Justice delayed is growth denied: Evidence from a comparative perspective*

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VERY PRELIMINARY DRAFT, PLEASE DO NOT CIRCULATE

Abstract

Institutions have been widely studied as drivers of countries' economic development. In the present work we investigate how the performance of judicial systems affects property rights' protection and contract enforcement. We focus on judicial timeliness as an objective proxy of courts' performance on a panel of 175 countries. The hypothesis being tested suggests that slower courts hinder economic development. We employ several econometric specifications and techniques in order to estimate a plausibly unbiased effect of judicial delay on economic growth.

JEL Classifications: O43, K41, H4.
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1 Introduction

In the aftermath of the 2008 financial crisis, governments all around the world have been struggling to restore confidence in economic transactions and thus reignite economic development. However, while no "magic recipe" guarantees this outcome, economists tend to unanimously agree that the proper enforcement of contracts and the protection of property rights are a necessary condition for economies to prosper (Acemoglu et al., 2001; Glaeser et al., 2004; Rodrik et al., 2004). At the same time, at least since the late nineties, international organization as the World Bank (for example with its "Doing Business" Project) or the European level (with the CEPEJ commission) have been trying to stress the importance of improving the functioning of national judicial systems as a key factor to foster economic development. Many countries have thus been trying to reform their domestic justice sector. Interestingly, not only developing countries have been struggling with this policy issue. On the contrary, it has been shown that poor indicators of judicial performance trouble indistinctly rich and developing countries.

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Judiciaries and economic growth are thus much more related that one might think. Property rights protection is not so solely determined by the characteristics of law on the books (*de jure* institutions). Also the way the law is enforced matters (*de facto* institutions) and this task is ultimately performed by the judiciary. In the present study we wish to highlight the link between the functioning of the judiciary (its speed in particular, as measured by judicial delay) and economic growth. After having motivated the theoretical channels through which a fast judiciary ought to be beneficial for economic development we will perform an empirical analysis exploiting both temporal and spatial variation in a cross-country dataset of 175 nations.

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While this is not the first time that a similar issue has been proposed by scholars, our work proposes several original contributions to the literature. First of all, we wish to shed light on the very concept of judicial performance. Economic literature has so far often made confusion between various ideas as efficiency and delay, often referring to both indistinctly as synonyms. Second, we will focus on

a very narrow dimension of judicial performance, judicial delay, and explain why this measure captures to a better extent the functioning of the judiciary as some more generic indicators of contract enforcement or respect of the Rule of Law. Third, we try to fill a gap in literature as, to our very best knowledge, no previous study has attempted to causally link judicial delay and economic growth across countries and over time. Despite endogeneity problems are a common problem of cross-country studies wishing to show the causality of institutions on economic performance (Chemin, 2009), we propose to shift the most advanced methodological innovations to this topic to motivate the robustness of our results. Previous econometric analyses correctly identify the impact of judicial delay on the economy. However this has come at the cost of focusing on a very narrow perspective (within-country), exploiting some exogenous changes in the performance of the judiciary's performance. Nonetheless it is equally necessary to supply a comparative analysis of how judicial systems' functioning can contribute explaining the variance in economic prosperity across the world. In this perspective the only possible solution is cross-country analysis. Our contribution is to use an "objective" measure of judicial delay rather than some index of contracts enforcement perceptions. Furthermore, we are able to account on data regarding multiple years in order to exploit not only cross-sectional variance but also within-country time variance.
//check out the contributions with respect to Marciano et al 19
The remaining of the paper is structured as follows. Section 2 presents the theoretical framework with respect to the importance of enforcing institutions and, more specifically the delay of justice, for economic growth. Section 3 describes our empirical strategy, data employed and results obtained. Conclusions are drawn in Section 4 .

2 Enforcing Institutions and Economic Development

A relatively non-controversial principle in modern economics is that property rights need to be protected in order to foster economic performance (Marciano et al., 2019). Most literature has focused on how the historical evolution of legal systems

(common law *vs.* civil law) might explain today's variance in countries' prosperity. Not only the evolutionary nature of judge-made-law proper of common law system allows legal institutions to better adapt to changes in societies (Hayek, 1960). It has also been shown that common law legal systems supply a better environment for the development of financial markets (Beck et al., 2003; La Porta et al., 1998; Levine et al., 2000; Mahoney, 2001; Massenot, 2011). However, these results do not allow to account for the sometimes substantial differences in economic development existing today amid countries sharing the same legal tradition. That is, how come two countries supported by a very similar legal system might differ to a great extent when it comes to property rights protection?

//EXAMPLES??? ||| difference between necessary and sufficient condition

Only more recently scholars have started to pay attention also to the way legal systems are actually enforced. The necessity of embracing a similar perspective is due to the fact that even the most efficiently designed rule will at best be ineffective if not properly enforced. Accordingly, not only formal laws (or otherwise said *de jure* institutions) must be enquired, but equally the way these are enforced (*de facto* institutions) (Hodgson, 2006; Voigt, 2013). If institutions are to be considered as "humanly devised constraints" (North, 1990), the effectiveness of such constraining effect depends directly on such *de facto* institutions which are ment to implement them (Safavian and Sharma, 2007). If this is true, it is necessary to shift scholarly attention on the judiciary. A "well functioning" court system will ease the establishment of new commercial relations, lowering barriers to entry and at the same time fostering markets' dynamics, thus ultimately enhancing economic growth[1].

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// change "fast" in "well performing"

But to what extent judiciaries affect the economy? Trading partners usually encourage suppliers to undertake investments by writing long-term contracts. How-

[1]National judiciaries are in charge of processing the vast majority of litigation. On the contrary, Alternative Dispute Resolution systems (ADRs) are a sort of niche limited to international trade and informal non-judicial methods of law enforcement based on repetitional mechanisms are plausible only in long-term relationships within small groups (Johnson et al., 2002; Dixit, 2003).

ever, in a world of incomplete contracts that cannot be fully enforced by courts (Grossman and Hart, 1986), once investment costs are sunk, there is an immediate incentive to renege on contractual obligations and try to capture the trading partner's rent. On the other side, if transaction costs associated to searching for new business partners are high, the supplier will try to use its monopoly power and impose higher prices (Chemin, 2009). Within this setting, judiciaries play an important role in limiting transaction costs and opportunistic behavior, by reducing uncertainty in economic interactions. Each time a case is brought to court, uncertainty arises with respect to the legal issues hereby litigated. Economic actors might behave opportunistically and exploit the incapacity of the judicial system to enforce contractual obligations (Williamson, 1985). A well-functioning judiciary acts as an important deterrent against economic agents' willingness to deviate from previously signed contracts. At the contrary, a flawed institutional mechanism devoted to enforce the law might in the short-run make more attractive similar alternatives, as the discounted value of future monetary (and sometimes non-monetary) punishments will necessarily drop. Similar opportunism would undermine economic transactions, as firms would not be willing to trust partners and offer them trade credit in their business transactions, as the likelihood that this credit would be repaid diminishes (Chemin, 2009). Not only transactions costs would thus arise in the event of a slow judiciary, but equally financing opportunities would be constrained. A deficient law enforcement will incentivize opportunism on borrowers' side: anticipating the difficulty that creditors will face when recovering their loans, debtors will be more incentivized to default. However, a vicious circle would push creditors themselves to anticipate borrowers' opportunistic behavior and consequently to reduce the availability of credit (Jappelli et al., 2005; Chemin, 2009) or to increase interest rates (Visaria, 2009). It has also been shown that post-contractual opportunism incentivized by judicial institutions equally affects firms' employment decisions (Autor, 2003) and markets' structure (Melcarne and Ramello, 2018).

Similar institutional frictions would dissuade an efficient level of *ex ante* investments, deterring some (mutually beneficial) transactions from even taking place. This should also help to explain why the overall effect of a slow judiciary on economic performance ought to be negative. It is undeniable that judicial delay might

impact to a different extent different economic actors. For example, Chemin (2009) claims that Justice's slowness should worsen the condition of poorest members of society and of courts' most inventive users. One could imagine that "unproductive" forms of entrepreneurship (Baumol, 1990) might benefit from flawed justice. However our prediction is that the overall effect of judicial institutions' performance on economic development ought to be negative. As we have seen, non-functioning enforcing mechanisms should prevent some otherwise lucrative economic transactions from even taking place. At the same time it is hard to think that, in modern economies, agents are purely creditors or debtors. Even opportunistic debtors, in order to sustain their economic activity, will need to face business relations that place them on the weak side with respect to judicial enforcement. As a consequence, even if greater judicial delay can be beneficial for some, this positive effect should be offset by its much larger detrimental consequences for economic transactions.

2.1 Judicial performance, efficiency & delay

// RIPHRASE!!! and differentiate from MMR
But when does a judicial system function well? Different possible dimensions related to judicial performance might be envisaged. For example, Staats et al. (2005) identify five different levels through which one might try to measure judiciaries' performance:i) independence, ii) efficiency, iii) accessibility, iv) accountability and v) effectiveness. Previous works (Voigt and Gutmann, 2013) have highlighted, for example, the importance of judicial independence in making the promise of respecting property rights credible. JD has been often used in previous literature as a synonym of judicial efficiency, while the two ought to be conceptually distinguished (Ippoliti et al., 2015; Marciano et al., 2019). This is particularly important from a public policy perspective. If one can agree that long delays are a symptom of an ineffective judiciary, this is not necessarily caused by an inefficient use of public resources (workforce and capital endowments). In this respect, different measures might not necessarily yield similar results: a given court might be very effective and thus solve cases quickly, but at the same time turn out to be inefficient because oversized. This means that a given court, although providing

a "good" service for society is not yet acting efficiently, thus yielding a waste of public resources. As a consequence, the choice of restructuring (either downsizing or incrementing) the magnitude of the judiciary might not necessarily be in line with a sharp empirical investigation of the way justice is actually organized.
//Explain better what's our research question below!
However, while the concept of (technical) efficiency is particularly important from a public policy perspective, especially in a time period during which governments all around the world have to face the necessity of dealing with public finance "austerity", what really matters for the purposes of this work is rather efficacy, as measured by JD. As highlighted by Marciano et al. (2019), we might say that, while policy-makers and bureaucrats are more interested by the efficiency of judicial systems, entrepreneurs are more affected by their timeliness. This is because what really matters for litigants is the time needed to dispose a case, not the way resources were allocated and employed. Of course this claim is not to be considered as an absolute one. For example, in order to guarantee an effective (fast) justice, the judiciary could use an excessive level of public resources to hire more judges than necessary: despite being effective, it would turn out to be inefficient. While beneficial for litigants, a similar excess of public expenditure would be detrimental in the long run for a nation's economic growth. The positive effect on economic transactions derived from an excessive, and yet effective, allocation of resources to the judiciary could be offset by the negative effects generated by an increase in public budget in the form of higher taxes. Two different aspects of our research design will allow our empirical analysis not to suffer from a similar problem. First of all, as judiciaries influence economic actors' behavior independently of their actual use (Chemin, 2012), we concentrate our attention on the short run effects on economic growth deriving from changes in JD. Second, our econometric models' specification will account for the level of a nation's level of public revenues.
The remainder of the paper is devoted to test empirically the hp...

3 Empirical Analysis

The empirical analysis devoted to shed light on the causal link between judicial delay and economic growth will be conducted on a cross-country dataset of 175

countries and for a panel of years (2004-2015). Accordingly, we will be able to exploit both spatial and temporal variation in JD in order to explain changes in gdp per capita growth rates. The baseline model will thus include both country and year fixed effects as well as a number of standard controls. While the use of country-dummies does not solve once for all endogeneity concerns, at least it mainly deals with the issue of "omitted variable bias". The baseline model we estimate is represented by Equation 1:

$$g_{i,t} = \beta JD_{i,t} + \mathbf{X}'_{i,t}\delta + \alpha_t + \gamma_i + \epsilon_{i,t} \tag{1}$$

where $g_{i,t}$ represents the yearly gdp per capita growth rate in country i in year t, JD is our measure of judicial delay (measured in years), $\mathbf{X}$ is a vector of control variables, α_t are year fixed effects, γ_i are country fixed effects and $\epsilon_{i,t}$ is the stochastic error. β is our coefficient of interest for which we expected negative values.

3.1 Judicial Delay

Our JD variable deserves a few words of explanation. The variable is extracted from the World Bank "Doing Business" Project, more precisely the *enforcing contract - time* measure. The methodology behind the measurement of this variable can be found in Djankov et al. (2003).

While using country-level data entails several endogeneity concerns that will be later addressed in Section 3.3.2, concentrating on a specific institutional dimension as judicial delay has some clear advantages when it comes to identify the impact of the judiciary on economic performance. First of all, JD is a fairly stable indicator of judicial performance, at least in the short run (Voigt and Gutmann, 2013). Because of the "ossification" of legal phenomena, substantial changes in judicial performance are the outcome of either reforms of the substantial law, the judiciary's organization or general litigation habits (Melcarne and Ramello, 2015). While the latter aspect will be more troublesome to deal with, legal and judicial reforms are not highly volatile and not strictly sensitive to changes in the socio-economic environment. Furthermore, they can easily be controlled for in an econometric model.

JD has also the advantage of representing an "objective" measure of judicial performance: the time needed to solve a case. Ideally, the institutional measure of judicial performance should be as independent as possible from our dependent variable. "Subjective" measures of judicial performance could be affected by economic conjuncture (Voigt and Gutmann, 2013). For example, metrics based on entrepreneurs' perception of judicial effectiveness, could be positively affected by previous economic growth, thus yielding biased results. Differently from other previous studies employing indexes reflecting subjective perception of the quality of legal enforcement granted by courts (Bose et al., 2012; Du et al., 2012; Lu and Tao, 2009; Safavian and Sharma, 2007), we are able to capture the effect of timely justice. However, as correctly emphasized by Voigt (2013), JD is not a "real" objective measure, but rather only an hypothetical one. This is because the World Bank does not collect the "real" estimated average time needed to solve a lawsuit. On the contrary, this measure is based on pundits' beliefs regarding the delay of a standardized simple case, that can be easily compared across countries. The subjectivity bias connected to our JD variable is thus partially compensated by the objective nature of what JD actually measures. However, we are fully aware that such bias is not completely taken control for, as such objective measures remain based on individual perceptions rather than on administrative data. Nonetheless, given the scarcity of reliable cross-country datasources, this represents a viable second-best solution for allowing a comparative analysis involving measures of judicial delay.

If on the one side the objective nature of JD yields the aforementioned advantages in terms of institutional measurement, on the other side it equally poses a potential problem, that has only been superficially dealt by previous literature. Delay does not necessarily capture the "qualitative" dimension of judges' work. XXXXXX [WRITE AGAIN–>] XXXXX One might imagine that a fast judiciary could in fact, not only do not at all protect property rights, but also, at the same time, hide a systematic violation of the Rule of Law. In other words, there might be a trade-off between judicial performance in its quantitative dimension (as we measure JD) and the intrinsic quality of justice as delivered by courts. Several reasons might motivate this concern. First of all a (qualitatively) "good" protection of property rights should make the judiciary more bounded to law and thus potentially slower

and less effective. If an ideal court should almost instantly resolve legal disputes (Djankov et al., 2003), the necessity of respecting procedural hurdles and other physical constraints necessarily slow down their work (Chemin, 2012). This could in fact be interpreted as a sort of *inverted-u* non-linear effect of judicial delay on economic growth: reducing delay to much might in fact be detrimental for the economy. The empirical analysis will try to deal with this potential non-linearity in JD.

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Another potential cause of concern is related to the interplay between the law and its enforcement. One might imagine that if a rule enjoys legitimacy within a given population, it needs less enforcement and thus longer delays might not necessarily imply the hypothesized causal relation (Buchanan et al., 2014). This aspect can be dealt by including country fixed effects in a regression. However, the same rationale cannot be applied to other situations. As said, a fast judiciary could in fact hide a systematic violation of the Rule of Law. We might think of a fast judiciary, but that is biased in favor of a certain (political, religious or ethnic) group. From a theoretical perspective, this could be dealt by strictly interpreting the well-known legal maxim: *Justice delayed is* - always (we add) - *justice denied*. The very etymological origin of the word "justice", comes from the latin *ius dicere*, state the law, something ultimately done by solving cases. Hypothetically one could imply that the an effective delivery of justice could itself be beneficial for economic actors, regardless the respect of fair procedures guiding judges' work. Alternatively, corruption and discrimination could work as institutions favoring the quick dispositions of lawsuit. If one assumes that, even if biased, justice is consistent and thus predictable, despite being deplorable, the hypothesized effect of delay should nonetheless remain valid. In this sense we would be facing a sort of "extractive" institution favoring economic growth (Acemoglu and Robinson, 2012). However, while this claim might seem quite provocative, the present work does not wish to look directly at the differences in economic performance when the quality and the quantity of justice are substitutes. We adopt a rather more conservative *ceteris paribus* approach trying to account for this potential trade-off. However, in this case using country-level fixed effects might not be sufficient as

in the considered time period the respect of Rule of Law might have substantially changed within different countries. For example, countries affected by the "Arab Spring", might have either improved (see Tunisia) or worsened (see Turkey) their respect for the Rule of Law and human rights. Accordingly we will include in all model specification an apposite control for Rule of Law.

3.2 Data Description and Preliminary Results

A description of all variables used and their source can be found in Table 1, while Table 2 presents descriptive statistics. All regressions include a vector ($\mathbf{X}$) of controls: the amount of total investments as a share of GDP (*invest*), the general level of government revenue as a share of GDP (*govrev*), yearly variation in population size (*pop*), lagged value of GDP per capita (*gdp*), an index of Rule of Law (*RoL*), the cost related to the use of the judiciary as a share of claim size (*cost*). Observation with extreme values in g were excluded from our analysis: respectively the 1^{st} ($g < -10\%$) and the 99^{th} ($g > 20\%$) percentiles.

[Table 1 about here.]

[Table 2 about here.]

[Table 3 about here.]

Table 3 shows our baseline estimates. In column (2) we include a full set of controls and both country and year fixed effects. We estimate a negative coefficient, in line with our prediction: *ceteris paribus*, higher levels of JD are negatively correlated with gdp growth rates. This implies that the longer it is needed to solve a legal disputes the lower a country's economy will grow. More specifically, the estimates in Table 3 imply that every additional year of judicial delay would lower annual growth rate by 1.3%.

While the magnitude of this coefficient might seem very big, it is worth noting that JD is not a very volatile measure and thus a change of 1 year in JD would be a very relevant one. As can be seen in Table 2, JD is bounded between 3.5 months and slightly less than 5 years, with a mean value of 1.7 years. Thus for a country with an average delay, decreasing JD by one year would imply an improve

in judicial performance by almost 60%. In Table 4 we report the highest values for a yearly change in JD and the associated gdp growth rate. We also report, if any, the kind of reform that could have determined an improvement in JD[2].

[Table 4 about here.]

3.3 Discussion

3.3.1 Robustness Checks

In order to stress the robustness of our baseline results, we estimated Equation 1 with respect to different sub-samples and using different specifications. Results can be found in Table 5.

[Table 5 about here.]

In columns (1) and (2) we estimate Equation 1 respectively only for countries belonging or not to the common law legal tradition. According to legal origin literature (La Porta et al., 1998; Levine et al., 2000; Mahoney, 2001), common law countries ought to be characterized by better institutions. However, we do not find substantial differences in sign and magnitude of our coefficients. In column (1) of Table 5, the estimated coefficient for JD turns out to be not statistically significant. However this is mainly due to the fact that the size of this sub-sample (only 32 countries) is substantially lower and so the power of our model.
COMMENT MORE
In column (3) we try to test the potential concerns proposed in Section 3.1 of a non-linear effect of JD on economic growth. Accordingly, we add a quadratic term to Equation 1. As emerges from our estimates, there seems to be no evidence of a non-linear effect.
COMMENT MORE
In column (4) we try to test whether excluding developed countries might impact our estimates. Our sub-sample thus includes only countries not member of the OECD: results are not affected.

[2]Information on reforms is available at `http://www.doingbusiness.org/reforms`

COMMENT MORE
Given the fact that the specific timespan we consider (2004-2015), contains observation both before and after the 2008 financial crisis, in column (5) we include in our baseline specification a dummy = 1 if the country/year data point is observed after the start of the crisis. Results turn out to be unaffected by this further control.
COMMENT MORE
In column (7), instead of using a yearly gdp growth rate, we use a 5 year growth rate in order to account for business cycles: results remain consistent.
In column (6) we exploit year-to-year variance. We thus estimate a first-difference model represented by Equation 2:

$$g_{i,t} - g_{i,t-1} = (JD_{i,t} - JD_{i,t-1})'\beta + (\mathbf{X}_{i,t} - \mathbf{X}_{i,t-1})'\delta + (\epsilon_{i,t} - \epsilon_{i,t-1}) \qquad (2)$$

Even with this different estimator, our coefficient does not dramatically change in magnitude and significance.

3.3.2 Endogeneity Concerns

While the results obtained in Section 3.3.1 allow us to reasonably exclude problems of omitted variable bias, the same cannot be concluded with respect to reverse causality. As mentioned above, endogeneity is a common problem with cross-national studies dealing with institutions. In fact one cannot rule out *ex ante* that the causal link between growth and JD might run backward: that is, economic development causes judicial delay.
On the one side, it could be that a positive causal relation links growth to JD. Previous studies have shown that (material) wellbeing is positively correlated with litigation rates (Eisenberg et al., 2013). ADD: greater wealth implies greater economic transactions and thus more possibility of litigation. In the short run, greater litigation would imply a "saturation" effect on courts, that would not be able to adjust in real time to increases in their dockets' size. All in all, this should cause an increase in JD. While affecting our estimates, we are less concerned about this potential channel of transmission, as it would imply that our causal effect of interest is simply underestimated.

Much more troublesome in econometric terms is the case in which growth negatively affects judicial delay. In this sense we might think of three possible mechanisms through which economic wellbeing might reduce JD. First of all, it has been shown that growth not only enhances material capital, but equally social one (Glaeser et al., 2004). If this is true, one might object that better educated people might be less inclined to litigate. Accordingly, the "saturation" effect would apply in opposite terms and determine a reduction in JD. With respect to this point, previous literature has emphasized that social capital needs time to accumulate and with country fixed effects we account for long run historical factors linking social capital to economic performance (Acemoglu et al., 2008). The same would not be true if one imagines that economic recession should increase the rate of defaults and thus the number of cases brought to court. Via the "saturation" effect, this could imply an increase in JD. A third transmission channel regards public expenditure. In times of economic expansion, governments will be able to increase their public expenditure and invest resources in the public sector. Accordingly, we define as an "endogenous" reform any policy intervention that enhances the judiciary's production capacity by increasing the resources at its disposal. The typical example would be the recruitment of extra judges or investments in physical capital as IT infrastructure or new court buildings.

[Table 6 about here.]

In order to overcome these issues, we propose several possible solutions in Table 6. In Column (1) we include a dummy variable = 1 if an "endogenous" judicial reform has took place in a given country/year. We code a reform as endogenous if either labor force was incremented or if new capital was invested.
COMMENT.
In column (2) we use a lagged regressor (JD_{t-1}). COMMENT.
In column (3) we employ an instrumental variable approach by means of 2SLS. We use the number of procedural steps necessary to conclude a lawsuit (*proc*). Our claim is that *proc* should affect growth only through JD: the more procedural steps are necessary to obtain a ruling the longer the delay. Since *proc* is time-invariant, we average all our variables. Furthermore, since Djankov et al. (2003) have shown that legal origins are a significant determinant of JD, we add to our

vector of controls a dummy variable accounting for whether a country belongs or not to the common law tradition. We also add a dummy = 1 if a country belongs to the OECD. Equations 3 and 4 represent respectively the first and second stage of our IV-2SLS model.

$$\text{first-stage: } JD_i = \sigma proc_i + \mathbf{X}_i'\phi + u_i \tag{3}$$

$$\text{second-stage: } g_i = \alpha\widehat{JD_i} + \mathbf{X}_{i,t}'\psi + e_i \tag{4}$$

COMMENT 2SLS

Finally, in column (4) we employ a dynamic panel model, more specifically, a system-GMM estimator (Arellano and Bover, 1995). This model uses lags of endogenous variable as instruments and has been often used in recent literature trying to estimate with panel models the impact of institutions on growth (Acemoglu et al., 2008, 2018; Assiotis and Sylwester, 2015; Bose et al., 2012). We consider the following model:

$$y_{i,t} = \zeta_j y_{i,t-1} + \mu JD_{i,t} + \mathbf{X}_{i,t}'\xi + \tau_i + \nu_t + \varepsilon_{i,t} \tag{5}$$

Equation 5 considers as deponent variable, y, the gdp per capita. Since we adopt a two-step estimator we use gdp as dependent variables as differencing Equation 5 produces growth rates. The specification includes 1 lag of GDP per capita on the right-hand side to control for the dynamics of GDP. Since both GDP and JD are assumed to be endogenous, instruments come from lags 2-5 of the endogenous variables.

COMMENT GMM model

Our aim is to examine the contribution of judicial delays to economic growth consistently and determine whether judicial delays cause growth or vice versa. Our identification strategy is to exploit the variation in past delay and GDP per capita dynamics to estimate the impact of judicial delays on growth. To this end, we estimate the following panel vector-autoregressive (VAR) linear equations:

$$y_{i,t} = \sum_{t=1}^{p} b_p \cdot y_{i,t-p} + \pi \cdot y_{i,t-p+1} + \mathbf{X'}_{i,t}\Lambda + u_{i,t} + e_{i,t} \tag{6}$$

Where y is a $1 \times k$ vector of dependent variables which consist of the log-transformed per capita GDP and judicial delay variable, $\mathbf{X}$ captures the set of structural covariates of per capita GDP and judicial delays, $u_{i,t}$ and $e_{i,t}$ are $1 \times k$ vectors of dependent variable-specific fixed-effects and idiosyncratic errors, u represent the set of country fixed-effects unobserved by the econometrician. $e_{i,t}$ are $1 \times k$ vectors of dependent variable-specific fixed-effects and idiosyncratic errors, capturing transitory shocks to per capita GDP and judicial delays. The $k \times k$ matrix $\mathbf{b}$ and the $l \times k$ matrix Λ are parameters to be estimated. We assume that transitory shocks/innovations have the following characteristics: $E[e_{i,t}] = 0, [e'_{i,t}, e_{i,t}] = \sum$ and $E[e'_{i,t}, e_{i,t}] = 0$, and for all $t > p$. Eq. 6 represents the k-covariate panel vector VAR model specification of order p with panel-specific country-fixed effects represented by the system of linear equations for $i \in \{1, 2, \ldots, N\}$ countries and $t \in \{1, 2, \ldots, T_i\}$ years.

The structural parameters in Eq. 6 can be estimated jointly with the fixed effects estimator or independently of the fixed-effects transformation using equation-by-equation OLS estimator. We prefer to estimate the structural relationship between per capita GDP and judicial delays using fixed-effects transformation to ensure that the effects are not confounded by the unobserved heterogeneity. The traditional approach to estimate structural VAR specification is to use the set of lagged dependent variables. However, lagged estimates would be biased (Nickel 1981) even in the presence of large N. As T becomes large, the bias approach zero but Judson and Owen (1999) find significant bias even when, for example, $T = 30$. To overcome these issues in our panel with a large N and relatively small T, we use GMM estimation to obtain consistent estimates of Eq. 6 with a fixed T and large N as previously suggested by Kiviet (1995), Bun and Carree (2005), Everaert and Pozzi (2007), and Canova and Ciccarelli (2013) among others.

Our key assumption is that the transitory shocks to per capita GDP and judicial delays and serially uncorrelated. This effectively ensures that first-difference transformation of Eq. 6 may be consistently estimated by instrumenting the lagged differences with differences and levels of $y_{i,t}$ from earlier periods as suggested by Anderson and Hsiao (1982). If some $y_{i,t-1}$ is not available, then the FD transformation at time t and $t-1$ is likewise missing. Given fixed T, we set the number of lags for the set of dependent variables to $p = 2$, as this is the necessary time

period where each panel is observed to yield reasonably unbiased estimates. As the lag order of the panel VAR gets larger, more realizations are required to estimate the structural parameters consistently. For instance, with the second-order lag, instruments in levels require that at least $T_i \geq 5$ realizations are observed for each panel.

As a remedy, we follow Arellano and Bover (1995) who propose forward orthogonal deviation as an alternative transformation which does not have the weaknesses inherent in FD transformation. Instead of using deviations from past realizations, we subtract the average of all available future observations to minimize the data loss. Hence, since past realizations are not included in this transformation, the instruments remain valid and with $T_i \geq 4$, realizations are necessary to produce instruments in levels.

Including a longer set of lags of instrumental variables (IV) would clearly improve the efficiency of our estimates but observations may be sharply reduced, especially with unbalanced panels or with missing observations. Following Holtz-Eakin, Newey and Rosen (1988), we build IVs using observed realizations, and substitute missing observations with zero assuming that the IVs are directly uncorrelated with the transitory shocks. This permits equation-by-equation consistent GMM estimates of the structural parameters with a clear efficiency gain.

[Table 7 about here.]

Table 7 presents the estimated VAR system of equations on the relationship between per capita GDP, judicial delay and level of democracy. The estimated parameters from VAR system of equations are particularly telling and provide the necessary insights to determine whether per capita income differences across countries are driven by judicial delays or varying levels of political development. Compared to more traditional panel-level identification strategies, panel VAR modeling approach allows us to empirically investigate the hypothetical reverse causation between per capita income, judicial delays and political development. In particular, this approach allows us to unravel whether prolonged judicial delays reduce long-term economic growth and, conversely, whether richer countries are better able to afford lower duration of delays. Since the estimated relationship between judicial

delays and economic growth may be tainted by the confounding influence of potentially omitted variables such as the level of political development, we also add a third variable (i.e. Polity2) to the VAR system of equations that captures varying degrees of political institutions through the distinction between dictatorship and democracy. Given the sample size limitations and the associated statistical power, our empirical strategy is to include two lags for each variable in the model.

The estimated parameter indicates a strong intrinsic relationship between judicial delays. Panel A reports the specifications using the log GDP per capita as a dependent variable. Columns (1) reports our preferred full-sample specification. The estimated parameters on the lagged judicial delays are both large and statistically significant. Notice that the first lag of the judicial delay is statistically significant at 1% and quantitatively large. The point estimate on the first lag implies that doubling the judicial delays is associated with 2.5 percent drop in the per capita GDP, ceteris paribus. The estimated coefficients on the first and second lag of the democracy variable are both small and statistically insignificant at conventional levels which implies that the strength of the negative relationship between judicial delays and per capita income is unlikely to be undermined by the level of political development. Column (2) restricts the sample to the civil-law countries only and indicates no changes compared to the original specification. In slight contrast, column (3) considers common-law countries only. Point estimates suggest that common-law jurisdictions themselves appear to be somewhat less sensitive to the variance in the judicial delays. Pointwise, prolonging judicial delays by 1 percent is associated with a drop in per capita income 0.33 percent which is about 8 times lower magnitude in comparison with the civil-law jurisdictions. In column (4), we discard OECD countries from the sample and find similar evidence to our original specification. Panel B reports the specifications using the length of the judicial delays as a dependent variable. Compared to Panel A, we find evidence of strong persistence of delays over time indicated by the large magnitude of the coefficient on the first lag.

By contrast, the coefficients on the first and second lag of the GDP per capita and political institutions variable are both small and statistically insignificant. The size and sign of the coefficients does not seem to vary much across the specifications regardless of whether civil-law or common-law jurisdictions are considered.

Evidence of significant interrelationship between per capita income and judicial delays is continually imperceptible even when OECD countries are discarded from the full sample. Hence, the estimated VAR specifications invariably suggests that the effect of judicial delays on per capita income is both negative and reasonably strong whilst the relationship in reverse appears to be neither quantitatively large nor statistically significant at conventional levels.

[Table 8 about here.]

Table 8 investigates the causality between judicial delays and per capita income in greater detail and reports the results of the Granger causality test on each excluded variable. The general thrust behind the causality test is simple. If the causes precede the consequences, judicial delays should presumably Granger-cause per capita income differences across countries but not vice versa. If richer countries are better able to reduce the judicial delays, per capita income should Granger-cause judicial delays and not vice versa. To disentangle the web of causality, we perform a series of Granger tests for each specification as originally reported in Table 7. For each specification, we test the null hypothesis that the excluded variable of interest does not Granger-cause the equation-level variable. Through such parametric restrictions, our approach is able to highlight the specific chain of causation and determine whether it is unidirectional or mutually reinforcing one. The results from the causality tests support our theoretical expectations. Across the full spectrum of specifications considered, we find ample evidence in support of the notion that judicial delays Granger-cause per capita income differences but not vice versa. The p-value on the null hypothesis of judicial delays not Granger-causing the per capita income is 0.001 in the full-sample specification and is in the range between 0.004 and 0.03 in the specifications using restricted samples. By contrast, the level of political development does not seem to Granger-cause per capita income beyond the impact of judicial delays. Panel B reports the equivalent set of Granger causality test statistics with the corresponding p-values from the specifications using judicial delays as a dependent variable. Contrary to Panel A, per capita income does not seem to Granger-cause the judicial delays. The p-value on the Wald test statistics is about 0.31 in the original specification and in the range between 0.29 and 0.78 across sub-samples which, at least empirically,

rules out the notion that higher levels of per capita income tend to reduce judicial delays. In the similar vein, the level of political development does not seem to Granger-cause judicial delays, even at artificially high significance thresholds although some influence is perceptible in civil-law countries where greater levels of democracy tend to somewhat Granger-cause judicial delays whilst elsewhere the causal chain is particularly weak.

One of the remaining questions behind the estimated interrelationships between per capita income and judicial delays concerns the ability to build credible inference on the magnitude of the impacts and direction of causation therein. Inference after panel-level VAR typically requires that variables used in the analysis are covariance stationary if their first two moments exist and are independent of time. To properly interpret panel VAR model, Hamilton (1994) and Lütkepohl (2005) show that a strict stability condition should be met. Namely, if panel VAR is stable, it is inveritable with an infinite-order vector moving-average representation. This implies that any impulse-response function or forecast-error variance decomposition should have known interpretation. Hence, if the modulus of each eigenvalue of the companion matrix $\mathbf{A}$ is strictly less than one on both real and imaginary scales, the estimated panel VAR model may be stable. If the model does not satisfy the stability conditions with modulus scores outside the unit circle, impulse response functions and forecasting may not be plausible. Figure 1 provides the missing stability diagnostics with the modulus scores in the roots of the companion matrix across real and imaginary scales for all specifications considered so far. The evidence broadly suggests that all eigenvalues lie inside the unit circle and that our panel VAR model chiefly satisfies the stability conditions both in the full-sample specification and across a variety of sub-samples.

[Figure 1 about here.]

Figure 2 presents simple impulse-response functions for the estimated relationship between judicial delays and per capita income. For a finite time horizon, such functions may provide two missing insights. First, IRFs may highlight the nature of the judicial delays as a policy shock to distinguish between its possible permanent or temporary nature. If the negative effect of judicial delays persists over time, permanency can be discussed. If the effect fades away of over time, it is likely

that longer judicial delays provide a temporary source of variation in economic growth. And second, IRFs have the ability to unravel the response of per capita income to the innovation in judicial delays. Since such innovations emanate from vector moving-average representation, it should be noted that positive innovations correspond to the increasing judicial delays whilst negative innovations correspond to reduced delays, respectively. We build two sets of IRFs. The first set corresponds to IRFs with small-sample Monte Carlo simulation whilst the second set expands on large-sample simulation. Given sample size restrictions, we set the forecast horizon to 5 years. The IRFs uncover the evidence of significant impact of judicial delays on economic growth that appears to be particularly strong in the short run but gradually become temporary. Decomposing the forecast error variance reveals a steep but moderate drop in per capita in the first year after one-standard deviation increase in judicial delays one year ahead. The negative effect tends to somewhat accelerate until the third year whilst stabilizing thereafter. After the third year, confidence bounds around the estimated response to the impulse in judicial delays appear to become somewhat larger rendering the response of per capita GDP to the impulse in delays marginally significant in the fifth year after the shock. This pattern holds across both small-sample and large-sample simulations. The implications of the IRFs is both simple and straightforward. More specifically, the negative effect of the 1 standard deviation increase in judicial delays lasts for the period of four years after the shocks and does not seem to disappear whilst becoming marginally significant after five years. This indicates that prolonged judicial delays may act as a permanent brake on the process of economic growth that only seldom disappears in the medium term.

[Figure 2 about here.]

4 Concluding Remarks

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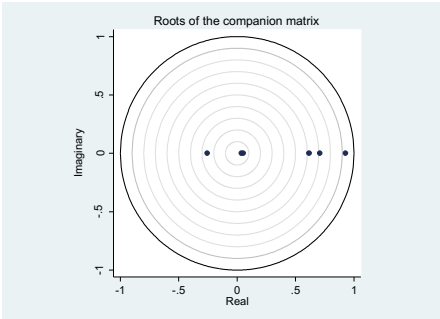
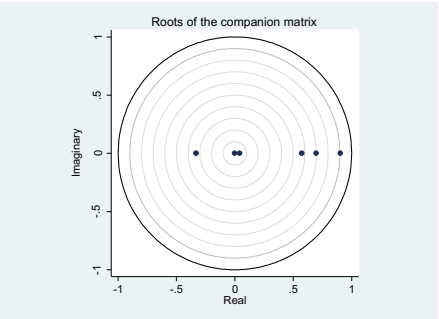
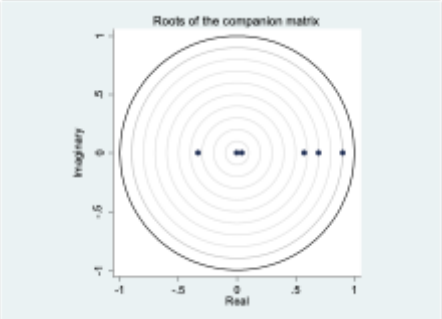
Figure 1: Stability diagnostics and eigenvalue conditions



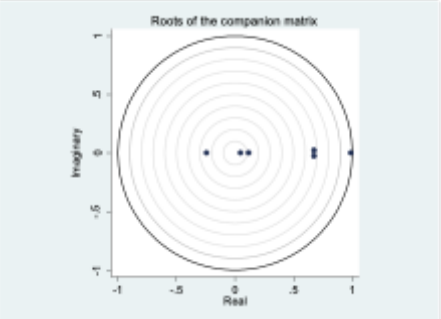
(a) Full sample



(b) Civil law countries only

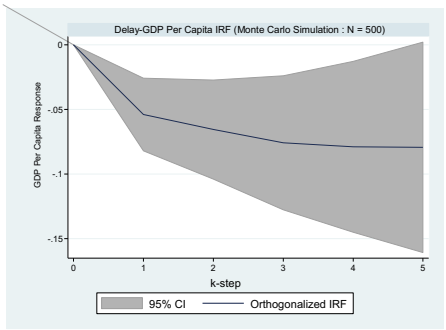


(c) Common law countries only

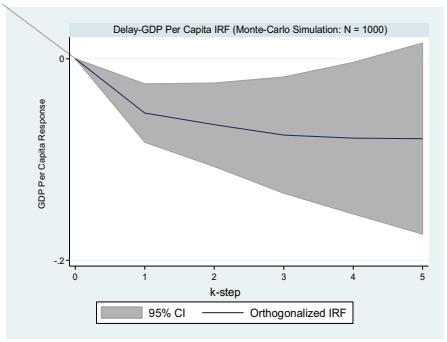


(d) Without OECD

Figure 2: Delay-GDP Per Capita Impulse Response Function



(a) Small-sample simulation



(b) Large-sample simulation

Table 1: Variables Description

Variable	**Description**	**Source**
g	gdp per capita yearly growth rate	IMF WEO
JD	judicial delay (in years)	WB DB
$invest$	Total investment as share of gdp	IMF WEO
$govrev$	General government revenue as share of gdp	IMF WEO
pop	yearly variation in population	IMF WEO
gdp_{t-1}	gdp per capita in $t-1$	IMF WEO
RoL	Rule of Law Index	WB GI
$costs$	costs of using the judiciary as share of claim size	WB DB
$proc$	number of judicial procedures necessary to conclude a case	WB DB
$reform$	Dummy = 1 if an "endogenous reform" took place in year t, 0 otherwise	WB DB
$crisis$	Dummy = 1 if year is ≥ 2008	

IMF WEO stands for International Monetary Fund's World Economic Outlook Database, WB DB stands for World Bank "Doing Business" Project, IMF GI stands for for International Monetary Fund's Governance Indicators

Table 2: Summary statistics

Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**	**N**
g	3.979	3.771	-9.782	19.586	2200
JD	1.729	0.857	0.329	4.932	2200
$invest$	24.397	8.939	2.681	81.94	2051
$govrev$	30.625	13.068	2.905	160.191	2199
Δpop	0.015	0.02	-0.186	0.349	2187
gdp_{t-1}	12137.52	17985.918	108.979	113611.891	2187
$costs$	34.876	25.904	0.1	163.2	2200
$proc$	38.208	6.434	21	55	2200
RoL	-0.052	0.969	-1.956	2.121	2198
$reform$	0.106	0.325	0	1	2200
$crisis$	0.583	0.493	0	1	2244

Table 3: Baseline Regression Results

	(1) full sample	(2) full sample
JD	-1.341***	-1.339***
	(0.514)	(0.471)
Controls	✓	✓
Year FE	✗	✓
Country FE	✓	✓
R-squared	0.139	0.286
# of Countries	175	175
Observations	2,048	2,048

Dependent Variable: yearly gdp growth rate. Clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4: Largest yearly variations in JD

	Country	**Year**	**ΔJD**	**Growth**	**Reform**
OECD	Italy	2007	-.493	1.474	Reform of procedural law
	Poland	2008	-.411	3.92	Reform of procedural law
	Poland	2013	-.397	1.668	Reform of procedural law and more judges hired
	Sweden	2010	-.532	5.989	Reform of procedural law and better case management
NO OECD	Bosnia	2005	-.822	3.868	No reform
	Botswana	2010	-.822	8.593	Reform of case management
	Cote d'Ivoire	2014	-.507	7.479	Creation of specialized commercial courts
	The Gambia	2007	-1.323	3.631	No reform
	Lesotho	2013	-.712	3.489	Creation of specialized commercial courts
	Mozambique	2014	-.521	7.373	No reform
	Nigeria	2014	-.603	6.31	No reform
	Serbia	2006	-1.077	4.904	No reform
	Timor-Leste	2010	-.999	9.368	No reform

Table 5: Regression Results Robustness Checks 1

	(1) only common law	(2) no common law	(3) full sample	(4) no OECD	(5) full sample	(6) first differences	(7) full sample
JD	-1.504	-1.206**	-2.490*	-1.939***	-1.339***	-2.023*	-1.176***
	(0.961)	(0.544)	(1.349)	(0.577)	(0.471)	(1.102)	(0.328)
JD^2			0.278				
			(0.305)				
crisis					-4.369***		
					(0.313)		
Controls	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓		✓
Country FE	✓	✓	✓	✓	✓		✓
R-squared	0.340	0.286	0.286	0.252	0.286	0.115	0.133
# of Countries	32	143	175	141	175	175	175
Observations	379	1,669	2,048	1,641	2,048	1,876	2,048

Dependent Variable (1)-(6): yearly gdp growth rate. Dependent Variable (7): five years gdp growth rate. Clustered standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table 6: Regression Results Robustness Checks 2

	(1) full sample	(2) full sample	(3) IV 2SLS	(4) System GMM
JD	-1.419***		-2.770*	-3,743**
	(0.479)		(1.506)	(1,474)
$reform$	-0.210			
	(0.265)			
JD_{t-1}		-1.387***		
		(0.522)		
Controls	✓	✓	✓	✓
Year FE	✓	✓		✓
Country FE	✓	✓		✓
R-squared	0.286	0.285	0.133	
# of Countries	175	175	175	175
Observations	1,880	2,050	175	1,878

Dependent Variable (10) & (11): yearly gdp growth rate. Instrument (9): # procedural steps. Dependent Variable (12): gdp per capita. Clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 7: Panel VAR Estimated Relationship between Judicial Delays and Economic Growth

	(1) full sample	(2) Civil Law	(3) Common Law	(4) no OECD
Panel A: Dependent Variable: GDP Per Capita				
ln gdp_{t-1}	.469***	.235	.769***	.520***
	(.139)	(.271)	(.118)	(.144)
ln gdp_{t-2}	.135	.237	.019	.134
	(.091)	(.158)	(.094)	(.099)
ln JD_{t-1}	-2.512***	-3.805***	-2.81	-2.354***
	(.659)	(1.412)	(.342)	(.714)
ln JD_{t-2}	0.77	.126	-.339**	.060
	(.121)	(.175)	(.155)	(.134)
Panel B: Dependent Variable: Judicial Delay				
ln JD_{t-1}	1.110***	1.146***	1.137***	1.069***
	(.155)	(.246)	(.178)	(.155)
ln JD_{t-2}	-.058**	-.052*	-.148	-.071**
	(.029)	(.030)	(.101)	(.035)
ln gdp_{t-1}	.023	.028	.032	.003
	(.034)	(.051)	(.046)	(.032)
ln gdp_{t-2}	-.004	-.006	-.016	.006
	(.020)	(.028)	(.034)	(.018)
GMM Q(b) Criterion	0.000	0.000	0.000	0.000
Observations	1,334	1,146	188	1,046

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 8: Granger-Wald Causality Test

	(1) full sample	(2) Civil Law	(3) Common Law	(4) no OECD
Panel A: Dependent Variable: GDP Per Capita				
log(Delay)	[0.001]	[0.027]	[0.021]	[0.004]
Polity	[0.634]	[0.396]	[0.693]	[0.794]
Panel B: Dependent Variable: Judicial Delay				
ln(y)	[0.310]	[0.289]	[0.780]	[0.427]
Polity	[0.577]	[0.095]	[0.512]	[0.642]

Note: the table reports p-values of Granger-Wald causality test. The null hypothesis is that the excluded variable does not Granger-cause the equation-level variable.