Justice Delayed is Growth Denied: The Effect of Slow Courts on Relationship-Specific Industries in India*

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Abstract

Are well-functioning formal judicial institutions important for economic development, or can informal contracting arrangements provide adequate substitutes? This paper aims to answer this question using variation across industries in their reliance on contracts along with variation across Indian states in the average speed of courts. The identification strategy is motivated by theory from the incomplete contracting literature in which it is argued that transactions involving relationship-specific investments are more exposed to post-contractual opportunism and hence have greater need for efficient contract enforcement. The paper finds that the interaction between state level court efficiency and industry level relationship-specificity is highly predictive of future growth in India's formal manufacturing sector. The threat of omitted variable bias is minimized by the inclusion of state and industry fixed effects, while a number of robustness checks and placebo tests rule out competing explanations and provide additional confidence in the hypothesized mechanism.

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1 Introduction

Institutions matter for growth and development, but which kinds of institutions matter most? Determining the relative importance of constituent institutional components - such as legal institutions, property rights institutions, political institutions, and cultural institutions - is the goal of a growing body of research. The continued lack of clarity may be partly due to the fact that much of the previous research on institutions was conducted at the cross country level, where measures of institutions are coarse, endogeneity concerns are uppermost, and convincing sources of identification are hard to come by.[1] The value of high quality formal judicial institutions in particular has been disputed. Some have argued that well functioning formal judicial institutions are important economic determinants (Berkowitz et al. (2006); Nunn (2007); Levchenko (2007); Chemin (2012)) while others have argued that they are not (Acemoglu and Johnson (2005)), the latter suggesting that informal arrangements (including relational contracts and kinship networks) may serve as workable substitutes. This paper aims to address the question of whether well functioning formal judicial institutions are important for growth and development, by taking advantage of insights from the theoretical literature on incomplete contracts as well as variation across states and industries in a within-country setting.

Judicial institutions can be dysfunctional in a variety of ways, but perhaps the most pervasive source of judicial dysfunction in developing countries is the slow speed of courts. India, with a backlog of cases that one High Court Justice has said would take 320 years to clear, is certainly no exception (The Times of India (2010)). Slow courts are detrimental to a well functioning legal system because they increase the cost of enforcing contracts by delaying the payoff of taking an agent to court. If contracts are difficult or costly to enforce, underinvestment is more likely to occur and potentially surplus generating transactions are less likely to occur (Williamson (1979)). This should be all the more true of transactions that involve

[1] See Pande and Udry (2005) for an overview of the concerns.

relationship-specific investments (Klein et al. (1978), Grossman and Hart (1986)).

Following Berkowitz et al. (2006), Levchenko (2007) and especially Nunn (2007), the identification strategy employed in this paper hinges on this last point: that well functioning judicial institutions should be especially important for growth *in contract intensive industries* - which I will define, following Nunn (2007), as those industries that require more relationship-specific inputs. My empirical methodology is then to regress growth (in firms' value added, fixed capital, employment and net entry) on the interaction between an objective state level measure of the speed of courts and an industry level measure of contract intensity from Nunn (2007).[2] Focusing on the interaction and including state and industry fixed effects insulates the analysis from the most obvious concerns regarding the endogeneity of contracting institutions and facilitates the application of a rigorous battery of robustness tests, while regressing future growth (growth over the period 1999 to 2008) against initial levels of court efficiency (measured by average trial durations in 1999) makes it is less likely that the results are driven by reverse causality.

The paper's main finding is that fast courts have a strong positive effect on growth (in all four dependent variables above) - *particularly* for more contract intensive industries. Figure 1, in the mode of Rajan and Subramanian (2011), presents an informal way of visualizing this result. The figure presents, for each state in India, the difference in average growth rates of gross value added between contract intensive and non-contract intensive industries, plotted against state level court efficiency. The positive correlation suggests that contract intensive industries experience relatively faster growth when they are located in states with more efficient courts. The results of the formal econometric tests suggest that, for an industry in the 75th percentile of contract intensity, an improvement of one standard deviation in state court efficiency would imply a higher annual growth rate for gross value added of 0.9 percentage points.[3] For comparison, the mean annual growth rate of gross value added among

[2]The decision to study the effect of court efficiency on growth in outcomes rather than on levels is motivated in Section 4.

[3]The growth rates of all dependent variables in the analysis, including gross value added, are constructed at

state-industry cells over the period of analysis is 2.0 percentage points.

Because the analysis includes state and industry fixed effects, the results cannot be generated by omitted variables that are simply correlated with court efficiency. However, the results might be biased if omitted variables exist that are correlated with *the interaction* between court efficiency and contract intensity. To mitigate this concern, I consider a number of possible alternative mechanisms that might conceivably be driving the results and attempt to control for them explicitly via the inclusion of additional interaction terms in the robustness checks. None of these additional potential mechanisms can explain the results. None of these additional potential mechanisms can explain the results. I also conduct a placebo test by replacing the efficiency of civil courts in the main specification with the efficiency of criminal courts, under the assumption that criminal court efficiency should *not* be a strong predictor of economic growth. The test confirms this intuition and thereby constitutes strong evidence in favor of the hypothesized mechanism: in order for the results to be biased by an alternative mechanism, this alternative must be able to explain why the speed of civil courts is spuriously correlated with growth in contract intensive industries while the speed of criminal courts is not.

This paper is part of a growing literature on the role of legal and judicial institutions in development (Aldashev (2009) contains a concise review of the literature up to 2009). As previously noted, the paper employs a similar identification strategy to Berkowitz et al. (2006), Levchenko (2007) and Nunn (2007).[4] All three papers provide evidence that countries with higher quality judicial institutions have relatively more exports in industries that are more dependent on contract enforcement (where this dependence is proxied by industry "complexity" or "contract intensity"). Building on this literature, Boehm (2018) employs a novel strategy of identifying contract dependence in industry *pairs* according to the frequency with which firms engage in litigation in order to show that firms in such pairs spend less on intermedi-

the state x industry level and reflect annualized growth for the period 1999 to 2008.

[4]Indeed, I make direct use of Nunn (2007)'s measure of contract intensity - after matching it to the available Indian industry codes.

ate inputs when they are located in countries with high contract enforcement costs - which lowers aggregate productivity. The main difference between the papers above and this one - aside from the fact that I examine the effect of judicial quality on domestic manufacturing generally rather than on trade patterns - is that the papers above conduct their analysis at the cross country level while the analysis here is within country. In addition to reducing concerns regarding endogeneity (due to the vast set of institutional, cultural, and historical differences that exist across countries and which may be correlated with one another as well as with economic outcomes), conducting the analysis within country allows one to use an objective and comparable measure of court speed rather than subjective measures that are likely to measure a host of other factors.[5] Conducting the analysis at a more micro level also allows one to subject the analysis to a potentially richer set of placebo and robustness tests as described above.

This paper also shares similarities with Ahsan (2013), Chemin (2012) and Boehm and Oberfield (2018), three within-country studies that also examine the effect of Indian courts on economic outcomes. Ahsan (2013) shows that the lowering of input tariffs had a positive effect on the productivity of incorporated firms located in states with faster courts. Chemin (2012) argues that a legal reform passed in 2002 (the Code of Civil Procedure Amendment Act, 2002) succeeded in speeding up courts and reducing trial backlogs - which he then argues induced investment by small informal firms. Boehm and Oberfield (2018) shed light on the mechanisms by which court efficiency may impact economic outcomes. In particular, they show that poor contract enforcement changes the structure of production by incentivizing firms in more contract-intensive industries to use fewer intermediate inputs and undertake more transactions within the firm.

Finally, it is worth mentioning that while formal and informal mechanisms of contract enforcement are most commonly assumed to be substitutes, there is a literature that sug-

[5]For example, both Levchenko (2007) and Nunn (2007) use the "Rule of Law" index from Kaufmann et al. (2004) as their primary measure of judicial quality. This measure is based on a survey of perceptions about the "rule of law" among certain non-randomly chosen subsets of agents in each country.

gests a more complex interaction. For example, Aldashev et al. (2012) provide a theoretical model in which changes in the formal law can exert a beneficial effect on regressive informal customs under certain conditions. Johnson et al. (2002) provide empirical evidence that well-functioning courts can in fact be complementary with relational contracting. Specifically, they show that entrepreneurs in transition economies who report greater faith in the effectiveness of courts have more trust in their trading partners and are more likely to develop new relationships with other trading partners. Even when informal contracting institutions do substitute for formal ones, there may be distributional consequences. This is suggested by Chakraborty et al. (2016), who argue that improvements in formal contracting mechanisms can disproportionately help entrepreneurs that are members of underprivileged social groups, because they tend to have smaller commercial networks and can therefore make less use of informal mechanisms such as community-based sanctions. Finally, there is work that suggests the effect of improvements in formal contract enforcement may depend on the quality of other formal institutions. For example, Aldashev and Zanarone (2017) argue that making courts faster promotes economic development only in states in which executive power is sufficiently constrained.

The rest of the paper is organized as follows. The next section (Section 2) presents background information on legal institutions in India, including information on the use of courts by firms in India. I discuss the data to be used in the rest of the paper in Section 3, and present the main results of the empirical investigation in Section 4. In Section 5, I present the results of a number of robustness and placebo checks, while Section 6 concludes.

2 Institutional Background

2.1 Judicial inefficiency in India

India's judicial system is inefficient, even in comparison to other developing countries. For example, India ranks 186 (out of 189) on the World Bank's "Doing Business" indicator for "Enforcing Contracts". According to many observers - including many within the government of India and the judiciary itself - the biggest problems related to Indian courts (especially lower courts) are the slow rate of case disposal and the concomitant large backlog of cases.[6] This is also reflected in the World Bank's "Doing Business" indicators, where it is estimated that it would take 1,420 days to resolve a hypothetical commercial sale dispute over the quality of goods (only 5 countries are worse on this measure).

Though bad on average, there is, however, considerable spatial variation in the extent of this problem. For example, in 2013, the pendency rate (i.e. the percentage of all cases that have been filed in court but are still pending trial - a common measure of case backlog) in West Bengal was an incredible 96.4%, while the pendency rate of Tamil Nadu was a significantly more respectable 64.8% (Jain (2014)). To get a sense of the extent of geographic variation in my primary measure of court efficiency, see Figure 2, which displays, for each State and Union Territory in India, the share of trials in the District/Sessions Court that were resolved in less than one year in 1999. States with faster courts are filled in with a deeper blue color, while States with slower courts are filled in with a lighter shade. States missing data are displayed in white.

2.2 Sources of geographic variation in court efficiency

The reasons for this geographic variation are manifold. The most proximate causes of this spatial variation are likely to include differences across states in judicial strength (i.e. numbers of judges) and rates of disposal - which may be a function of different legal norms and

[6] See, for example, The Times of India (2010), Rukmini (2015) and Mallet (2016).

procedures (Singh (2003), Mookherjee (1993), Debroy (2008)). The administration of all lower courts (as well as State High Courts) is under the purview of state governments. This power includes the appointment of judges and the creation of posts, and may explain much of the significant differences in court efficiency across states. Other sources of variation may have their roots further back in time.

Indeed, much of India's legal system has been inherited from the pre-Independence era. This includes its status as a system of common law but it also includes specific legislation and legal codes developed by the British[7] - as well as those prevailing in the Princely States, which maintained their own legal systems until Independence (and to which British laws did not usually apply). These different historical legacies in the different regions of India may be another source of geographical variation in contemporary court efficiency. Fully explaining the source of this geographic variation is beyond the scope of this paper, and I will take the variation as given in my empirical analysis. I note here that my identification strategy does not require variation in state level court efficiency to be uncorrelated with unobservable determinants of growth. It requires only that any unobservable determinants of growth are not correlated with *the interaction* of state level court efficiency and industry level relationship specificity.

2.3 The structure of the legal system

In order to understand the data on court efficiency and how it is used in my analysis, it is helpful to introduce some basic facts about India's court system. As shown in Table 1, the structure of India's court system is hierarchical, with the Supreme Court of India at the top of the hierarchy. Directly below are the State High Courts, and below them are several tiers of lower courts at the district level. The Court of the District and Sessions Judge is the highest court at the district level and is the only court at the district level that hears both

[7]The Indian Contract Act, for example, was passed in 1872, and to this day it is the primary law governing the circumstances in which contracts entered into will be legally binding.

civil and criminal cases. Below this court, the remaining district level courts are divided on the basis of whether they hear civil or criminal cases exclusively. Among civil courts below the District/Sessions Court there may be, in descending order, an Additional District Judge's Court, a Senior Civil Judge's Court, a Principal Junior Civil Judge's Court and a Junior Civil Judge's Court. Among criminal courts below the District/Sessions Court, there may be a Chief Judicial Magistrate's Court, a First Class Judicial Magistrate's Court, a Second Class Judicial Magistrate Court and a Special Judicial Magistrate's Court.

Since I am interested in the effect of court efficiency on the ability of firms to enter into contracts with one another, it is the efficiency of civil courts rather than criminal courts that is of primary relevance to this analysis. However, the available data on court speed cover mostly criminal courts at the district level - with one exception: some of data cover the Court of the District and Sessions Judge, which hears both civil and criminal cases. I therefore focus on the efficiency of these District/Sessions courts and use the data on the other types of courts only as a placebo test.[8]

2.4 Indian firms and the court system

At this point, it is worthwhile to ask: do firms even use courts? According to data from the 2005 World Bank Enterprise Surveys, they do: about 12.5% of firms in the survey report being involved in court cases over the period 2001-2004, and about 22.5% of firms report poor contract enforcement as a constraint to doing business (Ahsan (2013)). As the data suggest, even firms that do not take cases to court may be affected by court efficiency. That is because the formal court system can represent an outside option that influences how firms behave, even if they do not end up going to court.

There are several related questions regarding firms' use of the legal system in India. First,

[8] Although the efficiency of criminal and civil district level courts is correlated across states (because, within states, different types of courts may share similar procedures and even judges), one would nevertheless expect the efficiency of criminal courts to be a *less* effective predictor of industrial growth in contract intensive industries because it is a less direct measure of the relevant object from the firm's perspective.

to what extent can firms in India use courts outside of their geographic location? If firms can file suits anywhere, one might not expect delays in local court systems to be an impediment. According to the Civil Procedure Code (1908), a case will generally be instituted in the court presiding over the location in which the defendant resides or the location in which the breach of contract occurred. Under certain circumstances, such as cases in which the suit involves immoveable property, the case *must* be heard in the court with jurisdiction over the location of the property. Barring such statutory requirements, firms can write commercial contracts that specify the location in which disputes are to be resolved - although this is more commonly seen in arbitration agreements.

This brings up a further question: are district civil courts the relevant legal institutions to be studying? As the previous discussion suggests, firms may be able to use alternative dispute resolution mechanisms to bypass the civil court system altogether, at least in some cases. Examples of alternative mechanisms include arbitration agreements and tribunals (especially, the National Company Law Tribunal). Finally, there is the distinction between district courts and State High Courts. The data on average trial duration used in this analysis comes exclusively from District/Sessions Courts. However, in cases where the value of a contractual dispute is above some monetary threshold, State High Courts have immediate jurisdiction over disputes. Thus, for the very largest firms, the efficiency of State High Courts may be the more relevant object. The population of firms included in this study is representative of the formal manufacturing sector, the vast majority of which is made up of relatively small firms (e.g. the median firm size is close to 20), for whom district courts are likely to be the court of first appeal.[9]

The fact that firms can sometimes specify trial locations outside of their residence or enter into arbitration agreements that bypass the civil court system does not necessarily pose a problem for the analysis in this paper. These alternative avenues may function to weaken

[9]The above discussion is greatly informed by communications with several advocates and legal scholars based in India. I am especially grateful to Nikunt K. Raval, Pallavi Gopinath Aney, Shubhankar Dam and Shreehari Aney for their time and help in clarifying the matters above.

the relationship between local court efficiency and economic performance, but they should not cause a bias in the opposite direction. In fact, this paper finds a positive effect of local court efficiency on economic performance *in spite* of the availability of alternatives to local courts. If anything, the effect would be stronger in the absence of such alternatives.

A related point can be made regarding firms' choices over organizational form. It has been hypothesized that poor contract enforcement may incentivize firms to vertically integrate the production process by moving some transactions inside the firm (e.g., Acemoglu et al. (2007) and Acemoglu et al. (2009)). To the extent that vertical integration insulates firms from the need for efficient contract enforcement, such behavior should also work against finding positive results. However, it should be noted that vertical integration - as a response to poor contract enforcement - is still suboptimal compared to a first best world with efficient contract enforcement (Grossman and Hart (1986)). As such, one would expect efficiency gains from improvements in the contract environment even allowing for vertical integration.

3 Data

3.1 Data on Court Efficiency, Contract Intensity and Firm Outcomes

The data used in the analysis come from several sources. The primary outcome variables of interest pertain to India's formal manufacturing sector and include growth in real gross value added, real fixed capital, employment and the total number of factories.[10] The number of factories is used to capture net entry. These variables are taken from India's Annual Survey of Industries (ASI) over the period 1998/9 - 2007/8. The ASI is a factory level survey which is meant to be representative of the entire registered manufacturing sector (i.e. all manu-

[10]Gross value added is adjusted for inflation using price indices for the manufacturing sector from India's Index of Industrial Production (IIP). Fixed capital is deflated using a capital goods index from the IIP. Both series have 2005 as their base year.

facturing enterprises that are registered with the government).[11] I note here that India has a large unregistered manufacturing sector, which will be left out of the present analysis. This omission should not greatly change the conclusions of the analysis - for two reasons. First, although much smaller in employment terms, the registered manufacturing sector accounts for a disproportionate share (about two-thirds) of total manufacturing output in India (Amirapu and Subramanian (2015)). Second, units in the unregistered sector are less likely to make use of the formal legal system than units in the registered sector due to legal fixed costs and the illegal nature of some unregistered units. Now, it is possible that the efficiency of courts is a determinant of the size of the informal sector in the first place. While recognition of this possibility should not change the validity of my results for the formal sector, it is nevertheless an interesting possibility which I hope to study in later work.

The data on court efficiency are obtained at the state-year level from annual "Crime in India" Reports, published by India's National Crime Records Bureau. Among the data available from this report is information on the duration of cases brought to trial in various types of lower courts (i.e. courts at the district level, below the state High Courts). The focus of the report is on criminal rather than civil trials, and the types of courts for which data are supplied include mostly those that handle criminal cases exclusively (especially, those courts presided over by Judicial Magistrates). However, the report also provides data pertaining to the "Court of the District and Sessions Judge", the highest court at the district level, which handles both civil and criminal cases. My primary measure of court efficiency is therefore the fraction of cases resolved within one year in the District/Sessions Court. As a placebo test, I will also consider the fraction of cases resolved within one year by the other types of courts (i.e. those that handle criminal cases exclusively). The expectation is that the speed of criminal courts should be a less robust predictor of growth in contract intensive industries than the speed of courts that handle civil cases.

[11] Although the data are originally at the factory level, I collapse the data at the state-industry-year level for most of my analysis below as the relevant variation occurs at this level.

The last important set of data are industry level measures of "contract intensity", taken from Nunn (2007). Nunn (2007)'s measures are based on the work of Rauch (1999), who categorized each of 1,189 industries (4-digit SITC Rev. 2 codes) according to whether the products could be bought on an international organized exchange, reference priced[12], or neither. If a product could be bought on an organized exchange, that was taken to indicate significant market thickness (i.e. a large number of buyers and sellers of the good) or a certain degree of homogeneity in the production of that good, and hence a low level of relationship-specificity.[13] A product that could be reference priced was assumed to have an intermediate degree of relationship-specificity, while products that could neither be bought on an exchange nor reference priced were assumed to have relatively thin markets and a high level of relationship-specificity. According to theoretical work such as Klein et al. (1978) and Williamson (1979), goods with thick markets or less relationship-specificity should be less susceptible to the problem of hold-up and therefore less "contract-intensive", while goods with thin markets or greater relationship-specificity should be most in need of enforceable contracts to guard against the threat of hold-up. Using Rauch's classification of goods according to their relationship-specificity, Nunn created the following industry level measure of "contract intensity" (z_i^{rs}), equal to the share of an industry's inputs that cannot be purchased on an organized exchange:

$$z_i^{rs} = \sum_j \theta_{ij}(1 - R_j^{org\ exchange})$$

Here, θ_{ij} is the value of input j divided by the total value of all inputs used by industry i,

[12] i.e. whether a price for the good could be found in a trade publication.

[13] While Rauch's classifications were developed to study international trade and hence are based on the existence or not of *international* organized exchanges, the manufacturing firms in my analysis include those that export as well as those that do not. Although it is possible that a domestic exchange may exist for a product while there is no *international* exchange for the product (or vice-versa), I assume that the existence or nonexistence of an international exhange is reflective of the relationship-specificity of a product in general - including when it is used in domestic transactions. If relationship-specificity at the international level is a noisy estimate of relationship-specificity at the domestic level, my estimates of the coefficient of interest will be biased downwards by measurement error.

and $R_j^{org\ exchange}$ is the proportion of input j that can be bought and sold on an organized exchange.[14]

Nunn created this measure for 381 industries classified according to the US Bureau of Economic Analysis (BEA) Input-Output (IO) industry classification. In order to use this measure in my analysis, I created a mapping between the US BEA IO industry codes to 5 digit Indian National Industry Classification (NIC) codes from 1998. In constructing the mapping, industries were mapped according to their titles and descriptions. In most cases, industry codes could be matched cleanly and with relatively little ambiguity regarding the match. Cases in which the mapping between industry codes was more uncertain were recorded as such and are left out in robustness tests. In some cases, no reasonable mapping could be made between industry codes. This happened either because the industry classification structures differed considerably or because certain products were unique to the US or Indian context (e.g., glass bangles and bidi cigarettes[15] in India). In these cases, such products were left out of the analysis altogether. An example of how this mapping was done is to be found in Table 15 of the Appendix.[16]

[14]Nunn also generated a measure of contract intensity based on the share of inputs that could neither be traded on an exchange nor reference priced:

$$z_i^{rs1} = \sum_j \theta_{ij}(1 - R_j^{org\ exchange} - R_j^{ref\ price})$$

where $R_j^{ref\ price}$ is the proportion of input j that is reference priced, and other variables are defined as above. I will use this alternative measure in robustness checks.

[15]Bidis are a type of small tobacco cigarette whose consumption in India outpaces that of standard cigarettes. Bidi manufacturing was not matched with standard cigarette manufacturing, because the production process for bidis is very different from that of standard cigarettes: most bidis are produced in the homes of women who roll them by hand before they are sold to wholesalers and small retailers. The production of bidis may thus require inputs with very different levels of relationship-specificity in comparison with cigarette manufacturing.

[16]Using data on the contract intensity of industries from an international setting and applying it to India has some advantages and some disadvantages. One advantage arises from the fact that the structure of production in India may be endogenous to the presence of poor contract enforcement in certain states. That is, industries primarily located in states with slow courts may change their production structure to avoid relying on relationship-specific inputs (as suggested by Boehm and Oberfield (2018)), which might bias measures of contract intensity constructed from local firm data. To the extent this is the case, it is preferable to use a measure of contract intensity that is untainted by local conditions. On the other hand, if the production structure of industries differs greatly between India and other countries, an international measure of contract intensity will be a noisy measure of the true contract intensity among Indian industries. To the extent that this is the case, the estimates of the main coefficients of interest in the paper will be downwardly biased.

After applying the mapping from NIC codes to BEA IO codes, 282,651 observations in the ASI between 1998/9 and 2007/8 (about 80% of the total) could be matched with a BEA IO code and corresponding "contract intensity" measure (about 70% of these matched observations were "certain" matches), of which there were 201 unique BEA IO codes represented in the dataset. Tables 2 and 3 display the 15 least and most contract intensive industries, respectively, among those industries present in the ASI and matched to NIC codes with strong confidence in the match.

3.2 Summary Statistics

Summary statistics for the primary variables used in the analysis are provided in Table 4. Panel A of the table includes the main state level variables in 1999, including "district court efficiency", which measures the fraction of trials resolved in less than one year in the District/Sessions Court.[17] Figure 3 shows the distribution of trial durations in 1999 and 2007 - towards the beginning and end of the period of this study. One can see that the mode trial duration is 1 - 3 years, while the second most frequent category is 3 - 5 years. A significant fraction of cases take 5 - 10 years (or longer) to be resolved. Perhaps most sobering is that the distribution of trial durations in 2007 shows no improvement from 1999.

The other state level variables reported in Panel A of Table 4 are used in the placebo and robustness checks. The variable "court efficiency (criminal)" records the fraction of cases that were resolved in less than one year in 1999 in district courts that hear criminal cases exclusively. "log NSDP pc" measures the log of net state domestic product per capita in 1999. The "literacy rate" is taken from 2001 population census data, while "road length pc (km)" gives the kilometers of paved roads in a state divided by the population, and is taken from the Ministry of Road Transport and Highways. The remaining state level variables pertain to various measures of trust and corruption. "corruption (TI)" gives the state level

[17] For ease of interpretation, the analysis employs a standardized version of the variable, district court efficiency (norm), which is transformed to have mean 0 and standard deviation 1.

"corruption score" as measured in a survey conducted by Transparency International in 2005 on individuals' perceptions and experiences of corruption in the public sector. The variables "WVS Trust" and "WVS People Fair" are taken from a wave of the World Values Survey conducted in India in 2001. These variables record the fraction of people in a state who answered in the affirmative that "most people can be trusted" and "most people ... try to be fair", respectively.[18]

Panel B of Table 4 presents basic information for industry level[19] variables including "contract intensity" (i.e. z_i^{rs}), the measure from Nunn (2007) described in detail above. The distribution of this variable is highly skewed (see Figure 5 in the Appendix). As a robustness check, the sample of industries is divided according to whether they are above or below the 25th percentile (about .85 on the measure), considering all those above the 25 percentile to be "contract intensive" and those below to be "not contract intensive". I do not use the 50th percentile (about .96) to divide the sample, since doing so would classify many industries as not contract intensive, even though they have virtually identical values of z_i^{rs} to the contract intensive industries (Figure 5 includes vertical lines depicting the 25th and 50th percentiles).

The remaining industry level variables are only used in robustness tests and include "capital intensity" along with two measures of "skill intensity". The variable "capital intensity" corresponds to the output elasticity with respect to capital (i.e. the capital coefficient on a Cobb-Douglas production function) estimated using the method of Levinsohn and Petrin (2003) on plant level ASI data.[20] The variables "skill intensity (primary att)" and "skill intensity (secondary att)" measure the fraction of workers in an industry with at least primary or at least secondary education, respectively, as measured using data from the 1999/2000 National Sample Survey Organisation's Employment and Unemployment Survey.

[18]The actual wording of the questions asked in the survey is as follows. WVS Trust is based on the question, "Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?", where answers are coded as 1 or 0, respectively. WVS People Fair is based on the question, "Do you think most people would try to take advantage of you if they got a chance, or would they try to be fair?", where the answers are recoded so that a value of 1 is awarded to respondents who chose "try to be fair".

[19]Recall that industries are defined according to their BEA IO categories (see previous subsection).

[20]Estimation is done separately by (BEA IO) industry.

While the independent variables used in this analysis vary either at the state or industry level, the dependent variables are defined at the state x industry level. These variables (summarized in Panel C of Table 4) are constructed by summing either real gross value added, real fixed capital, employment or numbers of factories within each state x industry x year cell, and then calculating annual compounded growth rates over the 9 year period between 1999 and 2008.

3.3 The Distribution of Economic Activity by Court Efficiency and Contract Intensity

If efficient courts matter more for some industries than others, one should expect to see more contract intensive economic activity located in states with higher court efficiency, and indeed this is the case. Table 5 shows - separately for states with above versus below median court efficiency - the fraction of output, employment and number of establishments in contract intensive industries, where the latter is defined to include all industries with z_i^{rs} above the 25th percentile. The outcome data are generated from the 1998/99 wave of the ASI, which is the starting year of the analysis and the first year for which I have data. The table shows that a greater fraction of economic activity took place in contract intensive industries for states with higher court efficiency compared to those with lower court efficiency. This result is to be expected given the hypothesized mechanism, but it is hardly conclusive of the importance of formal courts - states with faster courts may be different in any number of ways, any one of which may be responsible for attracting or encouraging growth in contract intensive industries. For this reason, the next section will introduce a more demanding empirical test.

4 Main Results

4.1 Theoretical motivation and Identification Strategy

Before turning to the results, let us revisit the theoretical argument that underpins the identification strategy. The argument, based on a large body of theoretical work in contract theory and organizational economics (including Klein et al. (1978), Williamson (1979) and Grossman and Hart (1986)), is that when economic transactions involving relationship-specific investments take place in an environment characterized by incomplete (or unenforceable) contracts, hold-up can occur. The threat of hold-up dissuades efficient ex-ante investment and can even deter some transactions from ever taking place, even though they would potentially increase surplus (e.g., Blanchard and Kremer (1997)). Firms may be incentivized to insulate themselves from poor contract enforcement by shifting transactions inside the firm, but ex-ante investment under vertical integration is still inefficient compared to the first best with complete contracts (Grossman and Hart (1986)), and excessive vertical integration may have other negative consequences, as discussed below.

The implication from above is that the benefit of being located in a state with a better contracting environment should be greater for firms engaged in industries that require more relationship-specific inputs - what Nunn (2007) calls "contract-intensive" industries - than for firms in industries that don't use many relationship-specific inputs, because these latter firms are less exposed to the threat of hold-up and hence less reliant on contracts and courts to protect them from this threat. Significantly, this benefit is likely to accrue over a period of time, affecting future growth as well as contemporaneous outcomes. This may be due to a number of reasons. First, the process of changing production structure or organizational form to take advantage of good contract enforcement is likely to take time: firms cannot instantaneously find new suppliers, change their production process to use a different mix of inputs, or move a transaction outside the firm. But the literature on trust and delegation within firms suggests another mechanism with implications for dynamic consequences. Bloom et al. (2012) pro-

vide evidence that firms headquarted in areas with high trust are more likely to decentralize and delegate authority, because they fear opportunistic behaviour less.[21] Building on this, Akcigit et al. (2016) show that the inability to delegate due to low trust or poor contract enforcement can lead to lower *growth*, as innovative firms with increasing productivity run into constraints related to their limited span of control, which not only holds back their own future performance but also allows poor performers to remain in the market.

An improvement in court efficiency may thus impact growth in several distinct ways. First, it may improve the efficiency of potential transactions between firms, allowing them to change the structure of their production over time to incorporate more relationship-specific inputs. Second, it may allow firms to grow by delegating more authority within the firm - if faster courts make it easier to punish opportunistic behaviour within the firm. Third, given that firms are constrained in their ability to delegate authority to managers, efficient enforcement of contracts between firms may reduce the cost of vertical disintegration, allowing them to grow by undertaking more transactions outside the firm, and thus relaxing the constraint on their span of control.

These mechanisms suggest that court efficiency at one point in time should have an effect on firm behavior and outcomes over time, rather than only a contemporaneous effect on the level of outcomes. The empirical strategy, then, is to regress subsequent growth of firms in a state-industry cell on an interaction between industry level contract intensity and state level judicial efficiency. This analysis, in the mode of Rajan and Zingales (1998), employs the following specification:

$$g_{sj} = \beta CourtEfficiency_s * ContractIntensity_j + \gamma_s + \delta_j + \varepsilon_{sj}, \tag{1}$$

where γ_s and δ_j represent state and industry fixed effects, and g_{sj} represents the annualized growth rate of various outcome variables (gross value added, fixed capital, numbers of em-

[21] In India, where trust is relatively low and contract enforcement is poor, Bloom et al. (2013) show that the number of male family members is a strong predictor of firm size, as would be expected if firms are constrained in their willingness to delegate managerial responsibilities outside the family.

ployees and numbers of factories - the latter used to measure net entry) in state s and industry j over the period 1999 - 2008.[22] In the analysis below, $CourtEfficiency_s$ is proxied for by the fraction of cases in a state's District/Sessions Courts that are resolved in less than one year, as measured at the start of the time period (i.e., in 1999). $ContractIntensity_j$ is the industry level measure of relationship-specificity from Nunn (2007) described in Section 3. To make the results easier to interpret, both of these two independent variables have been standardized to have a mean of 0 and a standard deviation of 1.

The coefficient on the interaction term, β, is the parameter of interest. Main interaction terms are excluded in all but the preliminary results, because of the inclusion of state and industry fixed effects. The addition of state and industry fixed effects allows one to argue that, in order for the analysis to be biased by omitted variables, it is not sufficient that these omitted variables be correlated with the state level measure of court efficiency or the industry level measure of contract intensity. Rather, they must be correlated with *the interaction* between state level court efficiency and industry level contract intensity. The existence of such omitted variables is certainly possible: for example, if corruption were negatively correlated with court efficiency and had a particularly detrimental effect on the growth of contract intensive industries, the results would be biased if corruption were to be left out of the regression. In order to provide some assurance that the findings are not being driven by such effects, I perform a variety of robustness and placebo tests in Section 5.

4.2 Main Empirical Results

4.2.1 Preliminary and Primary Results

Before discussing the main results of the paper - presented in Panel B of Table 6 - let us briefly consider the results of a preliminary specification that includes the main terms of the

[22] In addition to the theoretical reasons given above, there are two other reasons for focusing on the effect of court efficiency on the *growth* of outcome variables rather than on levels. First, focusing on growth over the subsequent time period makes the analysis less susceptible to the possibility of reverse causality. Second, there is not enough variation in levels of court efficiency across time to use panel data methods.

interaction instead of state and industry effects, because the coefficients on these terms are of some independent interest. The results (see Panel A of Table 6) show that the interaction between the state level measure of court efficiency and the industry level measure of contract intensity is indeed positive and statistically significant at the 5% level in all cases. The coefficients are also of large magnitude. For example, the results suggest that a 1 standard deviation increase in the fraction of cases resolved in District/Sessions Courts (e.g., going from 0 to 1 on the standardized measure of court efficiency) for an industry in the 75th percentile of contract intensity (standardized contract intensity $\approx .590$), would imply a higher annual growth rate of gross value added by 0.9 percentage points. The mean value of annual growth in gross value added for a state-industry cell over this time period is 2.0 percentage points, so an increase of 0.9 would constitute an increase of almost 50% for the average state-industry.

The effect is almost as large for fixed capital (column 2) and somewhat smaller for employment and net entry (columns 3 and 4), though still of substantial magnitude. As one might expect, the coefficients on the main term for court efficiency are also positive and significant, although one should be cautious in interpreting these coefficients due to the likelihood of omitted variables at the state level. Nevertheless, these results are consistent with the interpretation that fast courts are good for growth in all industries - but especially those industries that are more reliant on efficient contract enforcement.

Panel B of Table 6 presents the results of the primary specification, which now include state and industry fixed effects in order to better address the threat of omitted variable bias. The results show that the interaction of court efficiency and contract intensity is still a strong predictor of growth in gross value added, fixed capital, employment and net entry. Although the geographical variation in the speed of courts is still not assumed to be exogenously determined, the inclusion of state and industry fixed effects guarantee that the result is not driven by omitted variables at the state or the industry level alone. Nevertheless, this specification does not rule out the possibility of bias due to omitted variables at the state X industry level. For this reason, I will attempt to consider and address a number of potential threats to

the identification strategy in the next section of the paper. Before that, however, I will first present a number of robustness tests in order to establish that the above result is not unique to a particular specification or sample.

4.2.2 Basic Robustness Tests

In the regressions reported above, and in most of what follows, the sample consists of all firms in the ASI (collapsed by industry) that could be matched by NIC code to BEA IO codes in all Indian states and union territories (UTs). In 1999 there were 32 states and union territories. However, a number of UTs and some states have extremely small populations and economies (relative to the average state) and as such may act as outliers driving the results. To be sure this is not the case, I present results in Panel A of Table 7 that restrict the sample to only those industries located in the 20 largest states (by gross state domestic product). As can be seen, in all cases, the coefficients are similar or somewhat larger than was found in the main results.

I noted in Section 3 (on Data) that when matching 5 digit NIC codes to BEA IO codes, some industries could be matched with confidence, others could be matched with less confidence and some could not be matched at all. To be certain that the results are not being driven by inappropriately matched industry codes, I rerun the main specification restricting the sample to only those industries that could be matched with confidence. The results in Panel B of Table 7 are encouraging: limiting the sample to exclude less certain matches produces similar or substantially strengthened results. This test also provides some assurance that the results are not due to great differences in technology or industrial organization between industry categories in India and the US (for which the z_i^{rs} indicator was originally constructed). If that were the case, one would expect the results to be weaker when removing the unsure matches, for which such differences are likely to be greatest.

Another potential concern is related to the measure of contract intensity, the distribution of which is significantly skewed (see Figure 5). To make sure that the results are not driven

by some aspect of this skewness, I rerun the analysis using a binary measure of contract intensity, for which only those industries above the 25th percentile are classified as contract intensive.[23] The results of this regression, shown in Panel C of Table 7, demonstrate that the main result holds. As a final check on the robustness of this way of measuring contract intensity, I use an alternative measure of contract intensity from Nunn (2007). This measure, z_i^{rs1} in Nunn (2007), captures the share of an industry's inputs that can neither be purchased on an organized exchange nor reference priced in a trade publication. The results from using this alternative measure and a binary version of it (analagous to the binary version of the primary measure) are shown in Panels D and E. The results are less strong than those for the primary measure, but they are still significant and of substantial economic magnitude for some of the dependent variables - and none of the estimated coefficients are significantly different from the corresponding coefficients in the main results at the 5% level.[24]

So far the analysis has focused on measures of contract intensity from Nunn (2007). To check whether the results are driven by these measures in particular, I replicate the main specification using four alternative measures of contract intensity or "institutional dependence" from Levchenko (2007). All four measures aim to capture

> ... product complexity based on intermediate good use. Intuitively, institutions are more important to industries that require joining of a relatively large number

[23] I use the 25th percentile (which corresponds to about .85 on the contract intensity measure) because it roughly divides the sample into two groups: those with very high contract intensity, and those with less than high contract intensity. Using the 50th percentile to divide the sample is more problematic. The 50th percentile corresponds to a very high absolute measure of contract intensity (about .96) and leaves many industries with values of contract intensity that are almost high: half of the industries with "low contract intensity" under this division would have values between .85 and .96. Such a division would not adequately distinguish high and low values of contract intensity. See Figure 5, where the vertical lines represent the 25th and 50th percentiles.

[24] That the results seem somewhat less strong for this measure (i.e. z_i^{rs1}) may suggest that reference priced inputs are more contract intensive in India than in the US. It is not entirely clear why this might be the case, but here is one *possible* explanation: if the use of trade publications to provide reference prices were more efficient or reliable in the US than in India (which might be the case if, perhaps, such publications were in wider use or supply in the US), such goods would be effectively less contract intensive in the US than in India - because the potential for hold-up and related problems would be alleviated to a greater extent by the publications. In this case, an index which counts reference priced inputs as contract intensive - such as the primary measure used in this paper - would be more accurate in the Indian case than one which counts reference priced inputs as *not* contract intensive (e.g. z_i^{rs1}). However, I am not aware of a source of reliable information on the use of trade publications across countries and thus must emphasize that this explanation is only conjecture.

> of parties to production, simply because there are more relationships that are potentially distorted due to imperfect institutions. (Levchenko (2007))

The four measures include: 1) a Herfindahl index of intermediate input use, 2) the share of an industry's 20 largest intermediate inputs, 3) a Gini coefficient of intermediate good use, and 4) the number of intermediates/1000. The results using each of these as an alternative measure of industry-level "contract intensity" are shown in Panels A, B, C and D (respectively) of Table 8.[25] Measures 1), 2) and 3) are inverted so that higher values correspond "to industries with dispersed and even intermediate use pattern" (Levchenko (2007)), making them more likely to be contract intensive. All measures are also scaled to have mean 0 and standard deviation 1. The results are strongest for the Gini coefficient measure of contract intensity and for regressions of growth of fixed capital, but they are broadly positive and significant for most specifications. Moreoever, the coefficients are not significantly different from those in the main results in nearly every case. Thus, we may conclude that the results do not rely on the particular measures of contract intensity from Nunn (2007).[26]

A last series of robustness tests pertain to the issue of inference. In all regressions thus far performed, outcomes are defined at the state-industry level and standard errors are not clustered. However, if one is concerned that outcomes may be correlated across industries in a state (or across states within industries), one may wish to cluster standard errors along either or both dimensions. The results in Table 9 aim to assuage such concerns. In Panel A, standard errors are clustered by state, allowing for arbitrary correlation of the error term within a state. Similarly, Panel B reports results when clustering by industry. Panel C implements two-way cluster robust standard errors, allowing for errors to be correlated along both state and industry dimensions simultaneously (see Panel B).[27] The results are robust to all such specifications.

[25] The number of observations is lower in this table because not all industries could be matched cleanly with the industry codes from Levchenko (2007).

[26] I thank an anonymous referee for suggesting this line of analysis.

[27] These results were implemented using the user-written ado file for Stata "cluster2.do" (Petersen (2009))

One potential concern with the specifications depicted in Panels A and C is that the number of states is relatively small (less than 30). Donald and Lang (2007) show that clustering by group can lead to standard errors that are systematically downward biased when the number of groups is small. They suggest a two-step procedure for more accurate inference in such cases. In this case, their procedure amounts to estimating the following two equations in turn:

1. $g_{sj} = \sum_s \Gamma_s StateDummy_s * z_j^{rs} + \lambda IndDummies_j + \varepsilon_{sj}$
2. $\hat{\Gamma}_s = \beta CourtEfficiency_s + \alpha_s$

In the first step I estimate growth in a state-industry (g_{sj}) against a full set of state dummies - each of which is interacted with industry level contract intensity (z_j^{rs}). The regression also includes industry dummies to control for differences in average industry level growth rates. The coefficient on each interaction (Γ_s) captures the extent to which contract intensive industries grow faster or slower *in that state*. In the second step, these estimated coefficients are regressed against state level court efficiency. The parameter β should capture the extent to which states with higher court efficiency have faster growth in contract intensive industries - just as my primary specification is meant to do. The results of this procedure are reported in Panel D of Table 9, and remain significant - even after thus allowing for errors to be correlated within states. Another virtue of this specification is that it allows for easy visualization of the relationship of interest: the second stage is a bivariate relationship and is depicted in Figure 4. The strong positive relationship is exactly what one would expect. Having demonstrated that the results pass the robustness tests above, I now turn to an examination of the possible threats to identification.[28]

[28] One last robustness check - not included here - excludes the top and bottom 1% and 5% growth outliers, neither of which changes the results appreciably.

5 Threats to Identification, Further Robustness Checks and Placebo Tests

5.1 Possible Threats to Identification and Robustness Checks

Recall the main estimating equation (eqn 1) from above:

$$g_{sj} = \beta CourtEfficiency_s * ContractIntensity_j + \gamma_s + \delta_j + \varepsilon_{sj},$$

Since I claim no source of demonstrably exogenous variation in state level court efficiency or industry level contract intensity, I must take concerns regarding omitted variable bias seriously. As noted previously, I am aided by the addition of state and industry fixed effects, which preclude the possibility of omitted variables at either the state or industry level alone biasing the results. Nevertheless, the fixed effects do not, by themselves, preclude the possibility that there exist omitted variables correlated with *the interaction* between court efficiency and contract intensity, and which also effect the economic performance of registered manufacturing firms.

One way this could happen is if court efficiency is correlated with other state level features that interact positively with industry level contract intensity - or with industry characteristics correlated with contract intensity. Another way in which the results may be biased is if contract intensity is correlated with other industry level characteristics that interact positively with court efficiency (or other state level attributes correlated with court efficiency). My approach to dealing with this issue is to explicitly consider as many such potential threats as possible and to control for them one by one. In particular, these robustness tests will amend the main specification (eq. 1) by including additional state level characteristics interacted with industry level contract intensity or additional industry level characteristics interacted with state level court efficiency.[29] To summarize the results of this exercise, the coefficients

[29] As in the primary specification, the main terms will be omitted due to the inclusion of state and industry

on court efficiency X contract intensity are very robust to the inclusion of a variety of state X industry controls, lending additional confidence to the hypothesized mechanism (see Tables 10 to 12).

I begin by considering a number of alternative mechanisms associated with different features of the state environment. The first alternative mechanism I consider is the interaction between (logged) net state domestic product per capita and industry level contract intensity. High state income is a likely correlate of good institutions generally (e.g., property rights, stable local government, positive social norms), and it is possible that some other institution associated with rich states - apart from the courts - is important for contract intensive industries. To the extent that this is the case, including an interaction between state income and contract intensity should dampen the coefficient on court efficiency X contract intensity. The results, presented in Panel A of Table 10, show that this is not the case. The inclusion of this interaction has almost no effect on the main coefficient of interest.

Panel B of Table 10 includes an interaction between state level literacy rates and industry level contract intensity[30]. The concern that this control is meant to assuage is that contract intensive industries might grow faster in states with higher average levels of education, which may be more likely to have faster courts - but the results suggest otherwise. Another possibility is that good physical infrastructure is particularly important for contract intensive industries - which may be true if, for example, contract intensive industries are more likely to make use of state infrastructure for trade. If physical infrastructure is correlated with court efficiency, this could bias the results. Panel C of Table 10, which includes an interaction between the length of paved roads (km) per capita in 1999/2000 and contract intensity, suggests that this mechanism is not driving the results either.[31]

The next series of robustness tests take seriously the idea that levels of corruption and trust may vary by region - in a way that is correlated with court efficiency - and may play

fixed effects.

[30]The data on literacy rates are generated from the 2001 Population Census.

[31]The data on road length come from the Indian Ministry of Road Transport and Highways.

a particularly important role in contract intensive industries. This could be the case if using contracts requires not only the ability to formally enforce them, but also a high degree of informal trust in one's contracting partner. The regressions reported in Table 11 test this hypothesis, by adding controls for three measures of corruption, trust and perceived fairness - each interacted with contract intensity in each of the three panels. In Panel A, the measure of state level corruption used is the "Corruption Score", generated by the corruption watchdog agency Transparency International from a 2005 survey of households on their perceptions and experiences of corruption in the public sector. Higher scores reflect higher values of corruption. Panels B and C include interactions with state level measures of trust aggregated from responses to questions from the 4th wave of the World Values Survey (WVS) conducted in India in 2004. In Panel B, the measure averages answers to the question, "Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?", where answers are coded as 1 (most people can be trusted) or 0 (need to be careful). In Panel C, the measure averages answers to the question "Do you think most people would try to take advantage of you if they got a chance, or would they try to be fair?", where answers are either 1 (try to be fair) or 0 (take advantage). These interactions are occasionally significant determinants of growth in themselves, but in no case do they appreciably change the magnitude of the main coefficients of interest.[32]

Finally, I consider the possibility that certain industry level characteristics may be correlated with contract intensity but may, independently, interact positively with state level court efficiency in determining the economic performance of firms. The two industry characteris-

[32]Interestingly, the coefficient on the first measure of trust has a negative sign. This may reflect certain problems with the measure that have been discussed in psychology literature. For example, Miller and Mitamura (2003) explain that "... despite its popularity, the validity of this question has not been confirmed, and results based on it sometimes have contradicted other results...". They offer the following suggestion to explain the problematic nature of the measure: "The question under investigation does not ask respondents to choose between trust and distrust, as is implied in virtually all of the research citing results from this question. Rather, respondents choose between trust and caution. Yet there is reason to believe that trust and caution are not opposites: certainly it is possible for a person to believe most people can be trusted, and at the same time believe that it is prudent to be cautious." (Miller and Mitamura (2003)) Therefore, the negative coefficient for this variable may be interpreted to mean that contract intensive industries do better when located in states in which people are more likely to exercise caution.

tics I consider are the capital intensity and the skill intensity of an industry. The potential concern regarding the former is that capital intensive industries may be more likely to be contract intensive (e.g., large investments in capital may encourage the use of special inputs tailored to the machinery), and court efficiency may be important to capital intensive industries for other reasons (e.g., perhaps it is easier to finance the purchase of capital if contracts are easier to enforce). I test this hypothesis in Panel A of Table 12 by including an additional control for industry level capital intensity interacted with court efficiency. The measure of capital intensity used corresponds to the output elasticity with respect to capital (i.e. the capital coefficient on a Cobb-Douglas production function) estimated using the method of Levinsohn and Petrin (2003) on plant level ASI data.[33] As before, the additional control has almost no effect on the coefficients of interest.

The other potentially confounding industry characteristic I consider is skill intensity. This characteristic may be important to control for if it is the case that contract intensive industries tend to be more skill intensive *and* that skill intensive industries require well-functioning courts to prosper more than other types of industries. This hypothesis is tested in Panels B and C of Table 12 using two proxy measures of skill intensity: the fraction of workers in an industry with at least primary (Panel B) and at least secondary (Panel C) education, as measured using data from the 1999/2000 National Sample Survey Organisation's Employment and Unemployment Survey. Again, the coefficient on court efficiency interacted with contract intensity is unchanged by the inclusion of these further controls, suggesting that such a channel is not driving the results.

In the above I have tried to consider the most likely potential alternative mechanisms and systematically rule them out one by one. Of course, it may not be possible to rule out every conceivable potential alternative mechanism, but the robustness and consistency of the coefficients of interest throughout these tests should provide a greater degree of confidence that the result is not spurious. As a final test of robustness, I add all of the state and industry

[33] Estimation is done separately by (BEA IO) industry.

controls discussed above - with their corresponding interactions - simultaneously, to see if some combination of factors may explain away the observed effect. The results, displayed in Table 13, show that while the coefficient of interest in the regression of gross value added is smaller and loses significance, the coefficients in the other 3 regressions are are even larger than without the controls, and maintain significance in two of the specifications. In the next subsection, I perform a placebo test with the goal of providing further assurance that the hypothesized mechanism is correct.

5.2 Placebo Test: Efficiency of Criminal Courts

The placebo test I consider takes advantage of the fact that there is trial duration data for different types of lower courts. Up until this point, the analysis has exclusively used duration data pertaining to the Court of the District and Sessions Judge, which hears both civil and criminal cases. Indeed, in communications with lawyers and legal scholars based in India, this seems to be the court that would be most likely to hear a contract dispute between two privately owned firms[34] over an alleged breach of contract. However, the Crime In India Report makes available court duration pertaining to a number of other lower courts: Additional Session Judge, Chief Judicial Magistrate, Judicial Magistrate (I), Judicial Magistrate (II) and Special Judicial Magistrate. All of these courts hear criminal cases exclusively. Therefore, I perform a robustness test by replacing the previous measure of court efficiency (the fraction of cases resolved by the District/Session Judge) with a new measure: the fraction of cases resolved in all types of lower courts, except the Court of the District/Sessions Judge. Since the new measure reflects the efficiency of criminal courts and not civil ones, it should not impact the performance of firms concerned about contract enforcement - except in so far as

[34]That is, if they are sole proprietorships or partnerships and have not made alternative arrangements (such as arbitration). Suits pertaining to companies may be more likely to end up in front of the Company Law Board. In the ASI data over the period 1999-2008, about 50% of plants are part of sole proprietorships or partnerships, about 27% are part of private limited companies and about 18% are part of public limited companies. Very significant cases may be heard directly by State High Courts. For further discussion on this topic, see Section 2.4.

criminal court efficiency is correlated with the efficiency of civil courts.

The results of this test are provided in Table 14 below. Indeed, it appears that the type of court considered does matter - speedy resolutions in criminal courts are *not* associated with faster growth in contract intensive industries. This can be taken as strong evidence in favor of the hypothesized mechanism: if the effect of civil courts is being driven by an omitted factor that is correlated with court efficiency, this factor would need to be correlated with civil court efficiency - *but not* criminal court efficiency. It is difficult to think of what such a factor could be.

6 Conclusion

In spite of wide recognition that good institutions generally are important for the promotion of growth, there is less clarity on the relative importance of constituent institutional components. The value of high quality formal judicial institutions in particular has been disputed. Some have argued that high quality formal judicial institutions are important (e.g., Berkowitz et al. (2006); Nunn (2007); Levchenko (2007); Chemin (2012)) while others have argued that they are not (Acemoglu and Johnson (2005)), suggesting that informal arrangements may serve as substitutes. Furthermore, most of the papers on the topic so far have used cross-country data, which are vulnerable to concerns regarding endogeneity and omitted variables.

In this paper I test whether efficiently functioning formal judicial institutions - as measured by the speed of courts - are important for the growth of output, fixed capital, employment and net entry in the Indian registered manufacturing sector. I use state level variation in the average duration of trials in district courts (an objective measure of court efficiency) and industry level variation in the need for contract enforcement in order to identify the effect in question. The evidence suggests that fast courts are a significant determinant of growth among formal manufacturing firms in India. In particular, the point estimates suggest that, for an industry in the 75th percentile of contract intensity, an improvement of one standard

deviation in court efficiency would imply a higher annual growth rate of gross value added by 0.9 percentage points (or 50% of the average value). The within-country setting for the analysis allows me to perform a battery of robustness and placebo tests, which demonstrate the robustness of the results and make them hard to explain via alternative mechanisms. Based on this analysis, it seems that informal contracting arrangements provide only a partial substitute for the formal court system, and that India would therefore enjoy significant economic benefits if it could improve the efficiency of its courts.

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Tables

Table 1: Structure of Courts in India

Federal Level	Supreme Court	
State Level	High Courts	
District Level (lower courts)	District/Sessions Court	
	Civil Courts Additional District Judge's Court, Senior Civil Judge's Court, Principal Junior Civil Judge's Court, Junior Civil Judge's Court	**Criminal Courts** Chief Judicial Magistrate's Court, First Class Judicial Magistrate's Court, Second Class Judicial Magistrate Court, Special Judicial Magistrate's Court

Note: This table depicts the hierarchical structure of courts in India. The data used to measure court efficiency in this study apply to District/Sessions Courts.

Table 2: 15 Least Contract Intensive Industries

NIC industry code	BEA IO industry code	industry description	contract intensity (z_i^{rs2})
15311	311211	Flour milling	.0959204
15321	311221	Wet corn milling	.1461177
15312	311212	Rice milling	.1506271
16001	312210	Tobacco stemming & redrying	.189198
15114	311615	Poultry processing	.2295379
20211	32121A	Veneer & plywood man.	.481376
20109	321113	Sawmills	.5495412
15111	311611	Animal, except poultry, slaughtering	.5900722
16003	312221	Cigarette man.	.5941259
21011	322110	Pulp mills	.6158003
15201	311514	Dry, condensed, & evaporated dairy products	.6278917
15116	311612	Meat processed from carcasses	.6304269
36912	339910	Jewelry & silverware man.	.6401256
27320	33152B	Nonferrous foundries, except aluminum	.6643769
24114	325130	Synthetic dye & pigment man.	.6725274

Note: This table presents the 15 least contract intensive industries, according to the measure from Nunn (2007), among those industries present in the ASI and matched to NIC codes with strong confidence in the match. Sources: Nunn (2007).

Table 3: 15 Most Contract-Intensive Industries

NIC industry code	BEA IO industry code	industry description	contract intensity (z_i^{rs2})
30006	334111	Electronic computer man.	.9995985
15440	311823	Dry pasta man.	.9994706
36991	322233	Stationery & related product man.	.9994073
34104	336110	Automobile & light truck man.	.9978275
22219	323116	Manifold business forms printing	.997521
22121	511110	Newspaper publishers	.9974136
34101	336120	Heavy duty truck man.	.9969729
32301	334300	Audio & video equip. man.	.9969063
22110	511130	Book publishers	.9963905
22122	511120	Periodical publishers	.9962443
30007	334119	Oth. computer peripheral equip. man.	.9949551
26915	327113	Porcelain electrical supply man.	.9941305
32204	334210	Telephone apparatus man.	.9940286
30009	333313	Office mach. man.	.9936688
35301	336411	Aircraft man.	.9910538

Note: This table presents the 15 most contract intensive industries, according to the measure from Nunn (2007), among those industries present in the ASI and matched to NIC codes with strong confidence in the match. Sources: Nunn (2007).

Table 4: Summary Statistics

	count	mean	sd	min	p50	max
Panel A:						
State Level Variables						
district court efficiency	30	0.230	0.225	0.000	0.166	0.877
district court efficiency (norm)	30	0.000	1.000	-1.021	-0.284	2.872
court efficiency (criminal)	32	0.320	0.242	0.000	0.296	0.927
log NSDP pc	35	9.916	0.476	8.774	9.852	11.109
literacy rate	36	69.431	10.645	47.000	68.725	90.860
road length pc (km)	32	0.004	0.003	0.001	0.003	0.017
corruption (TI)	20	4.890	1.048	2.400	4.935	6.950
WVS Trust	18	0.390	0.171	0.098	0.384	0.756
WVS People Fair	18	0.413	0.187	0.157	0.381	0.756
Panel B:						
Industry Level Variables						
contract intensity	195	0.878	0.169	0.096	0.956	1.000
contract intensity (norm)	195	0.000	1.000	-4.632	0.465	0.722
capital intensity	195	0.236	0.086	0.032	0.229	0.465
skill intensity (primary att)	189	0.733	0.243	0.000	0.787	1.000
skill intensity (secondary att)	189	0.458	0.286	0.000	0.401	1.000
Panel C:						
State x *Industry Level Variables*						
growth rate of gross value added	1908	0.020	0.218	-0.703	0.004	1.380
growth rate of fixed capital	2175	-0.029	0.245	-0.731	-0.055	4.563
growth rate of employment	2176	0.024	0.157	-0.532	0.017	0.925
growth rate of num factories	2185	-0.003	0.106	-0.343	0.000	0.759

Note: This table presents summary statistics for the primary variables used in the analysis. Variables are grouped according to whether they vary at the state level (Panel A), the industry level (Panel B), or the state x industry level (Panel C). Court efficiency is measured by the fraction of cases resolved within one year in the State or Union Territory's District/Sessions Court, and is presented both in raw and standardized form. The same is true of the paper's measure of contract intensity, which is taken from Nunn (2007). The dependent variables vary at the state x industry level, and include annualized growth in gross value added, fixed capital, employment and the number of establishments, between 1999 and 2008. All values reported in Panel A are from 1999, except literacy (2001) and the corruption measure from Transparency International (2005). Sources: Annual Survey of Industries, National Crime Records Bureau, Nunn (2007).

Table 5: Distribution of Economic Activity by Court Efficiency and Contract Intensity in 1999

	States with High Court Efficency	States with Low Court Efficiency
Fraction of Real Output in Contract Intensive Industries	0.554	0.471
Fraction of Employment in Contract Intensive Industries	0.520	0.390
Fraction of Establishments in Contract Intensive Industries	0.497	0.366

Note: This table depicts the fraction of output, employment and number of establishments in contract intensive industries in states with high versus low court efficiency using ASI data from 1999. Contract intensive industries are defined to be those with a measure of contract intensity (based on Nunn (2007) as described in the text) greater than the 25th percentile, while states with high court efficiency are defined to be those in which the fraction of cases resolved within one year was above the 50th percentile in 1999. Sources: Annual Survey of Industries (1998/99), National Crime Records Bureau, Nunn (2007).

Main Results and Basic Robustness Tests

Table 6: Industry Growth and Court Efficiency (Main Results)

	(1) growth in value added	(2) growth in fixed capital	(3) growth in employment	(4) growth in num units
Panel A: Main Terms Only				
court efficiency	0.014**	0.011**	0.017***	0.011***
	(0.006)	(0.005)	(0.004)	(0.002)
contract intensity	0.003	-0.002	0.001	-0.004**
	(0.005)	(0.006)	(0.003)	(0.002)
court efficiency X contract intensity	0.015***	0.013**	0.009***	0.005**
	(0.005)	(0.005)	(0.003)	(0.002)
Panel B: State and Industry FEs				
court efficiency X contract intensity	0.015***	0.014***	0.009***	0.006***
	(0.005)	(0.005)	(0.003)	(0.002)
Observations	1709	1939	1940	1947

Note: This table presents the main results of the paper. Panel A contains a preliminary specification, depicting regressions of growth in gross value added, fixed capital, employment and the number of establishments in a state-industry cell against state level court efficiency, industry level contract intensity and their interaction. The coefficient on the interaction is the main coefficient of interest. Panel B presents the primary specification, replacing the main terms in the above regression with state and industry fixed effects (not depicted). In this and all further regressions, growth is measured between 1999 and 2008 and is annualized. Court efficiency is measured by the fraction of cases resolved within one year in the State or Union Territory's District/Sessions Court while the measure of Contract Intensity is taken from Nunn (2007). Heteroskedasticity robust standard errors are reported in parentheses. *, **, and *** indicate significance at 10%, 5% and 1% levels respectively. Sources: Annual Survey of Industries, National Crime Records Bureau, Nunn (2007).

Table 7: Basic Robustness Tests

	(1) growth in value added	(2) growth in fixed capital	(3) growth in employment	(4) growth in num units
Panel A: Major States				
court efficiency X contract intensity	0.017***	0.014**	0.012***	0.007***
	(0.005)	(0.006)	(0.004)	(0.002)
Observations	1473	1662	1663	1670
Panel B: Confident Matches				
court efficiency X contract intensity	0.019***	0.016***	0.011***	0.006**
	(0.006)	(0.005)	(0.004)	(0.002)
Observations	1186	1339	1340	1347
Panel C: Binary CI Measure				
court efficiency X binary contract intensity	0.237***	0.231***	0.139***	0.098***
	(0.081)	(0.080)	(0.053)	(0.033)
Observations	1709	1939	1940	1947
Panel D: Alternative CI Measure				
court efficiency X contract intensity (alt)	0.012**	0.011	0.003	0.001
	(0.006)	(0.007)	(0.004)	(0.003)
Observations	1709	1939	1940	1947
Panel E: Binary Alt CI Measure				
court efficiency X binary contract intensity (alt)	0.181**	0.198**	0.054	0.053
	(0.083)	(0.084)	(0.055)	(0.036)
Observations	1709	1939	1940	1947

Note: This table replicates the main results using different specifications. Panel A includes only the 20 largest states (by net state domestic product), Panel B includes only those industries with NIC codes that could be matched to BEA IO codes with high confidence, Panel C includes a binary measure of contract intensity. Panel D includes an alternative measure of contract intensity from Nunn (2007), corresponding to the share of an industry's inputs that can neither be purchased on an organized exchange nor reference priced in a trade publication. Panel E includes a binary variable based on the alternative measure of contract intensity in Panel D. As before, court efficiency is measured by the fraction of cases resolved within one year in the State or Union Territory's District/Sessions Court while the measure of Contract Intensity is taken from Nunn (2007). State and industry fixed effects are included but not shown, and heteroskedasticity robust standard errors are reported in parentheses. *, **, and *** indicate significance at 10%, 5% and 1% levels respectively. Sources: Annual Survey of Industries, National Crime Records Bureau, Nunn (2007).

Table 8: Robustness Tests (Levchenko Complexity Measures)

	(1) growth in value added	(2) growth in fixed capital	(3) growth in employment	(4) growth in num units
Panel A: Herfindahl Index				
court efficiency X herfindahl index	0.011 (0.009)	0.010* (0.006)	0.005 (0.005)	0.006** (0.003)
Observations	1015	1136	1137	1142
Panel B: Share of 20 Largest Interm				
court efficiency X share of 20 largest interm	0.013 (0.008)	0.021*** (0.007)	0.009* (0.005)	0.006* (0.003)
Observations	1015	1136	1137	1142
Panel C: Gini Coefficient				
court efficiency X gini coefficient	0.015* (0.009)	0.020*** (0.007)	0.010* (0.005)	0.006* (0.003)
Observations	1015	1136	1137	1142
Panel D: Number of Intermediates				
court efficiency X no. of intermediates	0.009 (0.008)	0.019** (0.008)	0.009 (0.006)	-0.003 (0.003)
Observations	1015	1136	1137	1142

Note: This table replicates the main results using four alternative measures of industry-level contract intensity or "institutional dependence" from Levchenko (2007). Panel A measures contract intensity using a Herfindahl index of intermediate input use, Panel B uses the share of an industry's 20 largest intermediate inputs, Panel C uses a Gini coefficient of intermediate good use, and Panel D uses the number of intermediates/1000. The measures in Panels A, B and C are scaled so that higher values correspond "to industries with dispersed and even intermediate use pattern", making them more likely to be contract intensive. All measures are scaled to have mean 0 and standard deviation 1. See Levchenko (2007) for more details on the computation and interpretation of these measures. As before, court efficiency is measured by the fraction of cases resolved within one year in the State or Union Territory's District/Sessions Court, State and industry fixed effects are included but not shown, and heteroskedasticity robust standard errors are reported in parentheses. *, **, and *** indicate significance at 10%, 5% and 1% levels respectively. Sources: Annual Survey of Industries, National Crime Records Bureau, Nunn (2007), Levchenko (2007).

Table 9: Inference-related Robustness Tests

	(1) growth in value added	(2) growth in fixed capital	(3) growth in employment	(4) growth in num units
Panel A: State Clustering				
court efficiency X contract intensity	0.015*** (0.005)	0.014** (0.005)	0.009*** (0.003)	0.006*** (0.002)
Observations	1709	1939	1940	1947
Panel B: Industry Clustering				
court efficiency X contract intensity	0.015*** (0.005)	0.014*** (0.005)	0.009** (0.004)	0.006** (0.002)
Observations	1709	1939	1940	1947
Panel C: Two-Way Clustering				
court efficiency X contract intensity	0.015*** (0.005)	0.014*** (0.005)	0.009** (0.004)	0.006** (0.002)
Observations	1709	1939	1940	1947
Panel D: Donald Lang Two Step				
court efficiency	0.014*** (0.003)	0.012*** (0.003)	0.005* (0.003)	0.005** (0.002)
Observations	26	26	26	26

Note: This table presents the results of several different inference-related robustness tests. In Panel A, robust standard errors are clustered by state, allowing for arbitrary correlation among errors across industries within a state. In Panel B, standard errors are clustered by industry, allowing for correlation within an industry. In Panel C, standard errors are clustered by state and industry - following Petersen (2009) -, allowing for correlation in the error term across both dimensions simultaneously. In these specifications, the number of states is relatively small (less than 30), so that the results when clustering by state may be inaccurate. Panel D therefore reports the results from the second stage in a two step procedure suggested by Donald and Lang (2007) to deal with inference when the number of groups is small. As before, state and industry fixed effects are included, and heteroskedasticity robust standard errors are reported in parentheses. *, **, and *** indicate significance at 10%, 5% and 1% levels respectively. Sources: Annual Survey of Industries, National Crime Records Bureau, Nunn (2007).

Additional Robustness Tests: Ruling Out Alternative Mechanisms

Table 10: Including Additional Controls: State Environment X Contract Intensity

	(1) growth in value added	(2) growth in fixed capital	(3) growth in employment	(4) growth in num units
Panel A: Ln NSDP per cap				
court efficiency X contract intensity	0.015*** (0.005)	0.016*** (0.005)	0.009*** (0.003)	0.005** (0.002)
ln NSDP pc X contract intensity	-0.000 (0.012)	-0.015 (0.013)	-0.008 (0.007)	-0.004 (0.004)
Observations	1637	1856	1857	1864
Panel B: Literacy				
court efficiency X contract intensity	0.015*** (0.005)	0.015*** (0.005)	0.009*** (0.003)	0.006*** (0.002)
literacy X contract intensity	0.000 (0.001)	-0.000 (0.001)	-0.000 (0.000)	-0.000 (0.000)
Observations	1709	1939	1940	1947
Panel C: Road Length				
court efficiency X contract intensity	0.013*** (0.005)	0.014*** (0.005)	0.009*** (0.003)	0.006*** (0.002)
road length pc X contract intensity	4.413 (3.526)	0.292 (2.887)	-0.535 (1.800)	-0.344 (1.320)
Observations	1709	1939	1940	1947

Note: This table replicates the main results while adding several control variables to capture various features of the state environment that may interact with contract intensity. Panel A includes an interaction between logged net state domestic product per capita and contract intensity, Panel B includes an interaction between the state literacy rate and contract intensity, and Panel C includes an interaction between the length of paved roads per capita and contract intensity. As before, state and industry fixed effects are included, and heteroskedasticity robust standard errors are reported in parentheses. *, **, and *** indicate significance at 10%, 5% and 1% levels respectively. Sources: Annual Survey of Industries, National Crime Records Bureau, Nunn (2007), Reserve Bank of India, 2001 Population Census, Ministry of Road Transport and Highways.

Table 11: Including Additional Controls: State Corruption/Trust X Contract Intensity

	(1) growth in value added	(2) growth in fixed capital	(3) growth in employment	(4) growth in num units
Panel A: Corruption Score				
court efficiency X contract intensity	0.018**	0.019**	0.016***	0.010***
	(0.007)	(0.008)	(0.004)	(0.003)
TI corruption X contract intensity	-0.004	-0.010	-0.003	0.001
	(0.006)	(0.009)	(0.004)	(0.002)
Observations	1269	1422	1425	1429
Panel B: WVS Trust				
court efficiency X contract intensity	0.015***	0.016**	0.013***	0.009***
	(0.006)	(0.007)	(0.004)	(0.003)
WVS Trust X contract intensity	-0.017***	-0.014**	-0.007**	-0.002
	(0.006)	(0.006)	(0.004)	(0.002)
Observations	1310	1480	1481	1486
Panel C: WVS People Fair				
court efficiency X contract intensity	0.012**	0.012*	0.012***	0.008***
	(0.006)	(0.007)	(0.004)	(0.003)
WVS Fair X contract intensity	0.013**	0.016***	0.006**	0.002
	(0.005)	(0.006)	(0.003)	(0.002)
Observations	1310	1480	1481	1486

Note: This table replicates the main results while adding control variables to capture any interaction between differences in corruption or trust at the state level and contract intensity at the industry level. Panel A includes an interaction between the perception of corruption from Transparency International (2005) and contract intensity, while Panels B and C include interactions between trust (as measured in two different ways from the World Values Survey) and contract intensity. As before, state and industry fixed effects are included, and heteroskedasticity robust standard errors are reported in parentheses. *, **, and *** indicate significance at 10%, 5% and 1% levels respectively. Sources: Annual Survey of Industries, National Crime Records Bureau, Nunn (2007), Transparency International (2005), World Values Survey.

Table 12: Including Additional Controls: Court Efficiency X Industry Characteristics

	(1) growth in value added	(2) growth in fixed capital	(3) growth in employment	(4) growth in num units
Panel A: Capital Intensity				
court efficiency X contract intensity	0.014*** (0.005)	0.015*** (0.006)	0.009*** (0.003)	0.005** (0.002)
court efficiency X capital intensity	-0.080 (0.074)	0.054 (0.068)	-0.033 (0.044)	-0.023 (0.028)
Observations	1709	1939	1940	1947
Panel B: Skill Intensity (1)				
court efficiency X contract intensity	0.014*** (0.005)	0.014*** (0.005)	0.009*** (0.003)	0.005*** (0.002)
court efficiency X skill intensity (primary)	0.062** (0.031)	0.012 (0.030)	0.019 (0.022)	0.002 (0.014)
Observations	1672	1900	1901	1908
Panel C: Skill Intensity (2)				
court efficiency X contract intensity	0.014*** (0.005)	0.014** (0.006)	0.009** (0.003)	0.005** (0.002)
court efficiency X skill intensity (secondary)	0.074*** (0.025)	-0.001 (0.032)	0.024 (0.018)	0.005 (0.011)
Observations	1672	1900	1901	1908

Note: This table replicates the main results while adding several control variables to capture various characteristics of industries that may interact with state level court efficiency. Panel A includes an interaction between court efficiency and a measure of industry capital intensity, while Panels B and C include an interaction between court efficiency and two measures of industry level skill intensity. As before, state and industry fixed effects are included, and heteroskedasticity robust standard errors are reported in parentheses. *, **, and *** indicate significance at 10%, 5% and 1% levels respectively. Sources: Annual Survey of Industries, National Crime Records Bureau, Nunn (2007), National Sample Survey Organization.

Table 13: Including All Additional State and Industry Level Controls With Interactions:

	(1) growth in value added	(2) growth in fixed capital	(3) growth in employment	(4) growth in num units
court efficiency X contract intensity	0.009	0.016	0.015***	0.010***
	(0.007)	(0.010)	(0.005)	(0.003)
ln NSDP pc X contract intensity	–0.015	0.043	–0.020	–0.007
	(0.038)	(0.044)	(0.022)	(0.016)
literacy X contract intensity	0.001	–0.002	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
road length pc X contract intensity	2.398	5.582	–1.879	0.316
	(5.672)	(5.346)	(2.701)	(2.044)
TI corruption X contract intensity	0.010	–0.016	0.002	0.002
	(0.010)	(0.016)	(0.007)	(0.004)
WVS Trust X contract intensity	–0.018**	–0.007	–0.010*	–0.002
	(0.009)	(0.009)	(0.005)	(0.003)
WVS Fair X contract intensity	0.017**	0.023***	0.006	0.003
	(0.007)	(0.008)	(0.004)	(0.003)
court efficiency X skill intensity (primary)	–0.141**	–0.024	–0.056	–0.040
	(0.069)	(0.061)	(0.045)	(0.031)
court efficiency X skill intensity (secondary)	0.163***	0.006	0.041	–0.003
	(0.054)	(0.078)	(0.037)	(0.025)
court efficiency X capital intensity	–0.060	0.121	0.035	–0.043
	(0.095)	(0.109)	(0.067)	(0.046)
Observations	1162	1301	1304	1307

Note: This table replicates the main results while adding all control variables from the previous tests simultaneously. State level control variables are interacted with industry level contract intensity, while industry level control variables are interacted with state level court efficiency. As before, state and industry fixed effects are included, and heteroskedasticity robust standard errors are reported in parentheses. *, **, and *** indicate significance at 10%, 5% and 1% levels respectively. Sources: Annual Survey of Industries, National Crime Records Bureau, Nunn (2007), Reserve Bank of India, 2001 Population Census, Ministry of Road Transport and Highways, Transparency International (2005), World Values Survey, National Sample Survey Organization.

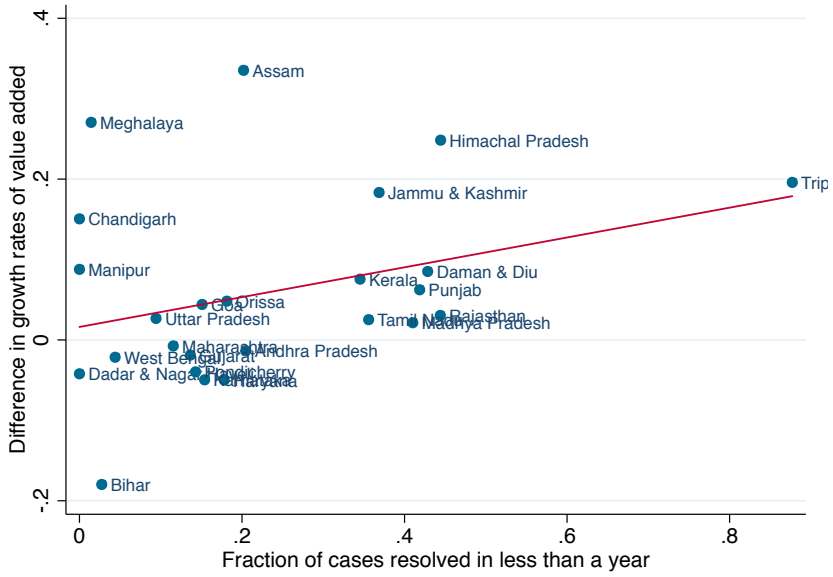
Table 14: Placebo Test - Industry Growth and Criminal Court Efficiency

	(1) growth in va	(2) growth in fix cap	(3) growth in emp	(4) growth in num units
court efficiency	0.005	0.002	0.004	0.003*
(criminal) X contract intensity	(0.006)	(0.006)	(0.003)	(0.002)
Observations	1801	2044	2045	2052
Sample	All States	All States	All States	All States

Note: This table replicates the main results using a measure of court efficiency in courts that handle exclusively criminal cases, with the expectation that court efficiency in such courts should not be relevant in explaining economic performance of firms in contract intensive industries. As before, state and industry fixed effects are included, and heteroskedasticity robust standard errors are reported in parentheses. *, **, and *** indicate significance at 10%, 5% and 1% levels respectively. Sources: Annual Survey of Industries, National Crime Records Bureau, Nunn (2007).

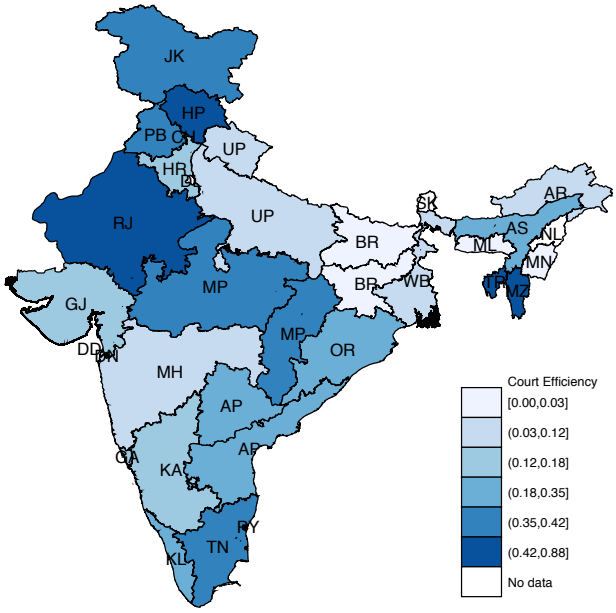
Figures

Figure 1: Difference in Growth Rates of Value Added versus Court Efficiency



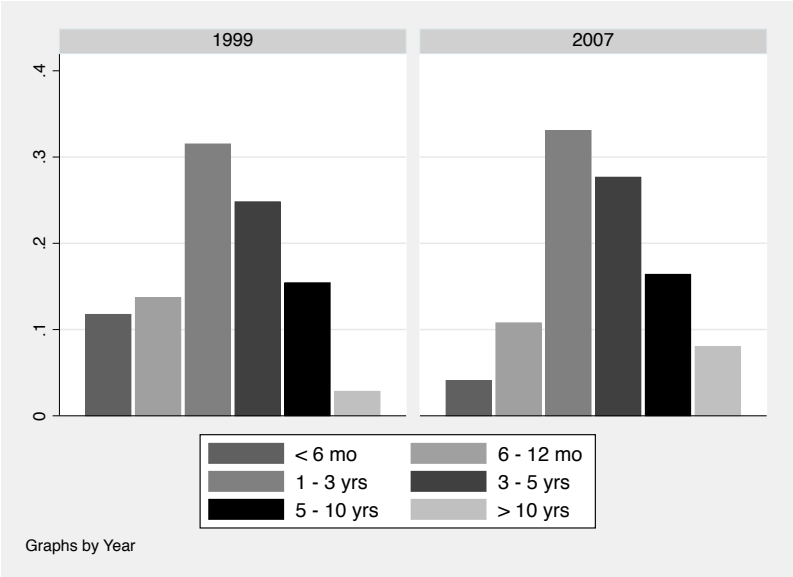
Note: This figure, in the mode of Rajan and Subramanian (2011), provides a way of visualizing the paper's main results. For each state, I first calculate the difference in average growth rates of gross value added between contract intensive and non-contract intensive industries, and then plot the difference against state level court efficiency on the x-axis. Contract intensive industries are defined to be those with a measure of contract intensity above the 25th percentile. Court efficiency is measured as the fraction of cases that are resolved within one year in the States' District/Sessions Courts in 1999. Sources: Annual Survey of Industries, National Crime Records Bureau, Nunn (2007).

Figure 2: Court Efficiency Across Indian States



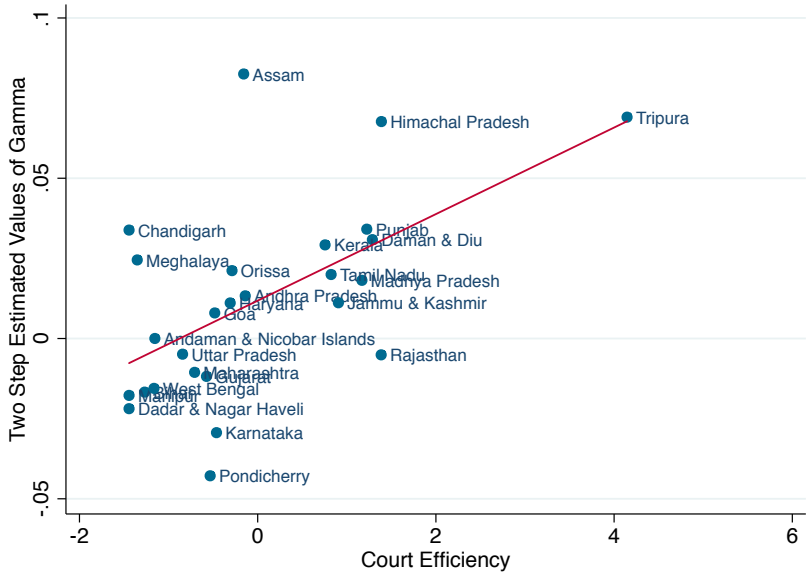
Note: This figure displays a map of court efficiency across Indian states, with darker blue representing more efficient courts. Court efficiency is measured as the fraction of cases that are resolved within one year in the States' District/Sessions Courts in 1999. Source: National Crime Records Bureau.

Figure 3: Fraction of Cases by Case Duration (District/Session Judge)



Note: This figure displays the distribution of cases heard by District/Sessions Courts according to their duration in 1999 and 2007, towards the beginning and end of the study. Source: National Crime Records Bureau.

Figure 4: Contract Intensive Growth in Value Added versus Court Efficiency Second Stage from Donald and Lang (2007)



Note: This figure presents the second stage of the two-step procedure from Donald and Lang (2007) and provides an alternative way of visualizing the paper's main results. The y-axis measures state-specific estimates of the correlation among industries between growth of value-added and contract intensity, controlling for industry fixed effects. The x-axis depicts state level court efficiency, measured as the fraction of cases that are resolved within one year in the States' District/Sessions Courts in 1999. See Section 4 for further details. Sources: Annual Survey of Industries, National Crime Records Bureau, Nunn (2007).

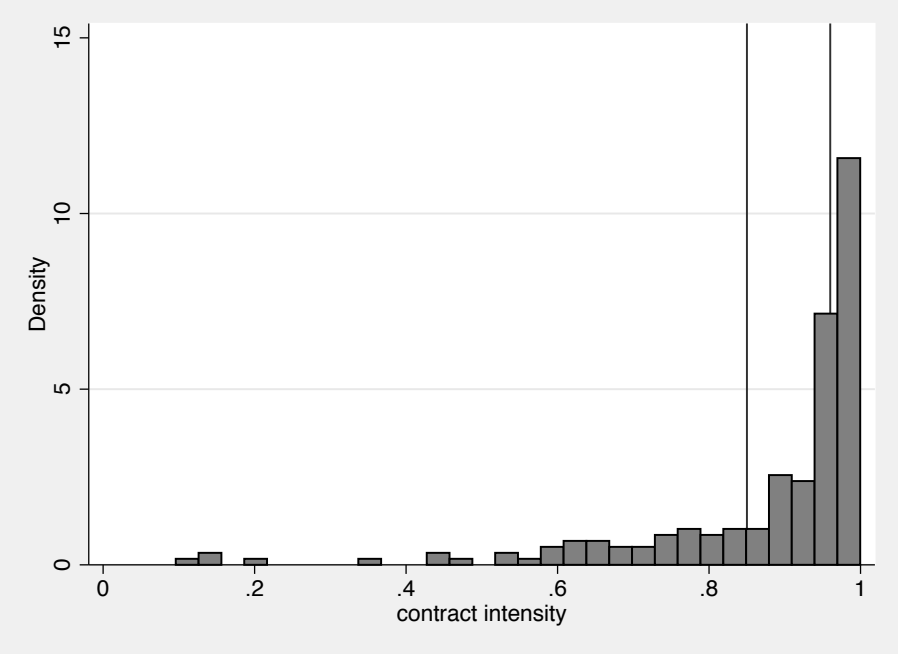
A Appendix: Additional Tables and Figures

Table 15: Example of concordance between NIC and BEA IO codes

Match	NIC code	NIC description	BEA IO code	BEA IO description
yes	16001	Tobacco stemming, redrying etc. of tobacco leaf	312210	Tobacco stemming & redrying
yes	16003	Manufacture of cigarette and cigarette tobacco	312221	Cigarette man.
no	-	-	312229	Oth. tobacco product man.
no	16002	Manufacture of bidi	-	-
no	16004	Manufacture of cigars and cheroots	-	-
no	16008	Manufacture of pan masala and related products	-	-
yes	2413	Manufacture of plastics in primary forms and of synthetic rubber	325211	Plastics material & resin man
yes	24131	Manufacture of synthetic rubber in primary forms	325212	Synthetic rubber man
unsure	24133	Manufacture of cellulose and its chemical derivatives in primary form	325221	Cellulosic organic fiber man

Note: This table presents an example of the mapping between the US BEA IO industry codes and Indian NIC (1998) codes. In constructing the mapping, industries were mapped according to their titles and descriptions. In most cases, industry codes could be matched cleanly and with relatively little ambiguity regarding the match. Examples of such cases includes the rows with "yes" in the Match column. Cases in which the mapping between industry codes was more uncertain were recorded as such ("unsure" in the Match column). In some cases, no mapping could be made between industry codes with any degree of confidence ("no" in the Match column). This happened either because the industry classification structures differed considerably or because certain products were unique to the US or Indian context (e.g., bidi cigarettes and pan masala in the above). Such products were left out of the analysis altogether.

Figure 5: Distribution of Contract Intensity Variable (1999)



Note: This figure displays the distribution across industries of the variable "contract intensity" from Nunn (2007). The vertical lines depict the 25th and 50th percentiles. Source: Nunn (2007).