an interaction effect, consider the difference in the predicted probability of formality for old and young workers depending on EPL, based on column 1 of Table 1. In the states with the weakest EPL, I predict that a 20-year old worker has a 5.9 percentage point higher probability of formality than a 55-year old, while in a state with the strongest EPL, I predict the 20-year old has an 8.5 percentage point lower chance of formality. Approximately 21 percent of both 20-year olds and 55-year olds are formal across the entire sample, meaning that the effect is substantial, but plausible.

In addition, based on each worker's predicted chance of formality with different levels of EPL, I can calculate the expected level of education in the formal sector at every level of EPL. I predict that moving from the strongest EPL of any state to the weakest would increase the average years of school in the formal sector by approximately 0.15 years. If education is a reasonable proxy for productivity, then this is evidence that EPL reduces TFP by distorting who has a job. The effect is driven by the fact that EPL increases the average age of the formal sector workforce, and younger workers in India on average have more education. It is true that these younger workers have less experience, but the fact that plants who are relatively unimpacted by EPL choose younger workers suggests that education is in fact important.

An increase of schooling by 0.15 years may appear to be a relatively modest effect in terms of magnitude, but the state with the weakest EPL still has very strong restrictions on firing relative to other countries. Even in relatively pro-employer Uttar Pradesh, government permission is still required to fire a worker in a plant with over 300 workers. If a state in India relaxed EPL to the level of the United States or even to Western Europe, it is likely

that the gains in education would be significantly larger.

Next, I test prediction 2, which is that the relationship in Table 1 is especially strong in those manufacturing sectors where employers would like to fire workers. A positive coefficient on Age x EPL x Firing suggests that prediction 2 is true. One way to think of this is that an older worker in a high-firing sector in a strict-EPL state is more likely to have a job in the formal sector than an older worker in a high-firing sector in a lax-EPL state, because this is exactly the type of employee who is most helped by EPL.

In columns 1 and 2, I test this relationship using the US firing rate, with linear and probit models, and results for both are positive and significant. With the Latin American firing rate, about 10 percent of the observations are missing because of a lack of firing rate data in some manufacturing sectors. The linear model, in column 3, is positive and significant, although at the 10 percent level, while the probit results are positive and significant at the one percent level.

Moving to the plant-level data, I estimate Equation (14) to test whether plants in sector-states that are most affected by EPL, as measured by the age shift in the sector-state, have a lower productivity (prediction 3). I find there is a negative and significant relationship for the plants with at least 100 workers (column 1), but no relationship for smaller plants (column 2), which is consistent with the hypothesis that EPL reduces TFP. The fact that there is no relationship between age shift and TFP for small plants provides some assurance that there is not some other variable that is invariant within each sector-state that is driving the results. I do find a positive and strongly significant coefficient on log of labor, which indicates that large plants tend to be more productive, but including that variable does not cause a major change to the coefficient on age shift.

In order to discuss the magnitude of this relationship, the first step is to estimate how much a change in EPL impacts the age shift. I compute expected chance of formality based on column 1 of Table 1. If all of India moved from the lowest level of EPL in the sample

to the highest level, then I estimate the age shift would increase by 2.6 years.

Based on column 1 of Table 3, we can conclude that moving from the weakest to the strongest EPL causes an increase in the age shift by 2.6, and also causes a decrease in the TFP variable of .021 ($2.6 \times 7.9 / 1000$). Because TFP is measured as the log of productivity, this corresponds to approximately 2.1 percentage points.

Finally, I estimate Equation (14) again, but splitting the sample in a different way to separate mechanisms that do not vary over the lifetime of a plant (such as the apprehension effect) and those that do (such as the legacy effects). I focus exclusively on the plants with at least 100 workers, and divide the sample into new and old plants. In Table 4, I define “old” plants as 17 years. For plants over 17 years, I find a significant negative coefficient on age shift with and without including log labor as a control. For plants under 17 years old, the coefficient on age shift is also negative, but it is not significant. An F-test reveals that the difference between the two coefficients is significant, at the 5% level without controlling for log labor, and at the 1% level when controlling for log labor.¹⁷

The fact that old plants are more affected by EPL than young plants is consistent with the hypothesis that the legacy effect is important for TFP. The coefficient is negative in all cases, which is what we would expect if the apprehension effect is important for TFP. However, the relationship is not statistically significant. This may reflect the fact that age shift is a noisy measure of EPL.

4.4 Alternative explanations

In this section I discuss some alternate explanations that could also generate the results presented above, and in some cases I present empirical analysis to test their importance.

¹⁷The comparison of the coefficients is performed by regressing TFP on age shift, fixed effects for year, sector, and state, where the coefficients on all variables are allowed to be different for new and old plants. Then I test for equality of the coefficient on age shift with new and old plants using an F-test.

4.4.1 Individual-level results

One way that the regression in Equation (12) could generate a spurious non-zero result is if some characteristic of sector-states leads to the passage of stricter EPL. For example, suppose that one state has old workers and strong unions in high-firing sectors, while another has old workers and strong unions in low-firing sectors. Unions in the former would have a stronger incentive to spend resources lobbying for stricter EPL, and if successful, then I would estimate a positive coefficient on $age_{ijk} * EPL_k * fire_j$ even if EPL did not cause the change in the age of the workforce. However, consider the identical scenario, but this time the workers are young instead of old. Unions in the high-firing sector would still lobby for stricter EPL, and now I would estimate a negative coefficient on $age_{ijk} * EPL_k * fire_j$. The issue is that strong unions will push for strict EPL whether their members are mostly old or mostly young, because there is value in helping any union member keep her job.

To generate a positive correlation without a causal relationship, we need to assume that union lobby more when their members were older. But if the main goal of the union is to protect its older members and they are able to influence legislation, it is surprising that the changes to EPL that are passed are provisions like making strikes easier or increasing the penalties for unauthorized firing, which apply broadly to all workers. The more logical step to help specifically older workers would be raise the retirement age, but this has not been done.

A related question is how characteristics of a state influence the types of legislation they pass. For example, those states with an older workforce may have been more likely to pass stricter EPL when those laws were being passed (primarily in the 1970's and 1980's). To the extent that these state-level characteristics are persistent, this phenomenon could generate a correlation between older formal sector workers and stronger EPL, even if the latter does not cause the former.

I can test whether this is a significant concern, albeit indirectly. The main question is whether “sunset industries,” those which are declining over time, are substantively different from industries that are growing over time. The concern would be that states with a larger share of sunset industries would be more inclined to protect older workers. I define a sector’s growth over time as the percentage growth in employment between 1999 and 2008, and a state’s exposure to sunset industries is the average growth of the sectors they produce, weighted by the labor force in 1999. Table 5 presents the same regression as those in column 1 of Table 1, but the sample is split into states whose production is mostly in slow-growing sectors (column 1), mostly in fast-growing sectors (column 2), and all sectors (column 3). The coefficients on the variable of interest, Age x EPL, is somewhat larger for the states focusing on fast-growing sectors, but this suggests that age leads to formal sector employment more strongly in the fast-growth states, which is the opposite direction from what one would expect if the states heavy in sunset industries are more likely to protect older workers. Table 6 presents analogous results for the regression in column 1 of Table 2. Here the coefficient on Age x EPL x Firing is nearly identical in the two subsamples, which provides some confidence that the results are not being driven by a state’s composition of slow- and fast-growing industries.

4.4.2 Plant-level results

Next, consider problems of selection when estimating Equation (14) for plants above and below 100 workers. The concern is that plants’ decision to cross the 100-worker threshold is itself impacted by EPL. The most natural version of this story is that in strict-EPL states, only the most productive plants are willing to become large enough to face EPL, but that would lead to a positive relationship between TFP and age shift in large plants, rather than the negative relationship that I find. That said, one way to address the selection issue is to consider plants who are further from the threshold, because a plant far above or far below

100 workers would likely be on the same side of 100 even if EPL were weaker or stronger. I estimate this regression again in Table 8 using thresholds of 50 and 200 workers, and find similar results.

My explanation for the results in Table 4 is that new and old firms are impacted differently by EPL because they are new or old, but there is another possible interpretation. Chapter VB, the most important form of EPL, was passed in 1976, and applied to all of India. A firm entering the market in, say, 1970 may not have anticipated the legal changes, while a firm entering in 1990 could easily observe it. If firms know a priori whether they will be more or less affected by EPL, then we would expect that in 1970 only the less impacted firms would enter the market, which would generate the observed results that older firms are more affected by EPL. In order to test whether this is an important concern, I reproduce Table 4, but among old firms, I include only those formed after 1976. The results, which are presented in Table 7, are substantively the same.

The results in Table 3 and 4 are consistent with the hypothesis that the distortion in who has a job is important for TFP. They also provide evidence against an alternative hypothesis: that the entire mechanism through which EPL reduces TFP is that EPL reduces worker effort and does not impact who has a job. This hypothesis is not consistent with the results in Tables 3 and 4 because it would imply that new and old plants should be equally affected by EPL.

There is at least one other hypothesis that is consistent with the results on new and old plants. EPL prevents plants from responding quickly to changes in demand. For example, suppose a new plant chooses an employment level that is appropriate for the time when it is created. As the optimal level of employment changes over time, the plant cannot adjust downward if EPL is strong, and may have too many workers. Older plants would then be more affected by EPL than newer plants, which is consistent with my findings. It is the subject of future work to disentangle this effect from the impact of changing who has a job.

4.5 Sensitivity analysis

In this section I consider variations of the regressions presented to test for sensitivity. I present results for some of the main tests, and describe the others. Results are available on request.

4.5.1 Individual-level results

In Table 1, I estimate the increased likelihood of being in the formal sector of being an older worker in a strong-EPL state. In Table 2, I test whether the relationship displayed in Table 1 is stronger in those manufacturing sectors where the involuntary job loss rate in the United States is higher.

Here, I modify the regressions that produce Tables 1 and 2 in a variety of ways. First, instead of using the aggregate EPL rating, I use each of the three component ratings (Besley Burgess 2004, Bhattacharjea 2008, and OECD 2007) separately. The results are similar. All coefficients remain the same sign, and while several results lose significance, some become stronger.

In a second set of tests, I treat the aggregate EPL score as a categorical variable rather than a continuous variable. The coefficients follow the broad pattern that would be expected, where the coefficients on the higher levels of EPL are generally larger. The coefficients do not move linearly though, possibly because there are few states in some of the EPL categories.

Next, I estimate each of the regressions in Tables 1 and 2 dropping each state one at a time, and doing the same for sectors. The results stay very similar throughout. I also drop the self-employed, use a wide variety of different fixed effects specifications, and include different individual controls. The results in all cases remain substantively the same.

4.5.2 Plant-level results

In Table 3, I estimate the relationship between age shift and TFP in plants with at least 100 workers and in those with fewer than 100 workers, which is the threshold where the main EPL takes effect. One concern is that plants select into the above and below 100 worker categories based on some unobserved characteristic. Plants that are far from the threshold in either direction are more likely to have chosen a size for some reason other than the EPL threshold, so restricting the sample to plants that are far from the threshold should minimize the concern regarding selection. In Table 8, I estimate the same specification as in Table 3, but I change the employment thresholds to 1 to 49 and 200 and above. The significance for the small plants goes from the five percent level to the ten percent level, but the coefficient remains the same sign and similar magnitude.

In Table 4, I test whether new and old plants have different relationships between age shift and TFP. I define “new” plants as those under 17 years old, but since this is an arbitrary cutoff, I present the same results as in columns 3 and 4 of Table 4 (that is, controlling for log labor) for every threshold between 2 and 30 years. For low cutoffs, there is no significant difference between the coefficients, meaning that I do not reject the null that new and old plants are affected equally by EPL. For cutoffs above 10 years, the difference is significant at the 10 percent level, and for cutoffs above 15 years, the difference is significant at the one percent level. One possible reason that I do not reject the null for low cutoffs is that relatively few plants are younger than 10 years old (recall that the median age is 17 years). These results are presented in Table 9.

5 Conclusion

I have presented a theoretical model and empirical evidence that EPL shifts formal sector jobs away from young workers in favor of older workers. This happens because a plant would rather not commit itself to a young worker for future periods, which distorts the hiring decision. Then when the plant does decide to hire a young worker, it will retain her when she is old, even if it would prefer to fire her, and both of these effects raise the age composition of the plant.

In the second part of the paper, I assume that those sector-states that have relatively old formal sectors are the sectors that are most impacted by EPL. Plants in those sector-states have lower TFP, which suggests that EPL reduces TFP. There is a relationship among plants with at least 100 workers, meaning that EPL applies to the plants, but no relationship in the smaller plants. This is a useful placebo test, so that we can rule out the possibility that an unobserved omitted variable is driving the results.

I have also presented evidence that is consistent with the hypothesis that the distortion in who is employed is responsible for at least part of the TFP reduction. I calculate predicted education rates in the formal sector under different EPL schemes, and find that they are lower when EPL is stronger. These results are driven by the fact that EPL prevents younger workers from securing jobs, and younger workers on average have higher education. One limitation of this approach is that education is an imperfect proxy for productivity, especially in tasks that are more dependent on experience.

EPL reduces TFP more strongly in old plants than in new plants. This is what we would expect if EPL reduces TFP by distorting who has a job, because new plants experience only the apprehension effect, while old plants experience both the apprehension and the legacy effect. The result on old and new plants can rule out the possibility that EPL reduces TFP entirely through a mechanism that does not vary over the life of a plant. For example,

this result is inconsistent with the hypothesis that the only mechanism is that moral hazard causes effort to decrease by a fixed percentage in every period.

However, there is at least one other possible explanation that is consistent with these findings: that EPL reduces TFP primarily by distorting the number of people employed, rather than distorting who is employed. It is plausible that new plants are closer to the optimal number of employees than older plants, depending on a plant's strategy regarding size, which would generate results consistent with the data, because new firms would be less affected by EPL than older firms. It will be the focus of future work to disentangle these two mechanisms.

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Tables

Table 1: Formality, age, and employment protection legislation

	(1)	(2)
Age x EPL	0.936** (0.350)	1.075** (0.421)
Age	-0.002* (0.001)	-0.002** (0.001)
Male	0.060*** (0.016)	0.073*** (0.025)
Observations	22897	22897
Model	Linear	Probit

The dependent variable is 1 if the worker is formal and 0 otherwise. EPL is divided by 1000 for readability. State-sector and year fixed effects, and an indicator variable for level of education, are not reported. Probit coefficients are marginal effects at the mean. Standard errors (in parentheses) are clustered at the state level.

Note: * $p < .1$ ** $p < .05$ *** $p < .01$.

Table 2: Formality, age, separation rate, and EPL

	(1)	(2)	(3)	(4)
Age x EPL x Firing (US)	0.084** (0.031)	0.096*** (0.026)		
Age x EPL x Firing (Latin Am.)			0.139* (0.067)	0.142*** (0.025)
Age	-0.001 (0.002)	-0.001 (0.002)	0.001 (0.001)	0.002** (0.005)
Male	0.060*** (0.016)	0.074*** (0.024)	0.063*** (0.015)	0.049*** (0.094)
Observations	22897	22897	20755	20755
Model	Linear	Probit	Linear	Probit

The dependent variable is 1 if the worker is formal and 0 otherwise. Age x EPL and Age x Firing are included in the regression but not displayed (EPL x Firing is not included because it collinear with the fixed effects). State-sector and year fixed effects, and an indicator variable for level of education, are not reported. Probit coefficients are marginal effects at the mean. EPL is divided by 1000 for readability. Standard errors (in parentheses) are clustered at the state level.

Note: * $p < .1$ ** $p < .05$ *** $p < .01$.

Table 3: The relationship between TFP and age shift in large and small plants

	(1)	(2)	(3)	(4)
Age shift in the sector-state	-7.894** (3.760)	1.696 (4.349)	-8.130** (3.619)	0.665 (4.208)
Log labor			0.217*** (0.016)	0.235*** (0.025)
Observations	57897	143965	57897	143965
Plant size	100+	1-99	100+	1-99

The dependent variable is plant-level TFP. The sample is divided between plants with below 100 (no EPL) and above 100 (EPL) workers. Sector, state, and year fixed effects are not reported. Standard errors (in parentheses) are clustered at the state level.

Note: * $p < .1$ ** $p < .05$ *** $p < .01$.

Table 4: The relationship between TFP and age shift in old and new plants

	(1)	(2)	(3)	(4)
Age shift in the sector-state	-3.290 (4.126)	-12.424*** (4.139)	-2.993 (4.068)	-13.732*** (4.004)
Log labor			0.205*** (0.020)	0.210*** (0.023)
Observations	29128	28769	29128	28769
Plant size	100+	100+	100+	100+
Plant age	1-17	18+	1-17	18+

The dependent variable is plant-level TFP. Age shift is divided by 1000 for readability. The sample is divided between new plants and old plants, all with at least 100 workers. Sector, state, and year fixed effects are not reported. Standard errors (in parentheses) are clustered at the state level.

Note: * $p < .1$ ** $p < .05$ *** $p < .01$.

Table 5: Formality, age, and employment protection legislation: States producing high- and low-growth goods

	(1)	(2)	(3)
Age x EPL	0.747* (0.319)	1.122** (0.410)	0.936** (0.350)
Age	-0.002 (0.002)	-0.002 (0.001)	-0.002* (0.001)
Male	0.097*** (0.025)	0.040** (0.014)	0.060*** (0.016)
Observations	8750	14147	22897
Model	Linear	Linear	Linear
Growth of state	Slow	Fast	All

The dependent variable is 1 if the worker is formal and 0 otherwise. EPL is divided by 1000 for readability. State-sector and year fixed effects, and an indicator variable for level of education, are not reported. States are split into fast- and slow-growth based on their production of fast- and slow-growing sectors. Standard errors (in parentheses) are clustered at the state level.

Note: * $p < .1$ ** $p < .05$ *** $p < .01$.

Table 6: Formality, age, separation rate, and EPL: States producing high- and low-growth goods

	(1)	(2)	(3)
Age x EPL x Firing (US)	0.060 (0.038)	0.065* (0.030)	0.084** (0.031)
Age	-0.003 (0.003)	-0.000 (0.002)	-0.001 (0.002)
Male	0.095*** (0.025)	0.041** (0.014)	0.060*** (0.016)
Observations	8750	14147	22897
Model	Linear	Linear	Linear
Growth of state	Slow	Fast	All

The dependent variable is 1 if the worker is formal and 0 otherwise. Age x EPL and Age x Firing are included in the regression but not displayed (EPL x Firing is not included because it is collinear with the fixed effects). State-sector and year fixed effects, and an indicator variable for level of education, are not reported. States are split into fast- and slow-growth based on their production of fast- and slow-growing sectors. EPL is divided by 1000 for readability. Standard errors (in parentheses) are clustered at the state level.

Note: * $p < .1$ ** $p < .05$ *** $p < .01$.

Table 7: The relationship between TFP and age shift in old and new plants: Dropping firms started before 1976

	(1)	(2)	(3)	(4)
Age shift in the sector-state	-3.290 (4.126)	-12.181* (5.937)	-2.993 (4.068)	-12.792** (5.602)
Log labor			0.205*** (0.020)	0.233*** (0.035)
Observations	29128	12356	29128	12356
Plant size	100+	100+	100+	100+
Plant age	1-17	18+	1-17	18+

The dependent variable is plant-level TFP. Age shift is divided by 1000 for readability. The sample is divided between new plants and old plants, all with at least 100 workers. Firms formed before 1976 are dropped. Sector, state, and year fixed effects are not reported. Standard errors (in parentheses) are clustered at the state level.

Note: * $p < .1$ ** $p < .05$ *** $p < .01$.

Table 8: The relationship between TFP and age shift in large and small plants: Different plant-size threshold

	(1)	(2)	(3)	(4)
Age shift in the sector-state	-7.097*	2.105	-7.891**	0.964
	(3.561)	(4.266)	(3.504)	(4.093)
Log labor			0.281***	0.277***
			(0.021)	(0.028)
Observations	33675	119171	33675	119171
Plant size	200+	1-49	200+	1-49

The dependent variable is plant-level TFP. The sample is divided between plants with below 50 (no EPL) and above 200 (EPL) workers. Sector, state, and year fixed effects are not reported. Standard errors (in parentheses) are clustered at the state level.

Note: * $p < .1$ ** $p < .05$ *** $p < .01$.

Table 9: The relationship between TFP and age shift in old and new plants: different age cutoffs

Cutoff for new/old	Coeff. for new plants	Coeff. for old plants	Prob > F
2	0.83	-8.2	0.104
3	-0.32	-8.14	0.206
4	-1.6	-8.34	0.194
5	0.34	-9.17	0.117
6	-3.04	-8.89	0.303
7	-4	-8.92	0.301
8	-4.44	-8.83	0.379
9	-2.9	-9.78	0.167
10	-3.15	-9.94	0.116
11	-2.72	-10.46	0.093
12	-2.84	-10.57	0.076
13	-3.39	-10.52	0.08
14	-3.28	-10.94	0.041
15	-3.23	-12.22	0.018
16	-2.89	-13.17	0.007
17	-2.99	-13.73	0.007
18	-3.23	-14.26	0.004
19	-3.68	-14.51	0.006
20	-4.03	-14.8	0.004
21	-5.17	-13.08	0.02
22	-5.1	-13.83	0.014
23	-5.39	-14.03	0.017
24	-5.32	-15.08	0.011
25	-5.05	-16.76	0.001
26	-5.23	-16.47	0.002
27	-5.54	-16.14	0.007
28	-5.51	-17.69	0.003
29	-5.64	-18.23	0.004
30	-5.7	-18.58	0.006

Each row of Table 9 corresponds to a regression of the same form as Table 4, but with a different cutoff for when a plant is considered new as opposed to old. See the body of the paper for more details.

Appendix A: Theory

In this appendix I provide details for several assertions made in the theory portion. Recall that e ranges from 0 to 1 and f ranges from $-1/2$ to $1/2$, and β ranges from 0 to 1. The following lemma will be useful:

Lemma: Suppose a smooth function $f(e, f, \beta)$ satisfies two conditions for all $(e, f, \beta) \in (0, 1) \times (-1/2, 1/2) \times (0, 1)$: (i) $\lim_{e \rightarrow 0} f(e, f, \beta) = 0$ and (ii) $\frac{\partial f}{\partial e} < 0$. Then $f(e, f, \beta) < 0$ for all $(e, f, \beta) \in (0, 1) \times (-1/2, 1/2) \times (0, 1)$.

Proof by contradiction: Suppose that (i) and (ii) are satisfied and there exists some $(\hat{e}, \hat{f}, \hat{\beta}) \in (0, 1) \times (-1/2, 1/2) \times (0, 1)$ such that $f(\hat{e}, \hat{f}, \hat{\beta}) \geq 0$. Then $f(\hat{e}/2, \hat{f}, \hat{\beta}) > 0$ by (ii), and $f(e, \hat{f}, \hat{\beta}) > f(\hat{e}/2, \hat{f}, \hat{\beta}) > 0$ for all $e < \hat{e}/2$, but this contracts the assumption (i).

Therefore there is a contradiction and the lemma is true. \square

Claim 1: For all $(e, f, \beta) \in (0, 1) \times (-1/2, 1/2) \times (0, 1)$,

$$\frac{\partial}{\partial e} \frac{z^*}{1 + ez^*} < 0 \quad (1)$$

Proof: We know that for $e > 0$,

$$z^* = \frac{\sqrt{\beta e + 2\beta e f + 1} - 1}{\beta e}, \quad (2)$$

so

$$\begin{aligned} \frac{\partial}{\partial e} \frac{z^*}{1+ez^*} = & [\beta^2(-(2ef+e)) + -4\sqrt{\beta(2ef+e)+1} + 4 + \\ & 2\beta \left(-e(2f+1) \left(\sqrt{2\beta ef + \beta e + 1} - 2 \right) + \sqrt{2\beta ef + \beta e + 1} - 1 \right)] / \quad (3) \\ & 2e^2 \sqrt{\beta(2ef+e)+1} \left(\sqrt{\beta(2ef+e)+1} + \beta - 1 \right)^2 \end{aligned}$$

The task remaining is to prove that this quantity is negative for all valid values of e, f , and β . I define the numerator of equation (3) as $N(e, f, \beta)$. The denominator of equation (3) is always positive, so to prove the claim, it is sufficient to show that $N(e, f, \beta) < 0$.

N is smooth, and it can be verified that $\lim_{e \rightarrow 0} N(e, f, \beta) = 0$. That means that showing $\frac{\partial}{\partial e} N(e, f, \beta) < 0$ for all $e, f, \beta \in (0, 1) \times (-1/2, 1/2) \times (0, 1)$ is sufficient to show that $N(e, f, \beta) < 0$, which in turn is sufficient to prove the claim.

$$\begin{aligned} \frac{\partial}{\partial e} N(e, f, \beta) = \\ \frac{-\beta(2f+1) \left(-4\sqrt{2\beta ef + \beta e + 1} + \beta \left(\sqrt{2\beta ef + \beta e + 1} + e(6f+3) - 1 \right) + 4 \right)}{\sqrt{\beta(2ef+e)+1}} \quad (4) \end{aligned}$$

It is still not immediately clear that this quantity is negative, so I apply the lemma again. I define the numerator of the right-hand side of equation (4) as $M(e, f, \beta)$. Similar to above, it can be shown that $\lim_{e \rightarrow 0} M(e, f, \beta) = 0$. Again, it is sufficient to show that $\frac{\partial}{\partial e} M(e, f, \beta) < 0$ to show that $M(e, f, \beta) < 0$. Taking the derivative gives

$$\frac{\partial}{\partial e} M(e, f, \beta) = \frac{-(2\beta f + \beta)^2 \left(6\sqrt{\beta(2ef+e)+1} + \beta - 4 \right)}{2\sqrt{\beta(2ef+e)+1}} \quad (5)$$

This expression is negative. The denominator is positive and the squared term in the numerator is positive. The term in parentheses in the numerator is positive as well (note that $2ef+e > 0$), and there is a negative sign applied to numerator, so the expression is negative. To wrap up, $\frac{\partial}{\partial e} M(e, f, \beta) < 0$, so $M(e, f, \beta) < 0$, so $\frac{\partial}{\partial e} N(e, f, \beta) < 0$, so $N(e, f, \beta) < 0$,

and finally, $\frac{\partial}{\partial e} \frac{z^*}{1+ez^*} < 0 \square$

Claim 2: For all $(e, f, \beta) \in (0, 1) \times (-1/2, 1/2) \times (0, 1)$,

$$\frac{\partial}{\partial e \partial f} \frac{z^*}{1+ez^*} < 0 \quad (6)$$

Proof:

$$\frac{\partial}{\partial e \partial f} \frac{z^*}{1+ez^*} = - \frac{\beta^3(2f+1) \left(3\sqrt{\beta(2ef+e)+1} + \beta - 1 \right)}{2(\beta(2ef+e)+1)^{3/2} \left(\sqrt{\beta(2ef+e)+1} + \beta - 1 \right)^3} \quad (7)$$

The denominator is positive, and the numerator is positive as well, because $2f+1 > 0$.

There is a negative sign applied to the whole fraction, so the right-hand side is negative. \square

Next, there are two predictions about the profitability of firms. Recall that V

Claim 3: For all $(e, f, b) \in (0, 1) \times (-1/2, 1/2) \times (0, 1)$,

$$\frac{\partial}{\partial e} \phi(e, f, \beta) < 0 \quad (8)$$

Proof:

$$\frac{\partial}{\partial e} \phi(e, f, \beta) = - \frac{(2bef + \beta e) \left(\sqrt{2\beta ef + \beta e + 1} - 3 \right) + 4 \left(\sqrt{2\beta ef + \beta e + 1} - 1 \right)}{2(1 - \beta)\beta^2 e^3 \sqrt{\beta(2ef+e)+1}} \quad (9)$$

We can disregard the denominator because it is positive. It is straightforward to show that $x(\sqrt{x+1} - 3) + 4(\sqrt{x+1} - 1) > 0$ for all $x > 0$, and $2\beta ef + be > 0$, so the claim is true.

Claim 4: For all $(e, f, \beta) \in (0, 1) \times (-1/2, 1/2) \times (0, 1)$,

$$\frac{\partial^2}{\partial e \partial f} \phi(e, f, \beta) < 0 \quad (10)$$

Proof:

$$\frac{\partial^2}{\partial e \partial f} \phi(e, f, \beta) = \frac{2 \left(\sqrt{2\beta ef + \beta e} + 1 - 1 \right) + (2\beta ef + be) \left(2\sqrt{2\beta ef + be} + 1 - 3 \right)}{2(\beta - 1)\beta e^2(\beta(2ef + e) + 1)^{3/2}} \quad (11)$$

The argument is exactly parallel to the one made to prove Claim 3.

Appendix B: Empirics

In appendix table 1, I present the ratings of EPL strength by state. They are based on three papers, Besley and Burgess (2004), Bhattacharjea (2008), and OECD (2007). These papers use different scales, and they have all been converted to a weak (−1), neutral (0), or strong (+1) EPL category by Gupta et al. (2009). The last column indicates the sum of the three ratings for each state, and is what I use for an overall EPL rating.

Appendix table 1 presents the EPL ratings for each state for each of the three different ratings, as well as the aggregate rating that I use throughout. See the description in the body of the paper for more information.

Appendix table 2 presents the rank of involuntary job loss rates in the United States and a group of Latin American countries. A low number indicates a low level of job loss. See the body of the paper for more information.

Appendix table B1: EPL ratings by state

State	BB	Bhat.	OECD	Aggregate
Andhra Pradesh	-1	0	-1	-2
Assam	0	0	1	1
Bihar	0	0	1	1
Gujurat	0	0	-1	-1
Haryana	0	0	-1	-1
Karnataka	-1	-1	0	-2
Kerala	-1	0	1	0
Madhya Pradesh	0	0	0	0
Maharashtra	1	1	1	3
Orissa	1	1	0	1
Punjab	0	0	0	0
Rajasthan	-1	0	-1	-2
Tamil Nadu	-1	-1	0	-2
Uttar Pradesh	0	-1	-1	-2
West Bengal	1	0	1	2

Appendix table B2: Rank of involuntary job loss rates by sector

Manufacturing sector	NIC-2004 code	<u>IJLR</u>	
		US	Latin Am.
Food manufacturing	15	1	9
Textiles	17	18	11
Clothing	18	18	11
Luggage and shoes	19	18	11
Wood products	20	6	13
Paper products	21	7.5	6.5
Publishing	22	7.5	6.5
Coke, refined petroleum	23	2	3
Chemical products	24	4	3
Rubber and plastics	25	13	3
Non-metallic mineral products	26	5	5
Basic metals	27	14.5	1
Fabricated metals	28	14.5	8
Machinery n.e.c.	29	3	-
Office and computing machinery	30	16	-
Electrical machinery	31	9	-
TV and communication	32	12	-
Medical, optical, clocks	33	20	-
Motor vehicles	34	10.5	-
Other transport equipment	35	10.5	-

Rents and Economic Growth

Daniel Schwab

Eric Werker

“Without development there is 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 and 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 and 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 and Rodan 1997) can incentivize innovation and thus bring about the economic development Schumpeter was talking about, as the economy became more sophisticated and productive.

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 et al. 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

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 economies grow slower. 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, Wallis, and Weingast (2009). Business interests can capture the state (e.g. Stigler 1971), or vice versa (e.g. Shleifer and 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 and 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, and Vishny 1993). Rent-seeking can draw talent from the productive sector and be destructive to entrepreneurship in particular (Baland and 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

¹Introducing an edited volume that applies North, Wallis, and Weingast (2009) to today’s developing countries, North et al. (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).

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.

To tackle this question, we first construct a model that allows for either productive rents or unproductive rents. Our model is a basic one to provide the intuition behind our empirical approach. A number of firms compete in Cournot competition, such that the profit of each firm is decreasing in the number of other firms in the market. Firms can either use their profits to create new products or collude to prevent new entrants to the market, and their profit-maximizing decision depends on the relative returns to each. This captures the ambiguous overall prediction of the effect of rents on growth.

We model developing countries as having three characteristics. One, a lower quality of public administration or competition policy makes it easier to bribe regulators to prevent entry. This makes the cost of rent-seeking lower in poorer countries. Two, profits on new products are higher due to the ability of firms in poor countries to copy existing technologies. These two features lead to the prediction that observed ex-post profits should be higher in developing countries, consistent with the financial literature on market segmentation (Bekaert et al. 2011). Three, credit constraints are more likely to be binding in poor countries. The combined effects result in an ambiguous prediction of whether rents are better or worse for growth in poor countries than they are in rich countries.

We test the model using the Lerner index as a measure of rents, following Nickell (1996), Aghion et al. (2005a), and Aghion, Braun, and Fedderke (2008). The Lerner index (Lerner 1934), also called a mark-up ratio, 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 (Aghion et al. 2005a).

Since firm-level data in less-developed economies is spotty and unavailable in time series for most countries, we follow Aghion, Braun, and Fedderke (2008) and use industry-level value-added data from the United Nations Industrial Development Organization (UNIDO 2013). UNIDO's INDSTAT data are available for around 20 manufacturing sectors in over 100 countries since the 1960s. The mark-up ratio we calculate is a measure of both rents and (lack of) competition (Davies 1982), and in both the theory and empirics, 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 test the predictions of the model. Unlike our predictions, which are ambiguous about the relationship between profits and growth, our results are decidedly unambiguous. First, we find support for the prediction that observed rents are higher in less developed countries—virtually any indicator of underdevelopment is associated with a higher average Lerner 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 higher political risk) 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 split the sample along two dimensions: financial sector development (as measured by the size of the banking sector relative to GDP) and the degree of external finance required by the industry (taken from Rajan and 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 this quadrant. 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). Since state-owned enterprises are important in developing countries, and they are on average less profitable and efficient than private firms (Boardman and Vining 1989, Megginson and 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 Lerner on the change in tariffs, which of course have been on a secular decline over the period of the sample. As predicted by the model, the higher the Lerner, the slower the reduction in the tariff rate. We also use data from Bloom et al. (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.

Having established that rents are associated with lower initial development levels, slower growth within manufacturing sectors, as well as a slower-improving business climate, we then test whether there are any macroeconomic implications. We include the average level of mark-up across industrial sectors as a right-hand side variable in standard growth regressions and find that it is a robust negative predictor of economic growth, although the

differential effect on poorer countries is not distinguishable from zero. A reduction in average mark-up by one standard deviation predicts higher GDP growth of 0.39 percentage points (compared to an average annual growth rate in our sample of 2.3 percent), in spite of the mark-up measure being just for the manufacturing sector. The effect of average mark-up on GDP growth is about one and a half times the size we would expect just from the direct effect of mark-up on growth in manufacturing value added, suggesting that high mark-up in manufacturing is indicative of high mark-up in non-manufacturing sectors as well. In a model allowing for conditional convergence, the growth penalty from a one standard deviation increase in mark-up is about half of the growth advantage from a one standard deviation reduction in GDP, indicating that the benefits of catch-up growth from being poor are larger than the costs of having a bad political economy.

Poor countries grow faster than rich countries because of the benefits of catch-up, but those countries also tend to have higher rents, which slows growth. Developing countries have a tailwind from being poor in the catch-up sense, but a headwind from being poor through an inferior political economy of rent-seeking business. Together, these findings allow for a more contextualized interpretation of the Knack and Keefer (1995) result that controlling for property rights variables increases rates of convergence.

Our findings are consistent with the earlier political economy literature which finds a destructive effect of rents (Ades and Di Tella 1999, Baland and Francois 2000) as well as the business literature seeking to understand how the business environment can help explain sustained rents. For example, Chacar, Newburry, and Vissa (2010) find that a stronger antitrust environment is associated with decreases in performance persistence, or sustained profits. Chari and David (2012) find that the pro-market reforms in India resulted in a decrease in firms' ability to sustain superior profits. 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 modeling of innovation, rather than being inventions per se: new plants, new products, new technologies, or getting quality accreditation. Our comprehensive results are consistent with Diwan, Keefer, and Schiffbauer (2014)'s study of crony capitalism in Egypt, which was seen by many 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.

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 also examine both mechanisms and growth effects. Unlike the business literature, the focus of our paper is not on firm profitability but instead from industry-level profitability to growth, reforms, and the overall growth of the economy. 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 presents the formal model. Section 3 describes the data and empirical specifications. Section 4 contains the main results establishing the link between rents and growth at the level of the industry. Section 5 evaluates the mechanisms of rent-seeking and managerial slack. Section 6 explores the growth implications. Section 7 concludes.

2 A simple model

In this section we develop a simple model to illustrate the tradeoff between rent-seeking and growth. It is not meant to capture all of the complexities of competition, especially in

the developing world; rather, it should be taken as illustrative. We model rents or mark-up as determined by the level of competition within a market. Rent-seeking is the attempted blocking of a new entrant into the market by bribing or lobbying bureaucrats, and it is easier when the level of development is lower. Probabilistic entry is similar to the model in Aghion et al. (2005b), although they focus on entrance of foreign firms. Growth occurs when a firm innovates to produce a new product, which generates an increased incentive for a firm in a more competitive environment to innovate to leave the competition behind, as in Arrow (1962).

2.1 Set-up

In the first period, N identical firms compete in quantities, producing a homogenous good with inverse demand given by $P(Q) = f - gQ$, where Q is the total quantity produced and f and g are positive parameters. Marginal cost is constant at c , and $f > c$. The total profit generated by the firms is

$$\pi(N) = \left(\frac{f-c}{N+1}\right)^2 \left(\frac{N}{g}\right). \quad (1)$$

Mark-up of price over marginal cost is higher when N is lower, so high N in the theory corresponds to low mark-up in the empirical section. In between the first and second period, a potential innovator will have the option to pay a fixed cost h to leave the original market (market A) and create a new product, allowing it to operate as a monopolist in market B , earning a profit of π^M . The firm may be prevented from undertaking a profitable innovation by a credit constraint; it may spend only the profits it earns in the first period and an exogenous level of credit λ .

Whether or not the firm decides to innovate corresponds to productivity growth in the empirical section.² If the potential innovator decides to stay in market A , the incumbents

² As will be discussed below, productivity is measured in revenue terms, rather than physical terms.

then collusively decide on a level of rent-seeking, which reduces the likelihood of an additional competitor in the second period. If they collectively spend a , then the probability of an additional firm entering is $1 - \alpha\sqrt{a}$, with $\alpha > 0$ an indication of how easy it is to persuade bureaucrats to restrict entry. Note that the credit constraint can never bind here: the firms never want to spend more on rent-seeking than they earned in profits in the first period, because that would guarantee losses.

If an entrant does not arrive, the incumbents will once again earn $\pi(N)$ in the second period. If one does, they will earn

$$\pi(N+1) * \frac{N}{N+1} = \left(\frac{f-c}{N+2}\right)^2 \left(\frac{N}{g}\right). \quad (2)$$

For convenience, we define the reduction in total profits for the N incumbents caused by entry as $d(N)$:

$$d(N) \equiv \pi(N) - \pi(N+1) * \frac{N}{N+1}. \quad (3)$$

It is natural to treat N as a discrete variable, but we can also consider it as a continuous variable when analyzing the effect of a change in N . Note that the reduction in profits caused by one additional entrant is declining in N (because moving from a monopoly to a duopoly reduces profits far more than moving from 12 firms to 13):

$$d'(N) = -\frac{(f-c)^2}{g} \left(\frac{N-1}{(N+1)^2} - \frac{N-2}{(N+2)^2} \right) < 0. \quad (4)$$

The solution concept is symmetric Nash equilibrium. This allows us to disregard im-

Creating a new market is only one way to generate more value per worker, but it is an important one, especially in poor countries where relatively few different kinds of goods are being produced (Hidalgo and Hausmann 2009).

plausible equilibria where, for example, the incumbents could threaten to produce huge quantities in the second period if the potential innovator did not leave the market, or where one incumbent paid less than their share of the rent-seeking. It also allows us to ignore what happens in market A if the innovator leaves, because the outcome of interest is the innovation itself. Working backwards, if the innovator stays in market A , the incumbents' total profits in the second period as a function of rent-seeking will be

$$\pi(N, a) = \pi(N) - d(N) * (1 - \alpha\sqrt{a}) - a. \quad (5)$$

Solving the first-order condition gives

$$a^* = \left(\frac{1}{2}\alpha d(N)\right)^2, \quad (6)$$

and

$$\pi(N, a^*) = \pi(N+1) * \frac{N}{N+1} + \frac{1}{4}\alpha^2 d(N)^2. \quad (7)$$

Thus, the firm will innovate if the fixed cost is less than the additional profits that are created:

$$h \leq \pi^M - \frac{\pi(N, a^*)}{N} \equiv h^{want}, \quad (8)$$

and it has sufficient credit:

$$h \leq \frac{\pi(N)}{N} + \lambda \equiv h^{can}. \quad (9)$$

We can interpret h^{want} as the highest fixed cost where innovation is still profitable and h^{can} as the highest fixed cost where innovation is feasible given the credit constraint. To ensure that $1 - \alpha\sqrt{a}$ remains above zero, it is sufficient to assume that $\alpha < \sqrt{2d(N)}$. The purpose of this assumption is just to guarantee an interior solution so that we don't have to keep track of the possibility that rent-seeking drives the probability of entry to zero.

2.2 Comparative statics

2.2.1 Profits and development

In considering the relationship between observed profits and the development of a country, we analyze the situation where the potential innovator stays in the original market; innovation is generally difficult so this is more likely to be the relevant case. The change in total profits as a function of α can be found from equation (7):

$$\frac{\partial \pi}{\partial \alpha} = \frac{1}{2} \alpha d(N)^2 > 0. \quad (10)$$

Therefore we expect that profits will be higher in countries with lower quality of government institutions (higher α), consistent with the political risk premium demanded by investors in such jurisdictions, and this result is confirmed in the data.

2.2.2 Productivity growth and competitiveness

An exogenous increase in the number of firms has two effects: it is less profitable for the potential innovator to stay in market A , which encourages it to flee from competition and create the second market, but it also reduces profits in the first period, which may prevent it from doing so. The result is that without appealing to the data, we cannot make any predictions of the effect on growth of increasing N and reducing mark-up.

2.2.3 Productivity growth and development

There are three relevant differences between rich and poor countries in the model. First, we assume that rent-seeking is easier in poor countries; this is motivated by the fact that corruption is generally decreasing with development. When rent-seeking is easier (i.e., α is higher), the potential innovator has less incentive to create a new market, increasing the maximum fixed cost it would be willing to incur:

$$\frac{\partial h^{want}}{\partial \alpha} = -\frac{\partial \pi(N, a^*)}{\partial \alpha} \frac{1}{\alpha} \quad (11)$$

$$= -\frac{\alpha d(N)}{2N} < 0 \quad (12)$$

Second, there is unconditional convergence in manufacturing productivity because of the availability in catch-up growth (Rodrik 2013). To incorporate that fact into the model, we assume that the profits generated by an innovator (π^M) are higher in poor countries, because in poor countries it is possible to copy existing technology to create a new market. Increasing π^M raises the threshold fixed cost that the innovator is willing to pay, making productivity growth more likely, because $\frac{\partial h^{want}}{\partial \pi^M} = 1$.

Finally, the credit constraint may be more likely to bind in poor countries because there is less access to financial markets, which would make innovation more difficult. Two of these effects indicate that productivity growth should be lower in poor countries, and one suggests that it should be higher, but we cannot predict which side dominates without making further assumptions.

2.2.4 Productivity growth and the interaction of competitiveness and development

The differential effect of rent-seeking in rich and poor countries is a central question of this paper. In the model, with tension built in between the temptation to rent seek and the reward to innovate, we have ambiguous predictions for each of competitiveness and development on productivity growth, and not surprisingly we also generate an ambiguous prediction on the cross partial without further assumptions.

3 Data and Empirical Specifications

3.1 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 former. The first is rents, or excess profits, defined as a fraction:

$$\text{rents} = \frac{\text{revenue} - \text{total cost}}{\text{revenue}}. \quad (13)$$

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

$$\text{Lerner} = \frac{\text{price} - \text{marginal cost}}{\text{price}}. \quad (14)$$

In practice, we do not observe the data that would be required to calculate either rents or the Lerner index exactly, but we can use the data that is available to generate a good approximation. Following Aghion, Braun, and Fedderke (2008), we define mark-up as follows,

$$\text{mark-up} = \frac{\text{value added} - \text{wage bill}}{\text{revenue}}. \quad (15)$$

Since value added is revenue minus cost of materials, and total variable cost is the sum of the wage bill and the cost of materials, we can also write,

$$\text{mark-up} = \frac{\text{revenue} - \text{total variable cost}}{\text{revenue}}. \quad (16)$$

The only discrepancy between our definition of mark-up and rents is that rents should also subtract depreciation of capital, so mark-up will be higher than true rents. We have access to data on capital expenditures, but not to capital stocks. We do use sector fixed

effects throughout, so to the extent that depreciation as a fraction of output is similar across countries within the same manufacturing sector, we can address this concern. Also, as a robustness check, we proxy for depreciation using a lagged average of capital expenditures and the main results hold.

To see how our definition of mark-up approximates the true Lerner index, substitute average variable cost for marginal cost, and multiply the numerator and denominator by the quantity sold:

$$\text{Lerner} \approx \frac{\text{price} * \text{quantity} - \text{average variable cost} * \text{quantity}}{\text{price} * \text{quantity}}, \quad (17)$$

which is exactly the definition of mark-up in (16). 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. Disentangling the two remains on our agenda for future research.

Productivity is defined by

$$\text{productivity} = \frac{\text{value added}}{\text{employees}}, \quad (18)$$

and

$$\text{productivity growth in year } t = \frac{\text{productivity in } t - \text{productivity in } t - 1}{\text{productivity in } t - 1} \quad (19)$$

Note that productivity growth, our main dependent variable of interest, is defined as the change in the value added per worker, rather than the change in the number of units created per worker. In some ways, this is desirable, as a change from low quality coffee beans to gourmet beans will show up as a change in our definition of productivity, but it would not if we only focused on physical productivity. The downside to our approach is that value

added also reflects market power, so that an identical pound of coffee beans is reported as different levels of value added if the producer is a monopolist instead of a competitive firm, and productivity growth may also reflect a change in competitiveness (Foster, Haltiwanger, and Syverson 2008).

Many papers in the literature define productivity growth as improvement in total factor productivity; that is, the change in the output that could be generated from a given quantity of labor and capital. We cannot use the same measure because we lack data on capital, but we do not view this as a limitation. If firms are diverting firms away from capital investment in favor of wasteful rent-seeking, that would show up as no change in total factor productivity but a decrease in productivity, and this is exactly the kind of damage from rent-seeking that we are interested in.

3.2 Empirical Specifications

In the primary specification of the model, we assume that productivity growth from time $t - 1$ to t is a function of lagged mark-up, log GDP per capita, 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

$$P_{ijt} = \beta_1 M_{ijt-1} + \beta_2 Y_{it-1} + \beta_3 M_{ijt-1} * Y_{it-1} + u_{ijt}, \quad (20)$$

where i indexes countries, j indexes manufacturing sectors, and t indexes time. We assume that the error term is uncorrelated with the independent variables, so

$$E[M_{ijt}u_{ijt}] = E[Y_{it}u_{ijt}] = E[M_{ijt}Y_{it}u_{ijt}] = 0. \quad (21)$$

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 (20), meaning that a spurious negative

correlation would arise even in the absence of causality. In our main specification, we instrument for mark-up in period $t - 1$ with mark-up in the same country-sector in period $t - 2$.

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.

3.3 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 value added, output, wages (all reported in current US dollars), and employees, which are converted to constant (year 2000) values by multiplying by the US real GDP and dividing by the nominal US GDP, as reported in the World Development Indicators (WDI). 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 C1 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, and Kindred 2012), the average mark-up in 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, provides a number of

other variables we use. They include the cost of starting a business, which is an indication of the healthiness of the business environment, and is available from 2003 to 2010. There are also controls used in the growth regressions, which cover from 1970 to 2010: government consumption as a percentage of GDP, gross enrollment in secondary school, population growth, and life expectancy. Finally, the WDI is the source for GDP per capita (in 2000 USD), and for M2 as a percentage of GDP, which we take as a proxy for the development of the financial sector, both of which are available from 1963 to 2010.

The International Country Risk Guide (ICRG) provides data on political and social attributes from 1984 to 2010, compiled by experts in each country (PRS Group 2012). Military in politics, corruption, and bureaucratic quality are three components of the political risk rating, which we relabel political stability for clarity. In all cases, a higher number is better, so we relabel their variable corruption as “lack of corruption,” etc. These variables are rescaled from 0 to 1.

3.4 Summary Statistics

In table 10 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 model 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, consistent with the catch-up growth we model and 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 Hausmann and Hidalgo (2009) in which richer countries produce more products. Corruption, political stability, wealth, and

Table 10: 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
Lack of corruption	0.446	0.167	0.698	0.209
Political stability	0.584	0.113	0.781	0.102
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.

productivity fall in normal ranges and vary across the two samples as would be expected.

3.5 Correlates of mark-up

Our model predicts higher mark-up in less developed countries. We model development as the ease of bribing a bureaucrat to prevent the entry of a new firm. This measure correlates with income per capita but 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 11 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). There 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), 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 11: Correlates of Mark-up

	(1)	(2)	(3)	(4)	(5)	(6)
Log GDP per capita	-0.011 (-2.60)	0.001 (0.18)	0.002 (0.27)	0.002 (0.35)	-0.011 (-1.75)	-0.004 (-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. Standard errors are robust and clustered at the country level. See text for sources.

4 Results

4.1 Primary specification

The results for the primary (IV) specification and two comparison OLS regressions are in table 12.

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 $t - 1$,³ as discussed above, and the fourth and fifth columns are OLS.

³ To test for a weak instrument, we consider the Kleibergen-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

Table 12: 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 (-9.10)	-0.322 (-9.17)	-0.314 (-9.60)	-0.498 (-13.12)	-0.240 (-10.11)
Log GDP per capita		-0.033 (-1.44)	-0.031 (-1.39)	-0.034 (-1.52)	-0.031 (-1.35)
Mark-up x log GDP			0.058 (2.71)	0.099 (4.00)	0.037 (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.

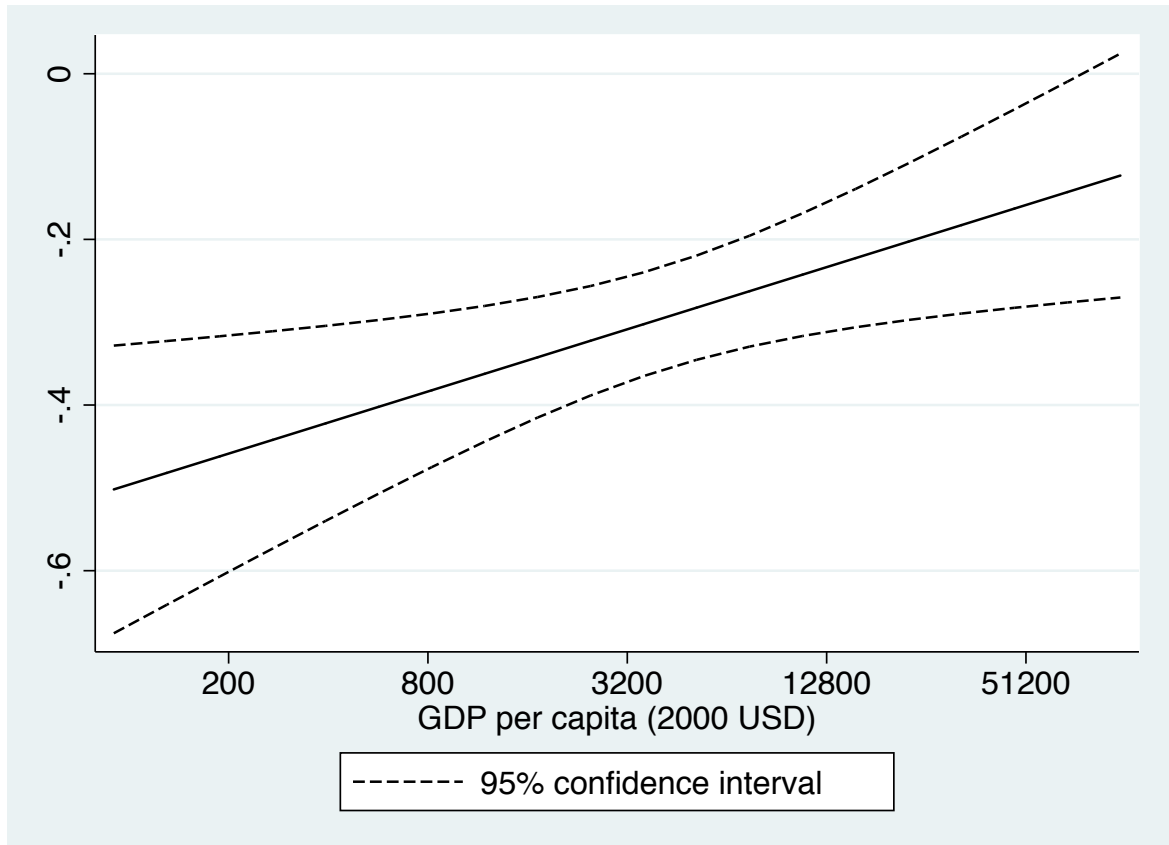
In the first model, mark-up is the only independent variable, along with fixed effects for country, sector, and year. In the second, real log GDP per capita is included as a control, to ensure that mark-up is not just a proxy for low GDP. Mark-up is strongly significant and is negatively correlated with 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.

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 over 300. The large F-statistics suggest that measurement error may be a relatively minor concern.

The interaction term here has been demeaned, or defined as $(\text{mark-up} - \text{mean mark-up}) \times (\text{log GDP} - \text{mean log GDP})$, 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 and 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 .12, with a mean of .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

Figure 1: Marginal effect of mark-up on productivity growth by income



Note: The dependent variable is the marginal effect of mark-up on productivity, evaluated at different income levels. It is based on column 3 of table 12.

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.

In figure 1, we show the marginal effect of mark-up on productivity growth at different income levels, based on our preferred specification (column 3), with a 95 percent confidence interval. Here we see graphically that mark-up is damaging at all income levels (except possibly at the very highest levels), and more so at lower incomes.

4.2 Robustness

4.2.1 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, Braun, and Fedderke (2008) find. They write that the opening of the economy to trade, the degree of tradability of the 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 Hellman (2014) which follows Berry, Levinsohn, and Pakes (1995) and others that instrument with local market conditions. 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.⁴ 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 table 13. 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

⁴We thank Sam Peltzman for suggesting this possibility.

Table 13: Instrumenting for mark-up with mark-up in other sectors

	(1)	(2)	(3)	(4)
	1st: mark-up	1st: inter.	IV	IV
Mark-up			-0.369 (-2.54)	-0.519 (-3.02)
Mark-up x log GDP				0.335 (2.96)
Mark-up other sectors	0.662 (16.77)	0.207 (2.83)		
Other mark-up x GDP	0.076 (3.40)	0.550 (9.56)		
Log GDP per capita	-0.005 (-0.83)	-0.008 (-0.66)	-0.034 (-1.47)	-0.027 (-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 12. 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.

log GDP is highly predictive of mark-up x log GDP. The Kleibergen-Paap F statistic is 24.8, 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.

4.2.2 Alternate fixed effects

In our primary specification, we have fixed effects for year, country, and sector. In the first two columns of appendix table C2, we remove one fixed effect, so that we have year and sector fixed effects and then year and country fixed effects. Removing the country fixed effects moderates the coefficient on mark-up from about -0.3 to -0.2, and on the interaction term from about 0.06 to 0.045. This is an indication that the relationship between mark-up and productivity growth is strongest when looking within a particular country, but the relationship is still evident when all countries are lumped together. Removing the sector fixed effects has surprisingly little impact on our estimated coefficients.

We also consider specifications with finer-grained fixed effects. With country-sector and year fixed effects, we are comparing French clothing manufacturing to French clothing manufacturing at different dates, but not two different French manufacturing sectors or two different countries. The coefficient on mark-up approximately doubles to -0.6, and the interaction term also becomes stronger, with a coefficient of 0.11, further evidence that the relationship between mark-up and productivity growth is strongest when looking within countries rather than between them. If we use country-year and sector fixed effects, meaning we compare data from Chile in 1980 but not across countries or years, the coefficient on mark-up is about the same as the primary specification, at -0.31, while the coefficient on the interaction is only 0.037. In every case, there is significance on both.

One advantage to this strategy is that we can rule out the possibility that all of the change in productivity growth is being driven by variation at the country-year level. For example, it is not possible that our results are being driven entirely by the business cycle (or any other

national-level panel data variable), because then mark-up would have no relationship with productivity growth when country-year fixed effects were included.

In the final column we remove all fixed effects entirely and the broad results hold.

4.2.3 Alternate variables and longer horizons

In appendix table C3, we examine the robustness of the results to replacing log GDP per capita with ICRG's political stability measure and productivity growth with growth in value added. Political stability and GDP per capita are closely correlated ($\rho = 0.76$), which is not surprising as rich countries tend to have more stable, better functioning governments. Political stability is probably a better proxy for the feasibility of rent-seeking than wealth, but GDP per capita has better coverage, so that is what we use in the main specification.

Growth in value added, also defined in percentage terms, is a useful check because we want to make sure that we are really capturing a positive outcome when productivity growth increases. If the output of a manufacturing sector drops by 50% and the employment drops by 75%, then we would say that productivity doubled, even though it is questionable whether the economy is really in a better position, especially in light of the fact that productivity in the manufacturing sector tends to be higher than in the overall economy (Rodrik 2013).

In the fifth column, we consider annualized productivity growth over a five-year period to allow for the possibility that it takes time for a less competitive business environment to affect growth. We use a non-overlapping specification starting including 1964, 1969, etc., with the starting year chosen to maximize the sample (results are similar for different starting years).

Table C3 shows that all of the main results hold when making these substitutions. The only exception is that the interaction term of mark-up x political stability is not significant when the dependent variable is productivity growth.

4.2.4 Alternate samples and Lerner

In appendix table C4 we examine the robustness of the results to dropping different subsamples of the data. First, our story is fundamentally about domestic business markets, and one may be worried that the data combine multinational activity with domestic manufacturing. We looked at the OECD statistics of outward value added activity by American and Japanese multinationals (the only countries that provided these data) in manufacturing and isolated the four categories with the highest share of multinational activity: electronics, motor vehicles, chemical products, and precision manufactures. In column 1 we drop these four industries and the results are barely changed, indicating that our results are driven by limitations on domestic entry, not just on foreign firms. Second, our model expects some underlying level of competition, but the sample includes small countries like Lesotho (with three years of observations) that may simply not offer the depth for competitive dynamics to result. In column 2 we drop the ten percent of the observations coming from the countries with the smallest population. The results are barely changed. Third, the construction of the competition measure is at the level of the country-industry, but in fact many firms compete with other firms from different countries in export markets. A Herfindahl measure would have been much more inappropriate given this dynamic than the Lerner is, but it is still interesting to examine whether the results are driven by the non-tradeables. In column 3 we examine export shares by country-industry-year and drop the most traded half of the sample. The results hold.

Next, in column 4 we redefine the Lerner index to subtract out our best measure of depreciation, which we define rather crudely as the average of the prior 5 years of capital expenditures. Again, the results hold.

In one final unreported robustness check, we confirm that dropping the three natural resource manufacturing sectors leaves the coefficients of interest essentially unchanged.

4.3 Credit Constraints

The evidence thus far indicates that high rents slow productivity growth, particularly in poor countries, but perhaps we can find subsamples where they are beneficial. We are looking for a case where rents could alleviate credit constraints, which is most likely in a sector where finance 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 characterize sectors by the amount of capital they require, and we split the 18 sectors into the nine with more and less capital required. Their argument (and ours) is that dependence on finance by US firms captures technological differences between sectors. Our proxy for financial market development is M2 as a fraction of GDP, from the WDI (2013), and we split countries based on their overall average M2/GDP, so that a country does not move between categories in different years.

Table 14 gives the results of our primary specification broken down into the quadrants of high and low finance required and financial market development. If rents can be useful, we would expect mark-up to be least damaging where finance is important and difficult to acquire (column 3) and most damaging in the opposite case (column 2). In fact, we see the reverse, where the coefficient on mark-up is most negative is column 3 and least negative in column 2. We cannot say that mark-up is most damaging in column 3, as the coefficient is very close to the one in column 4 when external finance needs are also high but financial market development is better, but we can reject the hypothesis that the coefficients in columns 2 and 3 are equal at the 1% confidence level. We interpret this as evidence that barriers to entry raise the returns for incumbents to keep a sector uncompetitive. This is consistent with our model, 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 don't need to grow at all.

5 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 rents. Tariffs are not a direct measure of barriers to entry of other domestic firms, but they do affect the cost of imported substitutes.

Table 14: Mark-up and productivity growth in sub-samples

	(1)	(2)	(3)	(4)
Mark-up	-0.351 (-7.50)	-0.196 (-4.77)	-0.457 (-5.58)	-0.446 (-5.17)
Log GDP per capita	-0.082 (-2.31)	0.053 (1.68)	-0.086 (-2.51)	0.022 (0.94)
Finance required	Low	Low	High	High
Financial sector development	Low	High	Low	High
Observations	8956	8242	8903	8814
R-squared	0.108	0.156	0.121	0.224

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 M2/GDP is from WDI (2013). There are fixed effects for country, year, and sector. Standard errors are robust and clustered at the country level.

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.

5.1 Tariffs

Tariff rates have been falling on average, 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). Tariffs are weighted averages, either for the actual tariffs applied (labeled AHS in the dataset), or for the tariffs applied to countries without a special trade agreement (MFN). Both cover the years between 1988 and 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 1% of values on both ends for the change in tariff rate are replaced with the 1st and 99th percentile values to reduce the influence of outliers, and the results of these regressions are in table 15. 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.

The coefficients on mark-up in both regressions are significant and positive in both regressions. The positive sign on the interaction term (significant in one pair of regressions but not the other) is a puzzle, because it says that mark-up prevents tariff reduction more strongly in rich countries than in poor countries, which is the opposite of what we would expect. We suspect this may be driven by nontariff barriers being the protectionist vehicle

of choice in poor countries (Michalopoulos 1999).

Table 15: Mark-up and change in tariffs

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

Note: t-values in parentheses. The dependent variable is percentage change in tariff levels, either effective (AHS) or the rate for countries with no special agreement (MFN). Tariff data is from UNCTAD (2013). 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.

5.2 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 (we leave 2010 out of the regression because the manufacturing data for 2010 is incomplete). 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 16, 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.

Table 16: Management, mark-up and productivity growth

	(1) Prod. growth	(2) Prod. growth	(3) Management score
Mark-up	-0.533 (-0.28)	-0.532 (-0.28)	0.333 (1.66)
Log GDP per capita	2.000 (6.37)	2.000 (6.64)	0.049 (1.06)
Management score		-0.009 (-0.04)	
Observations	115	115	440
R-squared	0.296	0.296	0.617

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.

6 Growth

We have found that high rents or mark-up is damaging to growth within a manufacturing sector, and the level of mark-up in other manufacturing sectors in a country positively predicts the level of mark-up in a manufacturing sector and negatively predicts the growth in that sector. But manufacturing is a relatively small share of the economy, particularly in most developing countries (Rodrik 2013). What goes on in manufacturing may have no overall bearing on the economy at large. This may be especially the case for the formal-sector firms that are picked up in the UNIDO data. After all, Rodrik (2013), with the same UNIDO dataset, documents unconditional convergence in manufacturing but does not find evidence for the rest of the economy.

However, the rent-seeking channel that this paper models and attempts to test is a political economy channel that we have no reason to believe is only expressed in formal manufacturing. For example, protectionist measures in Liberia limit access to such sectors as video clubs and tire repair shops (U.S. Department of Commerce 2012), and in the Philippines to recruiting firms and radio stations (Werker et al. 2013).

We take a simple average of the mark-up ratios in each of the manufacturing sectors and, using data from the WDI, include it as a control variable in the standard growth regressions: a non-overlapping ordinary least squares (OLS) Barro growth regression as well as an overlapping generalized method of moments (GMM) model. Table 17 presents the results. The first column includes only the average mark-up and initial GDP per capita, and the effect of the mark-up is negative and significant; the coefficient of -0.047 means a one standard deviation decrease in average mark-up is predicted to add 0.38 percentage points of growth. In contrast, the coefficient on initial income is essentially zero, consistent with a lack of unconditional convergence in the sample. The next column includes mark-up as well as some of the usual suspects in the growth regressions: government spending, school

enrollment, population growth, and life expectancy. These are chosen to follow the regressors used by Bekaert et al. (2005), who in turn is following Barro (1997), all with the aim of reducing the possibility of data-mining. With the other controls in column 2, the coefficient on initial income is now negative and significant, consistent with conditional convergence. The coefficient on mark-up remains statistically significant and almost unchanged in size at -0.049, meaning a one standard deviation decrease in average mark-up leads to a 0.39 percentage point improvement in GDP growth. Among the other variables, population growth is significant and negative, and life expectancy is positive. In four additional unreported regressions, we included one of the four control variables along with mark-up, and mark-up remains statistically significant in all cases, with a coefficient ranging from -0.038 to -0.054.

The growth penalty from a one standard deviation increase in mark-up is about half of the growth advantage from a one standard deviation reduction in GDP, indicating that the benefits of catch-up growth from being poor are larger than the costs of having a bad political economy. Column 3 includes an interaction term between mark-up and initial income to test whether the effect of rents is worse for growth in poorer countries. The coefficient is positive, suggesting that the effect may be worse in poor countries, but the coefficient is imprecisely measured.

Columns 4 and 5 are similar to 2 and 3, except we use GMM, following Caselli, Esquivel, and Lefort (1996). OLS estimates may be inconsistent due to correlated individual effects and endogenous explanatory variables, for which the method in Caselli, Esquivel, and Lefort (1996) (and used by many researchers since then) is designed to correct. The results are very similar to the OLS. The countries exhibit conditional convergence, with a negative and significant coefficient on initial wealth. The effect of the average mark-up remains negative and significant. The interaction term between wealth and mark-up in column 2 is also positive but statistically insignificant.

Table 17: GDP growth and mark-up: non-overlapping OLS and overlapping GMM

	(1)	(2)	(3)	(4)	(5)
	OLS	OLS	OLS	GMM	GMM
Average mark-up	-0.047 (-2.63)	-0.049 (-2.45)	-0.050 (-2.45)	-0.044 (-2.59)	-0.044 (-2.60)
Initial log GDP pc	0.001 (0.78)	-0.006 (-3.10)	-0.006 (-3.06)	-0.004 (-2.45)	-0.004 (-2.36)
Gov spending/GDP		-0.145 (-0.52)	-0.121 (-0.41)	-0.203 (-0.84)	-0.191 (-0.78)
Sec school enrollment		-0.010 (-0.09)	-0.015 (-0.14)	-0.001 (-0.01)	-0.000 (-0.00)
Population growth		-0.003 (-1.98)	-0.003 (-2.12)	-0.003 (-2.35)	-0.003 (-2.39)
Log life expectancy		0.057 (2.49)	0.058 (2.52)	0.046 (2.18)	0.046 (2.19)
Avg mark-up x log GDP pc			0.014 (0.88)		0.011 (0.78)
Observations	420	326	326	1586	1586
R-squared	0.023	0.087	0.090	0.069	0.071

Note: t-values in parentheses. See text for sources. The dependent variable is growth in GDP per capita. In the first three columns, we consider non-overlapping 5-year periods, and use OLS. In the last two, we consider overlapping 5-year periods with GMM. There are no fixed effects. Standard errors are robust and clustered at the country level.

Taken together, these results suggest that the rents we measure in the manufacturing sector may be indicative of an overall environment in which incumbents rent-seek to prevent challengers, which lowers the overall growth of the economy. Whether or not we control for initial income, countries in which businesses are able to maintain high profit margins grow slower, in spite of the fact that these countries might otherwise enjoy some of the “advantages of backwardness” (Gerschenkron 1962) consistent with a lower level of economic development. Of course income and mark-up are inversely correlated. That higher mark-ups are more prevalent in poor countries may explain why convergence has

not generally been a feature of economic development (Pritchett 1997). We find that developing countries are both poor (which provides a growth boost, once some other factors are controlled for) but also have a higher mark-up, indicative of a worse political economy of business (which provides a growth drag).

It may be the case, as suggested in North et al. (2012, p. 9), that many non-converging developing countries are politically “held together” by their rents even as those same rents prevent them from generating prosperity for their non-elites. Indeed, North, Wallis, and Weingast (2009) argue that rents are useful for political stability whereas we have restricted our attention to productivity and economic growth as outcome variables that proxy for welfare. We re-ran the analysis in table 17 but with political stability as the outcome variable. The results (available from the authors on request) are inconclusive: the coefficient of the association between mark-up and political stability appears to be slightly positive when income per capita is under \$1000, and otherwise negative, but the standard errors are so large that they do not rule out the opposite result.

7 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, Wallis, and Weingast (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. Higher rents are associated with a slower removal of tariffs, indicative of the channel in

our model: 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. Finally, we find that countries with more rents in the manufacturing sector grow slower, even when other controls are introduced.

Ideally, this paper would have looked at other measures of rents, or excess profits, like return on assets and return on equity. Unfortunately, the data coverage for these variables is much weaker, particularly across time in the less developed countries which are our focus in this paper. That said, this remains an avenue for future work. Another important research question that this paper leaves unanswered is what allows a country to be able to escape from an equilibrium of limited but non-expanding profits (Pritchett and Werker 2012).

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Appendix C

Table C1: 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

Table C1 (cont.)

Country	Years	First	Last	Country	Years	First	Last
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
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

Table C2: Alternate fixed effects

	(1)	(2)	(3)	(4)	(5)
Mark-up	-0.202 (-6.44)	-0.242 (-8.57)	-0.640 (-9.29)	-0.307 (-9.78)	-0.170 (-6.02)
Mark-up x log GDP	0.045 (2.14)	0.059 (2.61)	0.112 (2.30)	0.037 (1.82)	0.037 (1.78)
Log GDP per capita	-0.006 (-2.30)	-0.030 (-1.31)	-0.036 (-1.56)		-0.005 (-1.75)
Country FE	No	Yes	No	No	No
Sector FE	Yes	No	No	Yes	No
Year FE	Yes	Yes	Yes	No	No
Country-sector FE	No	No	Yes	No	No
Country-year FE	No	No	No	Yes	No
Observations	35031	35031	35031	35031	35031
R-squared	0.086	0.110	0.173	0.340	0.019

Note: t-values in parentheses. 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. Standard errors are robust and clustered at the country level.

Table C3: Alternate variables

	(1)	(2)	(3)	(4)	(5)
	Prod.	Prod.	VA	VA	5-yr Prod.
Mark-up	-0.314 (-9.60)	-0.343 (-7.58)	-0.325 (-10.46)	-0.346 (-8.01)	-0.265 (-7.80)
Log GDP per capita	-0.031 (-1.39)		-0.054 (-1.98)		-0.065 (-1.98)
Mark-up x log GDP	0.058 (2.71)		0.082 (3.63)		0.068 (3.24)
Political stability		0.000 (0.10)		-0.002 (-1.78)	
Mark-up x pol. stab.		0.004 (1.34)		0.008 (2.88)	
Observations	35031	17789	41816	21057	5799
R-squared	0.119	0.131	0.118	0.125	0.248

Note: t-values in parentheses. The dependent variables (productivity and value added growth) are measured from time $t - 1$ to t . We instrument for mark-up in $t - 1$ with mark-up in $t - 2$. In the fifth column, productivity growth is annualized growth from $t - 1$ to $t + 4$ (non-overlapping). The unit of observation is a country-sector-year. Political stability is ICRG's "political risk rating," with high numbers representing stronger political systems (PRS Group, 2012). There are fixed effects for country, year, and sector. Standard errors are robust and clustered at the country level.

Table C4: Other robustness checks

	(1) Less MN	(2) Drop small	(3) Less traded	(4) Cap exp.
Mark-up	-0.291 (-8.79)	-0.315 (-9.37)	-0.265 (-7.96)	-0.203 (-5.29)
Mark-up x log GDP	0.062 (2.84)	0.054 (2.31)	0.053 (2.48)	0.076 (3.78)
Log GDP per capita	-0.030 (-1.14)	-0.028 (-1.19)	-0.033 (-1.37)	-0.041 (-1.70)
Observations	31702	27454	17499	18451
R-squared	0.119	0.121	0.111	0.119

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. In column one we remove the four sectors with the highest multinational presence. In column two we drop the smallest 10% of countries. In column three we consider the less traded sectors. In column four we redefine mark-up by subtracting a five-year moving average of capital expenditures. The unit of observation is a country-sector-year. There are fixed effects for country, year, and sector. Standard errors are robust and clustered at the country level.

How Do Aid Inflows Affect the Likelihood of Conflict in Poor Countries?

This chapter is based on work with Faisal Ahmed and Eric Werker.

Does foreign aid promote peace in the country that receives it? In most of the theoretical and empirical literature, scholars have found foreign aid generates civil conflict, similar to a natural resource curse. On the other hand, Ahmed, Schwab, and Werker (2016) present empirical evidence that autocrats who receive foreign aid can use the additional resources to hold on to power, and deter insurgencies. This result runs counter to the predictions of the model in Besley and Persson (2009). In this chapter, we extend that model to accommodate the possibility that foreign aid prevents civil conflict.

In Besley and Persson (2009), when a state has a low level of resources¹ it will be at peace, when it has an intermediate level of resources there will be repression, and at a high level of resources there will be two-sided civil conflict. The extension presented in this chapter to that model is to allow the incumbents to choose between two different levels of violence in response to an insurgency. This creates a new possible ordering of outcomes, which is that as aid moves from zero to infinity, the recipient experiences peace, then repression, then civil conflict (as in the Besley-Persson model), but then a reversal back to repression, and then civil conflict again.

The intuition behind the result is that increasing the amount of aid a country receives has two effects. The first is that it makes the prize for a successful insurgency larger. The second is that it may provide the resources necessary for the incumbents to respond to an insurgency with a greater level of violence, which deters insurgency. The reversal back to repression from civil conflict occurs because the incumbent has

¹ These resources can come from aid, natural resources, or other sources.

sufficient incentive to use the maximum response. As aid grows even larger and the prize of holding power rises, the first effect will dominate the second, and the opposition will mount an insurgency even though it will face the most violent response.

In Ahmed, Schwab, and Werker (2016), we argue that foreign aid decreases civil conflict, and raises repression. This finding suggests that in the ranges of aid that we observe, the first effect dominates, meaning that the increased resources available to the state primarily serve to deter insurgency.

There is a rich theoretical literature on the economic causes of civil conflict, where the opposition attempts a coup if expected benefits exceed expected costs; Grossman (1995) and Collier and Hoeffler (1998) are seminal contributions. More specifically, there has been a focus on the relationship between civil conflict and unearned income from aid or natural resources.²

Collier and Hoeffler (2000) and Morrison (2007) present models where additional resources create civil conflict by increasing the value of power. This is analogous to the ordering of violence outcomes as in Besley and Persson (2009), where there is no possibility of non-monotonic relationship between aid and violence. The ability of the incumbents using aid revenues for the purpose of political repression is a common feature in models of aid and political outcomes. In Bueno de Mesquita and Smith (2009), aid can be used to prevent the opposition from protesting, which increases the incumbent's probability of holding power.

² Feyzioglu et al. (1998) and Morrison (2007) show that wealth from foreign aid and natural resources has many of the same properties.

Section 2 sets up the model, and section 3 characterizes agents' optimal strategies. Section 4 concludes.

2. Model Set-up

The model closely follows Besley and Persson (2009). There are two groups of equal size in a population normalized to 1 with wage w . Time is infinite, though since there are no state variables we model a single generation which is equivalent to any other. One group, the incumbent (denoted Inc) is in power, while the other group (Opp) is in the opposition. Being in power allows the incumbent to favor its own when distributing state resources R which can be thought of as a rentier's unearned income from natural resources and aid. The incumbent is restricted in how much it can favor its own population: when it gives a transfer of 1 to its own population, it must give $\sigma \in [0,1]$ to the other group. This results in per capita transfers of r^{Inc} and r^{Opp} . Utility for both groups are described in V^{Inc} and V^{Opp} .

Both the incumbent and the opposition have access to a violence technology: the incumbent can raise an army from the state's resources, while the opposition can mount an insurgency financed by its own members' income. An army of size $A \in (0,1)$ costs wA to raise. For simplicity, we assume that there are two sizes of armies available for the incumbents (low and high) and one for the opposition A_L^{Inc} , A_H^{Inc} , and A_L^{Opp} . Power changes hands stochastically with probability γ , but either side can improve their chances

by investing in violence so $\gamma(A^{Opp}, A^{Inc}) \in (0, 1)$ which ensures that no one is assured of winning or losing. The value of the prize for the group in power is equal to $Z = R/w$.

The timing at any generation is as follows:

1. Group I starts in power.
2. R and w are realized.
3. Group O chooses violence level 0 or A_L^{Opp} .
4. Group I observes this, chooses violence level 0, A_L^{Inc} , or A_H^{Inc} .
5. A new incumbent, I' , is chosen according to the conflict function.
6. Group I' chooses the level of transfers.
7. Payoffs are realized and the current generation dies.

We make several other simplifying assumptions. More violence always improves a group's probability of taking or holding power, or $\gamma(0, A^{Inc}) < \gamma(A_L^{Opp}, A^{Inc})$, and $\gamma(A^{Opp}, 0) > \gamma(A^{Opp}, A_L^{Inc}) > \gamma(A^{Opp}, A_H^{Inc})$ for all A^{Opp}, A^{Inc} . Opposition violence is never undefended, so if $A^{Opp} = A_L^{Opp}$, then $A^{Inc} > 0$. Opposition and incumbent break ties by choosing the lower level of violence.

Investing in Violence

The government has total resources R , which it spends on transfers to the two groups $r^{Inc'}$ and $r^{Opp'}$ (whose identities are determined by the violence function), and the incumbent's army A^{Inc} , so its budget constraint is:

$$R - r^{Inc'} / 2 - r^{Opp'} / 2 - wA^{Inc} \geq 0 \quad (1)$$

Since the incumbent is restricted in the degree to which it can favor its own group, giving $\sigma \in [0,1]$ to the other group for every 1 transferred to its own, and assuming that the government budget constraint holds with equality, we have:

$$r^{Inc'} = 2(1 - \Theta)[R - wA^{Inc}] \quad (2)$$

where, for convenience, $\Theta = \sigma / (1 + \sigma) \in [0, 1/2]$.

To keep the model as simple as possible, utility is equal to consumption,³ which is wages plus transfers. The expected payoff to the incumbent is then:

$$V^{Inc}(w, R; A^{Opp}, A^{Inc}) = w + [(1 - \Theta) - \gamma(A^{Opp}, A^{Inc})(1 - 2\Theta)] 2 [R - wA^{Inc}] \quad (3)$$

The term $[(1 - \Theta) - \gamma(A^{Opp}, A^{Inc})(1 - 2\Theta)]$ represents the share of the end-of-period transfers that the incumbent expects to get. If the incumbent retains power, they receive a share of $(1 - \Theta)$ of transfers while if they lose power they receive a share of Θ , meaning the amount they lose is $(1 - 2\Theta)$, and that occurs with probability $\gamma(A^{Opp}, A^{Inc})$. The opposition's expected payoff is similar, with the primary difference that they have to fund their own violence from their wages:

³ See below for a discussion of the ramifications of relaxing the assumption of linear utility.

$$V^{Opp}(w, R; A^{Opp}, A^{Inc}) = w(1 - A^{Opp}) + [\Theta + \Upsilon(A^{Opp}, A^{Inc})(1 - 2\Theta)] 2 [R - wA^{Inc}] \quad (4)$$

This allows us to define the groups' optimal actions. Working backwards from the end of a period, the incumbent chooses the violence level:

$$\hat{A}^{Inc} = \operatorname{argmax} V^{Inc}(w, R; A^{Opp}, A^{Inc}) \text{ for } A^{Inc} \in \{0, A_L^{Inc}, A_H^{Inc}\} \quad (5)$$

Because of the timing, A^{Opp} has been chosen earlier and are known to the incumbent, and we assume that if $A^{Opp} \neq 0$ then $\hat{A}^{Inc} \neq 0$. We define $\hat{A}^{Inc}(0)$ and $\hat{A}^{Inc}(A_L^{Inc})$ as the levels of violence that the incumbent will choose in response to levels of violence 0 and A_L^{Inc} from the opposition. The opposition, chooses its optimal level of violence:

$$\hat{A}^{Opp} = \operatorname{argmax} V^{Opp}(w, R; A^{Opp}, \hat{A}^{Inc}(A^{Opp})) \text{ for } A^{Opp} \in \{0, A_L^{Inc}\} \quad (6)$$

Commitment and Timing

As in the Besley-Persson models, violence is created by the incumbents' inability to commit to share resources more equally. If the two groups were able to write enforceable contracts, then in equilibrium there would always be peace, because violence wastes resources. In terms of the steps above, there is no time before the opposition chooses violence in step 3 where the incumbent can credibly promise transfers to the opposition in exchange for peace. Additionally, the incumbent cannot credibly commit to

using the highest level of violence in response to an insurgency if that response would not be optimal ex post.⁴

3. Characterizing Optimal Investment in Violence

Recall that $Z = R / w$ is a measure of the prize for having power, normalized for convenience by w . We define $Z^{Inc}(A^{Opp}; A_1^{Inc}, A_2^{Inc})$ as the threshold level of Z in which the incumbent is indifferent between investing between any two of violence A_1^{Inc} and A_2^{Inc} given that the opposition is investing A^{Opp} . First we solve for the incumbent's optimal investment in violence as the prize gets larger.

Proposition 1: There exist thresholds for Z that determine $\hat{A}^{Inc}(0)$ and $\hat{A}^{Inc}(A_L^{Opp})$, the optimal incumbent violence against peace and violence, respectively. The thresholds satisfy either (i):

- 1) $\hat{A}^{Inc}(0) = 0$ if $Z \leq Z^{Inc}(0; 0, A_L^{Inc})$, so $Z^{Inc}(0; 0, A_L^{Inc})$ is the threshold where the incumbent is indifferent between using 0 violence and A_L^{Inc} , given that the opposition is using 0.
- 2) $\hat{A}^{Inc}(0) = A_L^{Inc}$ if $Z^{Inc}(0; 0, A_L^{Inc}) < Z \leq Z^{Inc}(0; 0, A_H^{Inc})$
- 3) $\hat{A}^{Inc}(0) = A_H^{Inc}$ if $Z > Z^{Inc}(0; 0, A_H^{Inc})$

⁴ For example, we assume that it is not possible for the incumbents to purchase military equipment that would decrease its marginal cost of future military operations. Such a model would be plausible, but it would add additional complexity.

or (ii):

- 1) $\hat{A}^{Inc}(0) = 0$ if $Z \leq Z^{Inc}(0; 0, A_H^{Inc})$
- 2) $\hat{A}^{Inc}(0) \neq A_L^{Inc}$ for all Z
- 3) $\hat{A}^{Inc}(0) = A_H^{Inc}$ if $Z > Z^{Inc}(0; 0, A_H^{Inc})$

In addition, (iii) is always satisfied:

- 4) $\hat{A}^{Inc}(A_L^{Opp}) = A_L^{Inc}$ if $Z \leq Z^{Inc}(A_L^{Opp}; A_L^{Inc}, A_H^{Inc})$
- 5) $\hat{A}^{Inc}(A_L^{Opp}) = A_H^{Inc}$ if $Z > Z^{Inc}(A_L^{Opp}; A_L^{Inc}, A_H^{Inc})$

Case (i) means that as Z goes from zero to infinity, the incumbent's optimal response to peace will be peace, then low violence, then high violence. Case (ii) means that the incumbent never wants to use low violence against peace; it's always better to use peace or high violence. Either case is possible depending on the parameters. The thresholds (iii) mean that as Z goes from zero to infinity, the incumbent's optimal response to violence will be low violence, then high violence.

Proof: See Appendix D.

Next we solve for the opposition's optimal investment in violence as the prize Z gets larger.

Proposition 2: For all $Z \neq Z^{Inc}(A_L^{Opp}; A_L^{Inc}, A_H^{Inc})$, \hat{A}^{Opp} is non-decreasing in Z .

This means that as the prize gets larger, the opposition will switch from peace to insurgency. There is one exception in which the opposition's optimal strategy may switch back to peace, however, even as Z grows. At the exact Z when the incumbent is indifferent between using high and low violence, the opposition—mounting an insurgency to combat a low investment of violence by the incumbent—chooses to revert to peace since the higher investment in violence by the incumbent reduces the likelihood that the opposition could win. From that particular point of Z on up, the opposition may then choose again to mount an insurgency even against a large investment in violence by the incumbent once the prize got attractive enough.

Proof: See Appendix D.

The Consequences of Aid Inflows

In this subsection, we articulate what may happen when aid arrives in another fragile state. Besley and Persson (2011a) model a potential hierarchy of violence from peace via repression to civil war as unearned income rises, and find broad support for their prediction in cross-country data using quasi-experimental methods. In our extended model, we find that it is possible to observe low levels of aid leading to low-investment civil war and higher levels of aid leading to repression. These are not necessarily the most likely outcomes, but there are nonetheless reasonable conditions within the model in which such an ordering might occur.

Proposition 3: In order to observe civil conflict for Z_L and repression for $Z_H > Z_L$, the following conditions are necessary and sufficient:

- 1) $Z_H > \min \{ Z^{Inc}(0; 0, A_L^{Inc}), Z^{Inc}(0; 0, A_H^{Inc}) \}$
- 2) $Z_L > ((A_L^{Opp} / 2) + [\Theta + \Upsilon(A_L^{Opp}, \hat{A}^{Inc}(A_L^{Opp}))(1 - 2\Theta)] \hat{A}^{Inc}(A_L^{Opp}) - [\Theta + \Upsilon(0, \hat{A}^{Inc}(0))(1 - 2\Theta)] \hat{A}^{Inc}(0)) / ((1 - 2\Theta)(\Upsilon(A_L^{Opp}, \hat{A}^{Inc}(A_L^{Opp})) - \Upsilon(0, \hat{A}^{Inc}(0)))) \equiv Z^T(Z_L)$
- 3) $Z_H < ((A_L^{Opp} / 2) + [\Theta + \Upsilon(A_L^{Opp}, \hat{A}^{Inc}(A_L^{Opp}))(1 - 2\Theta)] \hat{A}^{Inc}(A_L^{Opp}) - [\Theta + \Upsilon(0, \hat{A}^{Inc}(0))(1 - 2\Theta)] \hat{A}^{Inc}(0)) / ((1 - 2\Theta)(\Upsilon(A_L^{Opp}, \hat{A}^{Inc}(A_L^{Opp})) - \Upsilon(0, \hat{A}^{Inc}(0)))) \equiv Z^T(Z_H)$

The first condition means that the incumbent will invest in violence against a peaceful opposition for sufficiently high Z , which is the definition of repression. The second and third conditions say that the opposition will mount an insurgency against a poor incumbent (low Z), but be peaceful against a rich incumbent (high Z). This situation is realized when the poor incumbent can only afford to invest in a small army, which means violence pays for the opposition, but the rich incumbent can finance a large army, so the opposition would have little chance of winning the state through violence and would prefer to receive transfers instead.

Proof: see Appendix D. Appendix D also describes some values over which these conditions are maintained. We note the right-hand side of conditions (2) and (3) are algebraically the same, but evaluated at the different Z s, so have different values.

4. Conclusion

This chapter has argued for the theoretical possibility that receiving aid may reduce the likelihood of an insurgency, and the withdrawal of this aid leads to civil conflict. We put forward a simple model that makes that prediction, and the purpose of this chapter is to present a theoretical model consistent with the empirical results found in Ahmed, Schwab, and Werker (2016).

Appendix D

Proof of Proposition 1:

Proof. To determine when peace is a better response to peace than low violence, we compare the utility functions for the two. $A^{Inc}(0) = 0$ is preferred to $A^{Inc}(0) = A_L^{Inc}$ if $V^{Inc}(w, R; 0, 0) \geq V^{Inc}(w, R; 0, A_L^{Inc})$, which is equivalent to:

$$[(1 - \Theta) - \gamma(0,0)(1 - 2\Theta)][Z - 0] \geq [(1 - \Theta) - \gamma(0, A_L^{Inc})(1 - 2\Theta)][Z - A_L^{Inc}]$$

or

$$Z \leq [(1 - \Theta) - \gamma(0, A_L^{Inc})(1 - 2\Theta)] A_L^{Inc} / [(1 - 2\Theta)(\gamma(0,0) - \gamma(0, A_L^{Inc}))] \equiv Z^{Inc}(0; 0, A_L^{Inc}).$$

Similarly, peace is a better response to peace than high violence (i.e., $A^{Inc}(0) = 0$ is preferred to $A^{Inc}(0) = A_H^{Inc}$) if

$$Z \leq [(1 - \Theta) - \gamma((0, A_H^{Inc})(1 - 2\Theta))] A_H^{Inc} / [(1 - 2\Theta)(\gamma(0,0) - \gamma(0, A_H^{Inc}))] \equiv Z^{Inc}(0; 0, A_H^{Inc}).$$

Note that a higher level is always preferred by the incumbent for a sufficiently high Z and a lower level of violence is always preferred for a sufficiently low Z , and there is never switching back and forth between violence levels as Z increases (holding opposition violence constant). Thus, peace is the best response to peace if both conditions are met, or $\hat{A}^{Inc}(0) = 0$ if

$$Z \leq \min \{ Z^{Inc}(0; 0, A_L^{Inc}), Z^{Inc}(0; 0, A_H^{Inc}) \} \quad (*)$$

If the first threshold in (*) is less than or equal to the second, then there exist value(s) of Z for which $\hat{A}^{Inc}(0) = A_L^{Inc}$. This would be true because the threshold for switching to high violence is higher than the threshold for switching to low violence, which is labeled case (i). If that is the case, then high violence is the optimal response against peace (i.e., $\hat{A}^{Inc}(0) = A_H^{Inc}$) whenever $A^{Inc}(0) = A_H^{Inc}$ is preferred to $A^{Inc}(0) = A_L^{Inc}$ (which will imply that it is preferred to $\hat{A}^{Inc}(0) = 0$). This happens when

$$Z > Z^{Inc}(0; A_L^{Inc}, A_H^{Inc}) \equiv$$

$$[(1 - \Theta) - \gamma(0, A_H^{Inc})(1-2\Theta)] A_H^{Inc} - [(1 - \Theta) - \gamma(0, A_L^{Inc})(1-2\Theta)] A_L^{Inc} / [(1 - 2\Theta)(\gamma(0, A_L^{Inc}) - \gamma(0, A_H^{Inc}))].$$

When $Z^{Inc}(0; 0, A_L^{Inc}) < Z < Z^{Inc}(0; 0, A_H^{Inc})$, low violence is the best response to peace (so $\hat{A}^{Inc}(0) = A_L^{Inc}$). This covers case (i).

If the second expression in (*) is larger, then $\hat{A}^{Inc}(0) \neq A_L^{Inc}$ for all Z . The reason this happens is that as Z increases from zero, it passes the threshold which makes high violence desirable against peace before it passes the threshold that makes low violence desirable. As Z continues to get larger, high violence continues to be more attractive by a greater margin over low violence. Up to $Z^{Inc}(0; 0, A_H^{Inc})$, $\hat{A}^{Inc}(0) = 0$, and then above $Z^{Inc}(0; 0, A_H^{Inc})$, $\hat{A}^{Inc}(0) = A_H^{Inc}$, which is case (ii).

The analysis of $\hat{A}^{Inc}(A_L^{Opp})$ is simpler, because we have assumed that it is impossible to have an undefended insurgency. Following the same logic as above, the optimal response against opposition violence is to use high violence (i.e., $\hat{A}^{Inc}(A_L^{Opp}) = A_H^{Inc}$) if

$$Z > Z^{Inc}(A_L^{Opp}, A_L^{Inc}, A_H^{Inc}) \equiv \frac{[(1 - \Theta) - \gamma(A_L^{Opp}, A_H^{Inc})(1 - 2\Theta)] A_H^{Inc} - [(1 - \Theta) - \gamma(A_L^{Opp}, A_L^{Inc})(1 - 2\Theta)] A_L^{Inc}}{[(1 - 2\Theta)(\gamma(A_L^{Inc}, A_L^{Inc}) - \gamma(A_L^{Inc}, A_H^{Inc}))]}.$$

Thus, holding the opposition violence constant, incumbent violence is non-decreasing in Z , and we can pinpoint the threshold at which violence changes. Also, for sufficiently low values of Z , the optimal response to peace is peace and the optimal

response to violence is the lower level of violence. For sufficiently high values of Z , the optimal response is high violence. \square

Proof of Proposition 2:

Proof. $Z^{Inc}(A_L^{Opp}, A_L^{Inc}, A_H^{Inc})$ is the level of Z at which the incumbent is indifferent between using high and low violence, given that the opposition uses violence. If Z moves above $Z^{Inc}(A_L^{Opp}, A_L^{Inc}, A_H^{Inc})$, then suddenly opposition violence will be met with high violence, and this is the only value of Z where an increase in Z can cause opposition violence to become unattractive.

Consider Z_1 and Z_2 , with $Z_1 < Z_2$ and $Z^{Inc}(A_L^{Opp}, A_L^{Inc}, A_H^{Inc}) \notin [Z_1, Z_2]$. We define $\hat{A}^{Opp}(Z_j)$ as the optimal violence for the opposition given $Z = Z_j$ and $\hat{A}^{Inc}(\hat{A}^{Opp}(Z_j))$ to be the corresponding violence level for the incumbent. Since we are trying to show that \hat{A}^{Opp} is non-decreasing, we can assume that $\hat{A}^{Opp}(Z_1) = A_L^{Opp}$ (otherwise it is trivially non-decreasing), and it is sufficient to show that $\hat{A}^{Opp}(Z_2) = A_L^{Opp}$. We define $\hat{A}^{Opp}(A^{Inc}_{peace}, A^{Inc}_{fight})$ as the opposition's optimal violence given that the incumbent will use A^{Inc}_{peace} if the opposition uses zero violence and $A^{Inc}_{fight} > 0$ if they use A_L^{Opp} . We are interested in characterizing when the opposition will choose violence, given that the incumbent responds optimally—that is, when $A^{Opp}(\hat{A}^{Inc}(0), \hat{A}^{Inc}(A_L^{Opp})) = A_L^{Opp}$ is preferred to $A^{Opp}(\hat{A}^{Inc}(0), \hat{A}^{Inc}(A_L^{Opp})) = 0$. This holds when the opposition's utility is higher under violence, $V^{Opp}(w, R; D, 0, \hat{A}^{Inc}(0)) < V^{Opp}(w, R; D, A_L^{Opp}, \hat{A}^{Inc}(A_L^{Opp}))$. Note that the opposition would never use violence when $\gamma(A_L^{Opp}, \hat{A}^{Inc}(A_L^{Opp})) \leq \gamma(0, \hat{A}^{Inc}(0))$, because then using violence would not increase their chances of coming to power. Substituting in

for the utility function, the opposition uses violence when $\gamma(A_L^{Opp}, \hat{A}^{Inc}(A_L^{Opp})) > \gamma(0, \hat{A}^{Inc}(0))$ and

$$(1 - 0) + [\Theta + \gamma(0, \hat{A}^{Inc}(0))(1 - 2\Theta)] \geq [Z - \hat{A}^{Inc}(0)] < (1 - A_L^{Opp}) + [\Theta + \gamma(A_L^{Opp}, \hat{A}^{Inc}(A_L^{Opp}))(1 - 2\Theta)] \geq [Z - \hat{A}^{Inc}(A_L^{Opp})]$$

or

$$Z > ((A_L^{Opp} / 2) + [\Theta + \gamma(A_L^{Opp}, \hat{A}^{Inc}(A_L^{Opp}))(1 - 2\Theta)] \hat{A}^{Inc}(A_L^{Opp}) - [\Theta + \gamma(0, \hat{A}^{Inc}(0))(1 - 2\Theta)] \hat{A}^{Inc}(0)) / ((1 - 2\Theta)(\gamma(A_L^{Opp}, \hat{A}^{Inc}(A_L^{Opp})) - \gamma(0, \hat{A}^{Inc}(0)))) \equiv Z^T(Z) \quad (**)$$

Note that Z^T is itself a function of Z , because the values of $\hat{A}^{Inc}(0)$ and $\hat{A}^{Inc}(A_L^{Opp})$ depend on Z . By assumption, $A^{Opp}(\hat{A}^{Inc}(0), \hat{A}^{Inc}(A_L^{Opp})) = A_L^{Opp}$ is optimal when $Z = Z_l$, so the inequality (**) holds at Z_l . By the previous proposition, if Z is increased to Z_2 , it must be the case that neither $\hat{A}^{Inc}(0)$ nor $\hat{A}^{Inc}(A_L^{Opp})$ has decreased because both expressions are nondecreasing in Z . Also, $\hat{A}^{Inc}(A_L^{Opp})$ has not increased because we have not crossed over $Z^{Inc}(A_L^{Opp}, A_L^{Inc}, A_H^{Inc})$, the only value for which $\hat{A}^{Inc}(A_L^{Opp})$ can increase. Thus, $\hat{A}^{Inc}(A_L^{Opp})$ has not changed, and neither has $\gamma(A_L^{Opp}, \hat{A}^{Inc}(A_L^{Opp}))$. $\hat{A}^{Inc}(0)$ has increased or stayed the same, which implies that $\gamma(0, \hat{A}^{Inc}(0))$ has decreased or stayed the same because γ (the probability of power changing hands) is decreasing in the second argument (incumbent violence). If $\hat{A}^{Inc}(0)$ increases, the direct effect is to cause the threshold in (**) Z^T to get smaller, and if $\gamma(0, \hat{A}^{Inc}(0))$ decreases, that also causes the

threshold to get smaller (because the denominator increases). Therefore, increasing Z from Z_1 to Z_2 does not increase Z^T , so if the inequality in (**) is satisfied at Z_1 then it must also be satisfied at Z_2 , and opposition violence is nondecreasing. \square

Proof of Proposition 3:

Proof. The first condition is the simplest; it says that with a high value for Z , the incumbent will use violence against a peaceful opposition, which is the definition of repression. In the proof of Proposition 1, (*) tells us the condition for when peace is the optimal response to peace, and condition 1 is the same, but with the opposite inequality. The second condition says that the opposition will use violence with $Z = Z_L$, and the third says that the opposition will use peace with $Z = Z_H$. This is the same condition as (**) in Proposition 2 to determine the opposition's choice of violence. We note that the right-hand side of conditions 2 and 3 are the same expression, but they are evaluated for $Z = Z_L$ in the second condition and $Z = Z_H$ in the third, so they have different values.

For example, $\hat{A}^{Inc}(A_L^{Opp})$, which represents the optimal incumbent violence given a violent opposition, must be A_L^{Inc} for $Z = Z_L$ and A_H^{Inc} for $Z = Z_H$. We can pin down those values since Proposition 2 tells us that for all $Z \neq Z^{Inc}(A_L^{Opp}, A_L^{Inc}, A_H^{Inc})$, \hat{A}^{Opp} is non-decreasing in Z . \hat{A}^{Opp} does increase as we increase Z from Z_L to Z_H , so we must have $Z_L \leq Z^{Inc}(A_L^{Opp}, A_L^{Inc}, A_H^{Inc}) < Z_H$. By conditions 4 and 5 from proposition 1, $\hat{A}^{Inc}(A_L^{Opp}) = A_L^{Inc}$ for $Z = Z_L$ and $\hat{A}^{Inc}(A_L^{Opp}) = A_H^{Inc}$ for $Z = Z_H$, so the three conditions above are satisfied if and only if Z_L leads to civil conflict and Z_H leads to repression. \square

We can verify that some reasonable values of the parameters generate this result.. In general, $Z = R / w$, so $Z_L = R_L / w$ and $Z_H = R_H / w$. We consider an example where a low R causes both groups to use low violence because it is cheap, and a high R means that an incumbent will use high violence in response to opposition violence, which deters the opposition and causes one-sided violence.

We want to show that it is possible Z_L satisfies

- 1) $Z_L \in (Z^{Inc}(0; 0, A_L^{Inc}), Z^{Inc}(0; 0, A_H^{Inc}))$,
- 2) $Z_L < Z^{Inc}(A_L^{Opp}; A_L^{Inc}, A_H^{Inc})$, and
- 3) $\hat{A}^{Opp} = A_L^{Opp}$

and Z_H satisfies

- 4) $Z_H > Z^{Inc}(A_L^{Opp}; A_L^{Inc}, A_H^{Inc})$
- 5) $Z_H \in (Z^{Inc}(0; 0, A_L^{Inc}), Z^{Inc}(0; 0, A_H^{Inc}))$
- 6) $\hat{A}^{Opp} = 0$

Conditions 1 and 2 mean that for Z_L the incumbents will use low violence against A_L^{Opp} or against peace, so $\hat{A}^{Inc}(0) = \hat{A}^{Inc}(A_L^{Opp}) = A_L^{Inc}$. Condition 3 says for Z_L the opposition will use violence. Conditions 4 and 5 say that the opposition will use high violence against A_L^{Opp} and low violence against peace, and that the opposition will choose peace.

$Z^{inc}(0; 0, A_L^{inc})$ is the threshold where the incumbent facing a peaceful opposition is indifferent between using low violence (A_L^{inc}) and peace (higher Z implies higher violence). $Z^{inc}(0; 0, A_L^{inc})$ will be sufficiently low if A_L^{inc} is low and $(\gamma(0, 0) - \gamma(0, A_L^{inc}))$ is not too low, meaning that low violence is very cheap and yields a significantly improved chance of holding power. $Z^{inc}(0; 0, A_H^{inc})$, the threshold between using high violence and low violence against peace, can be very high. Specifically, suppose that A_H^{inc} is high, and $(\gamma(0, A_L^{inc}) - \gamma(0, A_H^{inc}))$ is very low, meaning that high violence is very expensive and barely improves the incumbent's prospects, so $Z^{inc}(0; 0, A_H^{inc})$ is very high. These values guarantee that the incumbent will use low violence against a peaceful opposition, satisfying condition 1. The thresholds in condition 1 can be arbitrarily low and high, so it is simple to choose a Z_L that satisfies condition 2.

The third condition is satisfied when violence is sufficiently attractive for the opposition. This is condition 2 from proposition 2, but now we have specified the incumbent's optimal levels of violence, so we can substitute those in:

$$Z_L > ((A_L^{Opp}/2) + [\Theta + \gamma(A_L^{Opp}, A_L^{inc})(1 - 2\Theta)] A_L^{inc} - [\Theta + \gamma(0, A_L^{inc})(1 - 2\Theta)] A_L^{inc}) / ((1 - 2\Theta)(\gamma(A_L^{Opp}, A_L^{inc}) - \gamma(0, A_L^{inc})))$$

This condition is met when A_L^{Opp} is sufficiently low and $(\gamma(A_L^{Opp}, A_L^{inc}) - \gamma(0, A_L^{inc}))$ is high, meaning that opposition violence is inexpensive but effective. Together, these give us conditions under which the first three assumptions are satisfied.

The fourth condition is met by fixing some sufficiently high Z_L . Condition 5 will not be violated as long as $Z^{Inc}(0; 0, A_H^{Inc})$ is high enough. It can be arbitrarily large as long as $(\gamma(0, A_L^{Inc}) - \gamma(0, A_H^{Inc}))$ is close to zero. Once the first five conditions are met, the sixth would plausibly be met as well. The opposition has the choice between peace, which is met with low violence, and violence, which is met with high violence. The opposition's chance of gaining power may not even be improved by using violence, and they use up their own resources and cause the incumbent to waste more state resources with a larger army. Thus, it is possible to observe the pattern that we are interested in, of repression in periods when a poor country receives aid and civil conflict in periods when it does not.

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Development Economics, Labor Economics

TEACHING EXPERIENCE

Instructor, Economic Analysis of Legal Issues, Boston University, Spring 2015
Head teaching fellow, Introductory Macroeconomics, Spring 2013, Spring 2014
Teaching fellow, Introductory Microeconomics, Fall 2011, Fall 2012, Fall 2013
Teaching fellow, Introductory Macroeconomics, Spring 2012

WORK EXPERIENCE

Instructor, Game Theory, Boston University Summer Challenge (high school students),
Summer 2013
Leader, training session for teaching fellows, Summer 2014
Research assistant to Eric Werker, Harvard Business School, 2011 - 2015
Professional poker player, 2006 - 2010
Research assistant, Lincoln Institute for Land Policy, 2003 - 2005

FELLOWSHIPS AND AWARDS

Gitner Prize for Excellence in Teaching Undergraduates by a Ph.D. Student, Boston
University, 2015
Funded Participant, Advanced Graduate Winter Workshop, Bangalore, India, 2015
Special Research Fellowship, Boston University, Fall 2014
Finalist, Best Paper Award, Spring Meeting of Young Economists, Vienna, Austria, 2014
Dean's Fellowship, Boston University, Fall 2011 - present

CONFERENCES AND PRESENTATIONS

Western Economic Association International, Denver CO, 2014
Spring Meeting of Young Economists, Vienna, Austria, 2014

Northeast Universities Development Consortium Conference, Boston MA, 2014
Advanced Graduate Winter Workshop, Bangalore, India, 2015

WORKING PAPERS

“Employment Protection and the Labor Informality of the Youth: Evidence from India” *Job Market Paper*, October 2015.
“Profits and Economic Development,” (with Eric Werker), revise and resubmit at the *Journal of Development Economics*, 2015.

WORK IN PROGRESS

“The Political Transfer Problem” (with Faisal Z. Ahmed and Eric Werker)
“The Impact of Rural Employment Guarantees on Manufacturing in India”
“Do Firms Benefit from Regulations on their Competitors? District-level evidence from India”
“Improving by Importing: The Effect of Import Quality on Export Quality”

COMPUTER SKILLS

Stata, Matlab, Microsoft Office, Latex, Lyx, Shared Computing Cluster

OTHER

Development Reading Group Coordinator

CITIZENSHIP

United States

REFERENCES

Professor Dilip Mookherjee
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DANIEL SCHWAB

Employment Protection and the Labor Informality of the Youth: Evidence from India (Job Market Paper)

Employment protection laws (EPL), are designed to promote security for workers by placing restrictions on firing, but they generate unintended consequences. Using India as a setting, I argue that EPL shifts jobs from the young to older workers. The identification strategy relies on heterogeneity between manufacturing sectors, and is motivated by Rajan and Zingales (1998). The impact of EPL is strongest in those manufacturing sectors where Indian employers would like to fire workers, proxied by the involuntary separation rate in the United States or Latin America. Finally, I present suggestive evidence that EPL reduces plant-level total factor productivity, and that the shift in jobs from young to old is an important channel.

Profits and Economic Development (with Eric Werker, revise and resubmit at the *Journal of Development Economics*) [download](#)

Are rents, or excess profits, good for development? Using industry-level manufacturing data, this paper demonstrates a negative effect of rents, measured by the mark-up ratio, on productivity growth. The negative effect is strongest in poor countries, suggesting that high profits stymie economic development rather than enable it. Consistent with the rent-seeking mechanism of our model, we find that high rents are associated with a slower reduction in tariffs. A country's average mark-up in manufacturing is a strong negative predictor of future economic growth, indicating that we may be measuring a phenomenon of the broader business environment.

The Political Transfer Problem (with Faisal Z. Ahmed and Eric Werker)

Aid from oil-rich autocrats created unearned rents for many developing countries in the 1970s, which transformed their trajectories of governance. While some recipients of these funds eventually democratized as part of the "Third Wave," many languished in autocracy. Building on recent theoretical formulations on fragile states, we develop a model to account for these dynamics. We formally demonstrate that autocrats experiencing a windfall in unearned income may find it optimal to donate to other countries some of the windfall in order to make the state a less attractive prize to potential insurgents (and in the process further their geopolitical interests). The effect on the recipient varies with the level of rents, where high levels of rents generate repression, and low levels can lead to conflict or democratization. We develop case studies of the Middle East, Latin America, and Eastern Europe, and argue that the boom in oil prices from 1973-85 produced long-run political dynamics that are consistent with the model.