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Why Does *De Facto* Diverge from *De Jure?*

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Abstract:

An independent judiciary is often hailed as one of the most important aspects of the rule of law. Securing judicial independence (JI) via explicit constitutional rules seems straightforward and there is evidence that *de jure* and *de facto* JI are linked, at least in the long term. However, the realized degree of judicial independence often diverges significantly from the constitutionally guaranteed one. Based on a worldwide panel dataset from 1950 to 2018, we find that a negative gap, that is, when *de jure* JI > *de facto* JI, is very common. Factors associated with a decreasing gap are the number of veto players and the extent of press freedom and democracy, whereas corruption is associated with an increasing gap between *de jure* JI and *de facto* JI.

Keywords: Judicial independence; constitutional compliance; *de jure*; *de facto*; *de jure-de facto* gap.

JEL Codes: H11, K38, P51.

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**Judicial Independence: Why Does *De Facto* Diverge from *De Jure?*

1. Introduction

An independent judiciary is one of the most basic traits of the rule of law. Under the rule of law, all persons are to be treated equally, including those who govern. If government representatives are suspected of not complying with this basic principle, a neutral umpire is needed to evaluate government behavior: the judiciary. Judicial independence (JI)—and the rule of law more generally—have many beneficial consequences. Not only do they enable individuals to lead autonomous lives, they also help countries reach higher levels of economic growth and thus make everyone better off in economic terms.

Governments profit from faster economic growth in various ways: it makes them more popular, but it also increases their leeway as higher incomes imply higher tax revenues. This is why, generally, governments have an interest in an independent judiciary. Yet, a truly independent judiciary is not an unmixed blessing for government. Among the many examples of this are when the judiciary declares a newly passed law incompatible with the constitution or decides in favor of government critics or decides against members of the government in all types of cases, ranging from freedom of expression to criminal behavior. This is why the formally guaranteed level of judicial independence—its *de jure* level—often does not match its actually realized level—its *de facto* level. In this paper, we refer to the difference between *de jure* and *de facto* JI as a "gap." In principle, this gap can be either negative or positive: a negative gap is when *de jure* JI is greater than *de facto* JI; a positive gap occurs when *de facto* JI > *de jure* JI. A negative gap suggests that the constitution promises more than the government delivers, whereas the reverse is true of a positive gap. A negative gap is expected to have more serious consequences, which is why we confine our analysis to this type of gap.

Previous research is concerned with either the determinants of *de jure* JI or those of *de facto* JI. In this study, we add to the literature by asking what changes in the political system, and which environmental influences, induce changes in a negative *de jure/de facto* gap. We also discuss how this gap evolves over time and across countries, another contribution to the literature, because by combining *de jure* with *de facto*, we learn something about the circumstances under which deviations between the two occur.

In our dynamic-panel-data analysis, we find that an increase in the number of veto players, greater press freedom, and a higher level of democracy all bring *de facto* JI closer to *de jure* JI and thus reduce a negative gap. Corruption, on the other hand,

increases the negative gap between *de jure* and *de facto* JI. The magnitude of the estimated effects is noteworthy as it ranges from 1.5 to 3 standard deviations changes in the gap after a 1 standard deviation change in the explanatory variables. The relatively strongest (weakest) impact is estimated for changes in corruption (press freedom).

This paper does more than simply contribute to the discussion on JI and its determinants. The absence of a *de jure/de facto* gap can be viewed as an instance of constitutional compliance, leading to an interesting question: What are the factors that lead government to comply with formal constitutional constraints? This question is obviously relevant to much more than judiciary independence as compliance issues loom large in regard to basic human rights, democratic participation, and so on.

The rest of the paper is organized as follows: Section 2 briefly summarizes preceding studies related to our question. Section 3 develops a number of theoretical conjectures regarding factors that may cause a government to comply—or not— with formal constitutional constraints. In Section 4, we describe the data used to answer the research question. Section 5 contains our empirical results and Section 6 concludes.

2. Preceding Studies

One of the first studies that attempts to quantify both *de jure* and *de facto* JI is Feld and Voigt (2003). The authors find that while *de jure* JI does not cause faster economic growth, *de facto* JI is robustly correlated with economic growth. A follow-up study by Voigt et al. (2015) confirms these results. Since countries that improved their *de facto* JI over time did particularly well in terms of economic growth, the authors suggest that the relationship between *de facto* JI and economic growth is causal. These results greatly increased scholars' interest in the determinants of JI.

Based on the data by Feld and Voigt (2003), Hayo and Voigt (2007) inquire into the determinants of *de facto* judicial independence (JI) and find that while *de jure* and *de facto* JI are not very highly correlated, *de jure* JI is still the single best predictor for *de facto* JI.[1] At the time that paper was written, the only indicators available for both *de jure* and *de facto* JI, for a fairly large number of countries,

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[1] However, not all variables included in the *de jure* indicator are codified at the constitutional level.

were cross-sectional. Since then, however, panel data for both *de jure* and *de facto* JI have become available.

The determinants of constitutionally entrenched *de jure* JI are studied by Hayo and Voigt (2014). Focusing on time-invariant variables, they find that former British colonies are less likely to address JI explicitly in their constitutions but also that constitutional change with regard to JI is a rare event. Hayo and Voigt (2016) are interested in the type of changes to a political system that cause changes in the level of constitutionalized *de jure* JI. They find that an increase in the number of groups included in collective decision-making is the most important driver of higher levels of *de jure* JI. Individual leader characteristics also play a role: the longer a leader has been in office, the lower the chances of any change in *de jure* JI. But a leader losing office under irregular circumstances is likely to be followed by reductions in *de jure* JI.

In a paper dealing with the relationship between *de jure* and *de facto* JI, Melton and Ginsburg (2014) question whether *de jure* JI really matters for the level of *de facto* JI. Starting from the premise that, at the end of the day, constitutional constraints must be self-enforcing, the authors argue that constitutional provisions involving multiple players are most likely to be enforced because each player has an incentive to guard its own competences. They observe that procedures for both the selection and removal of judges are often divided between the executive and the legislature, thus making representatives of each branch guardians of the other. Other aspects mentioned as conducive to JI are less relevant as long as their enforcement is not secured via checks and balances.

The empirical findings reported in Melton and Ginsburg (2014) support the idea that none of the conventional variables for proxying *de jure* JI are significantly correlated with *de facto* JI. However, when the strength of checks on the executive is interacted with selection and removal procedures, the authors find a significant correlation with *de facto* JI. The institutional environment also appears to play a role in that, *ceteris paribus*, the correlations between selection and removal procedures and the actually realized degree of JI are higher in autocracies than in democracies. These results provoke a number of follow-up questions: (1) What aspects of *de jure* JI are good predictors of *de facto* JI? Is it really true that only two variables are relevant here? (2) What additional conditions are necessary for *de jure* aspects to have any significant effect on *de facto*?

Drawing on data from the EU Justice Scoreboard, Gutmann and Voigt (2020) identify a puzzle: *de facto* JI on the national level (as perceived by the citizens of EU member states) is negatively associated with formal legislation usually

considered conducive to judicial independence, that is, *de jure* JI.[2] The negative association is more pronounced in the "old" member states than in the "young" ones in Central and Eastern Europe, implying that the relationship is not driven by countries that were striving to become members of the European Union and simply passed independence-enhancing legislation without changing anything on the ground. The negative association also holds across legal families. Since none of the more standard ways to solve the puzzle work, the authors ask whether cultural traits could be the key. It turns out that countries with high levels of generalized trust exhibit high levels of *de facto* JI and, at the same time, low levels of *de jure* JI. It seems that explicit legislation (in this case dealing with JI) serves as a substitute for high levels of trust when they are absent. The authors conclude that cultural traits are fundamentally important to the quality of formal institutions, even in societies as highly developed as the EU member states. Thus, when informal institutions are not conducive to a high level of *de facto* JI, it seems obvious to try achieving that goal by implementing formal ones.[3]

Given the above findings of Gutmann and Voigt (2020), the next natural question is whether the relationship between *de jure* JI, *de facto* JI, and trust holds beyond Europe. This is exactly what Gutmann and Voigt (2019) did. Based on entirely different datasets, they not only replicated their previous findings with regard to Europe, but also found a very similar relationship for the Americas. In Africa, however, a new puzzle emerged: they found a highly significant positive correlation between *de jure* and *de facto* JI. In other words, in Africa, but not in Europe or the Americas, *de jure* JI is a good predictor of *de facto* JI. The counterintuitive results do not stop there. At the world level, *de jure* and *de facto* are almost perfectly uncorrelated. Yet, as soon as one distinguishes between democracies and non-democracies, a negative correlation is found for democracies and a positive one for non-democracies. Understanding these counterintuitive results requires a deeper look at environmental factors. One such factor could be colonial history. It appears that a history of colonization leads to an inversion of the coefficients: countries that have never been colonized have a negative correlation between *de jure* and *de facto* JI and a positive one between trust and *de facto* JI. In other words, the results found for Europe remain valid beyond Europe, but only for countries that were never

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[2] Mocan et al. (2020) identify another effect of the subjective perception of judicial independence. Survey respondents in countries with a low level of (perceived) judicial independence are more likely to endorse criminal behavior, such as accepting bribes or selling counterfeit goods.

[3] Aldashev et al. (2012) ask under what conditions the law can be used to shift informal institutions (called "customs" in their paper) towards the intended goal.

colonized. Countries with a colonial history tend to have a positive correlation between *de jure* JI and *de facto* JI, but a negative one between trust and *de facto* JI.

Hayo and Voigt (2019) are specifically interested in the long-term dynamic relationship between *de jure* and *de facto* JI. Separating OECD from non-OECD countries, they find a positive long-run equilibrium only for the latter group. Thus, their findings are largely in line with those reported above. Following up on a conjecture raised by Melton and Ginsburg (2014), Hayo and Voigt (2019) ask whether causality could also run from *de facto* to *de jure*, or, in other words, whether there is any evidence for actual independence levels being written into the law *ex post*. No evidence in favor of such reversed causality was found.

Research on determinants of constitutional compliance beyond the judiciary is still in its infancy. Voigt (2021) is more a description of a research program than an overview of existing studies. Gutmann et al. (2022) contains a broad dataset that is used, for example, by Choutagunta et al. (2022), who study whether major events—such as natural disasters—result in less government compliance with constitutional constraints. Bjørnskov and Voigt (2021) ask whether constitutionally protected media freedom is curtailed after terrorist events and find the answer to be "yes." Bjørnskov et al. (2022) analyze reasons that lead governments to overstep constitutional constraints during a state of emergency.

3. Possible Determinants of the Gap: Theory

3.1. Introductory Remarks

There is a simple answer to the question of why constitutional reality diverges from constitutional text: an independent judiciary can pose a formidable obstacle to government, limiting its policy options considerably. Indeed, a government might make itself better off by curtailing formally guaranteed judicial independence.

Because constitutional provisions form the most basic layer of rules, there are, basically, no rules undergirding them that can be employed to sanction noncompliance with these provisions. This implies that compliance with constitutional rules should be high when the relevant actors cannot make themselves better off by *not* enforcing the rules, a situation often referred to as "self-enforceability." In this section, we discuss a number of aspects likely to affect government behavior vis-à-vis the judiciary and thereby determine the *de facto* level of judicial independence.

We assume that JI is present when judges expect their decisions to be complied with, regardless of whether such decisions are in the interest of other government branches upon which implementation depends. Furthermore, judges need not fear being sanctioned for their judicial decisions, such as being remunerated less, transferred to another court, or even being expelled from the judiciary. *De jure* JI is comprised of the legal provisions intended to bring about the state of affairs just described. In this paper, we analyze only provisions included in a country's constitution; we analyze neither statutory law nor court decisions. *De facto* JI, in turn, describes the degree to which, in a particular country in a given year, judges' decisions are expected to be complied with, along with their true immunity from sanction for making such decisions.

We discuss four such aspects: (1) *de jure* provisions guaranteeing judicial independence, (2) aspects of constitutional design beyond the judiciary proper, such as the separation of powers, (3) "environmental factors," such as the dominant culture of a country and its constitutional history, and (4) traits of the members of the executive.

3.2. *De Jure* Provisions

Constitutions are often described as containing the highest law of the land. By including specific aspects in a country's constitution, its framers make those provisions more salient than if they were contained only in statutory law. Reneging on constitutional constraints may be costly if it triggers resistance from opposition parties, the media, and/or the people at large. Thus, explicit mention of JI in the constitution—as opposed to in statutory law or not at all—should lead to higher government compliance with these provisions, that is, higher *de facto* JI.

It is often assumed that formal entrenchment of JI has become more common over the last couple of decades. Our *de jure* JI indicator confirms that this is, indeed, the case (details in Section 4 below). Based on the ideas developed above, this could imply that older constitutions are more likely to be complied with than newer ones, given that JI provisions have remained unchanged.

The frequency with which constitutions are formally amended is also a function of the difficulty (i.e., the costliness) of doing so. Gavison (2002) and Elkins et al. (2009) argue that when it is very costly to amend constitutions, compliance with constitutional amendment rules becomes less likely. If, say, an absolute majority of legislators agree with a major part of the population that constitutional change is desirable, but very high supermajorities are needed to implement it, politicians may be able to simply ignore constitutional constraints. However, empirically testing

this hypothesis is fraught with difficulties. A cost of amendment variable is needed that can indicate how cumbersome—in terms of players consenting, necessary (super)majorities, and so forth—constitutional amendment truly is. Various attempts to produce such a variable have been made (Lutz 1994; Lorenz 2005; Rasch and Congleton 2006), but correlations between these indicators are, unfortunately, very low, possibly indicating conceptual disagreement between the authors.

3.3. Constitutional Architecture

Separation of powers is a key design element of constitutions. The concept not only refers to the usual separation between the three branches of government, but also to the responsibilities of various levels of government (federalism) and to those of independent agencies, such as a central bank. If more than one actor is needed to implement a policy decision, then each of these actors has incentives to make sure that other actors do not overstep their competences, as this frequently implies a diminution of one's own competences. We thus expect that *a higher number of constitutional veto players will increase the likelihood of constitutional compliance*.[4]

Democracy enables citizens to change the government in a peaceful and orderly manner, thus implying that governments in democratic countries have more incentive than autocratic governments to invest in their popularity. Arguably, this popularity is endangered when the government violates formal rules.

Moreover, as Inglehart and Welzel (2005) point out, self-expression values are the best predictors of effective democracy. "Effective democracy" is not identical with

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[4] Some veto players might be indifferent to noncompliance, or even support it some of the time. Their interaction situation can then be described by the game proposed in Weingast (1997) (here paraphrased): "I might insist on compliance with the constitution because not showing solidarity with those reneged on now might lead to non-solidarity of those at some other time when it is my rights that are being transgressed against." Here, again, the question is whether or not some norms of solidarity exist. When testing this hypothesis empirically, it is worth making an explicit distinction between institutional and actual veto players. When a legislature is bicameral and each house needs to consent to new legislation, there are two institutional veto players. If this occurs in a system with highly disciplined parties and the same party holds a majority in both houses, it might be advisable to count the legislature as only one actual veto player. Fortunately, this distinction receives explicit recognition in some indicators, such as Henisz's (2000), and hence is not a barrier to empirical testing. Another problem, potentially increasing in the number of veto players, is the volunteer's dilemma: if holding the executive accountable is costly, then all veto players might hope that another veto player is ready to bear these costs.

constitutional compliance, of course, but it can be argued that the two go hand in hand. It is hard to imagine a country with an effective democracy in which the government does not comply with the constitution. This leads us to hypothesize that *democracies are likely to experience lower* de jure/de facto *gaps than are non-democracies*.

3.4. Environmental Factors

In this paper, all potentially relevant factors that are independent from the constitution itself are termed "environmental factors." These factors include the country's geographic location, its constitutional and colonial history, and the prevalent societal values and norms.

A connection between constitutional design factors and environmental factors can be referred to as a "constitutional culture." Ferejohn et al. (2001, 10) define constitutional culture as "a web of interpretative norms, canons, and practices which most members of a particular community accept and employ (at least implicitly) to identify and maintain a two-level system of the appropriate sort." The authors argue that in order to understand how constitutional text (i.e., the *de jure* constitution) is implemented, one needs to look at how people actually think it should be operating (an environmental factor).

A consensus among citizens that government has overstepped its bounds (and then acting on this insight) seems more likely when the citizens share many values and norms. In other words, it is not only the precision of the constitutional text alone that is important—as Weingast (1997) would have it—but also the homogeneity of its interpretation. Vanberg (2011, 313) argues that when citizens share values, they are better able to coordinate with regard to legitimate and illegitimate government action. Here again, the interaction between constitutional design and environmental factors is crucial in the sense that the written constitution can contain precise constraints that will become effective if citizens have common expectations as to how these written constraints should be interpreted and acted upon.

Two preconditions for interpreting government behavior as being anti-constitutional are that the citizenry know what the constitution contains on the one hand (i.e., the *de jure* constitution), and that they also know that the government is actually doing on the other. That information allows evaluating whether government behavior is in compliance with the constitution. It might be difficult for individual citizens to gather the necessary information, but the media has certainly proved itself capable of both providing relevant information, as well as

offering an assessment of same. We thus hypothesize that *a high degree of media freedom is conducive to a small* de jure-de facto *gap*.[5]

Corruption is usually defined as the misuse of public office for private gain. Bribing as well as accepting bribes are both considered criminal behavior. For corruption to work, then, the bribe-giver as well as the receiver need to coordinate their behavior outside the realm of law. The mores and conventions on which corruption depends are part of the informal institutions of a society. Corruption not only implies that informal institutions are in conflict with formal law, but also that informal institutions are used as means to circumvent the law. Since bribes are used to outmaneuver formal law, high levels of corruption are indicative of low levels of the rule of law.

In a society suffering from a high level of corruption, there will be many instances when formal law is unlikely to be implemented. Inglehart and Welzel (2005, 154) even interpret corruption "as an indicator of 'elite integrity', or the extent to which power holders actually follow legal norms." We thus hypothesize that *societies characterized by a high level of corruption, will also be host to a relatively larger deviation between* de jure *JI and* de facto *JI*. Indeed, one can think of *de facto* JI as *de jure* JI conditional on the absence of corruption.

3.5. Traits of Government Members

The personalities of the governing could also matter for the *de jure-de facto* gap, as politicians with particular traits might be more rule-abiding. If the mechanism used to select and appoint political leaders rewards adherence to rules, the probability of noncompliance is reduced.

Different selection mechanisms can lead to the selection of politicians with different traits. The underlying assumption is that there will be various types of politicians and these different types will draw different benefits from not complying with the constitution. Brennan and Hamlin (2000) make an assumed dispositional heterogeneity among (political) actors concrete by additionally assuming that politicians can be driven by virtue or by self-interest. The authors then ask whether institutions can be designed such that virtuous individuals are more likely to run for and be elected to political office. They proceed in two steps. In the first (static) step, dispositions are assumed to be given. In the second (dynamic) step, institutions can affect the distribution of dispositions among actors (i.e., institutions can be virtue-

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[5] Of course, the degree of media freedom is endogenous to government behavior itself, which needs to be taken into account when empirically assessing the conjectures.

enhancing). This idea is related to the benefits of constitutional noncompliance in that some types of individuals are expected to realize higher benefits from reneging than are others.

Due to a lack of both theoretical and empirical insight, we simply propose a number of *ad hoc* conjectures regarding personal traits of leading politicians.

(a) Leaders who achieved power through irregular means may be more likely not to comply with the constitution. In other words, if a person acquired power by breaking rules, why should he or she comply with the rules while governing?[6]

(b) Leaders who once served in the military may be less likely to comply with the constitution. The underlying assumption is that a sizeable portion of military leaders are ready to place their own preferences regarding law and order above constitutional constraints that they view as slow and cumbersome. Indeed, many coups d'état are staged by military leaders, giving some *prima facie* plausibility to our hypothesis.

(c) Female leaders may be more likely to comply with the constitution. There is some evidence (Dollar et al. 2001) that countries ruled by women suffer less corruption, which could be interpreted as one proxy for rule compliance. There is also evidence that, in general, women are more risk averse, which could lead them to be more rule-compliant (Croson and Gneezy 2009).

(d) Younger leaders may be less likely to comply with the constitution because securing tenure has a higher value for them than it does for older leaders. The argument is that the present value of being in power is higher for younger leaders. However, one might expect the exact opposite: if reneging on the constitution increases the chances of being thrown out of office, younger leaders might be more careful not to renege.

The underlying assumption regarding all four conjectures is that the benefits of noncompliance are linked to an actor's personal characteristics.

4. Data, Methodology, and Descriptive Statistics

Testing the hypotheses developed above is a challenge, as it is difficult to find (i) good indicators that (ii) move over time, are available for not only (iii) a large number of countries, but also (iv) for a long time period. Many tradeoffs were necessary. Concerns about the panel-data structure of the analysis and the necessity

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[6] Hayo and Voigt (2016) find that reaching power through irregular means is significantly correlated with a change in the constitutionally guaranteed level of judicial independence.

for dynamic modeling made it important that the time dimension be large enough. Thus, we included only those countries for which we had 10 or more consistent observations over time. When there were more than 10 observations before and after the period with missing values, we used observations from the later period only, as the data quality tends to be better. Thus, we overweight more recent observations compared to earlier ones. We also put a great deal of weight on obtaining a large cross-section, which meant that we could not use indicators that were either unavailable for a decade in terms of the time dimension or for only a few countries in terms of geographic scope.

As a measure of *de jure* JI, we rely on a question from the Comparative Constitutions Project (Elkins et al. 2009), namely: "*Does the constitution contain an explicit declaration regarding the independence of the central judicial organ(s)?*" This question can be answered either in the affirmative or the negative. Affirmative answers are coded 1; negative answers are coded 0. Admittedly, this indicator is rather crude, as it does not really distinguish the degree to which JI is anchored in the constitution; however, it is easily measured and available for a large number of countries.

The *de facto* JI measure is based on five items from the Varieties of Democracy database (Coppedge et al. 2021). The questions deal with (1) judicial purges, (2) government attacks on the judiciary, (3) court packing, (4) high court independence, and (5) compliance with high court decisions. The general idea driving the choice of variables was that judicial purges, for example, would be detrimental to a factually independent judiciary. For all five items, the possible responses ranged between "never happened" and "very common." Here, we use just one example as an illustration; the four other questions and answers are documented in the Appendix. The question with regard to judicial purges was:

> *Judges are sometimes removed from their posts for cause, as when there is strong evidence of corruption; however, some judges are removed arbitrarily, typically for political reasons. With this distinction in mind, please describe the removal of judges that occurred this calendar year.*

These were the five possible responses:

(1) There was a massive, arbitrary purge of the judiciary;
(2) There were limited but very important arbitrary removals;
(3) There were limited arbitrary removals;

(4) Judges were removed from office, but there is no evidence that the removals were arbitrary; or

(5) Judges were not removed from their posts.

The *de facto* index can take on values between 0 and 1. Since we attach equal weight to all five items, any value between 0 and 0.2 could be assigned to each of the five *de facto* JI questions. With regard to the purge question, for example, when the respondent answered that no judges were removed in a given year, the item was coded 0.2, whereas when there had been a massive, arbitrary purge, the respective country year was coded 0. The gap itself is the difference between the *de jure* measure minus the *de facto* score.

In principle, both negative and positive gap measures can be constructed. Positive gaps would presume that JI is not constitutionalized—that is, its *de jure* value would be 0—but that *de facto*, judges do enjoy a certain degree of independence.[7] It is quite likely, however, that in countries that have not constitutionalized *de jure* JI, there is statutory law declaring the judiciary to be independent. This is why it would be misleading to refer to such countries as having a positive gap or as over-performing. Therefore, we constrain our analysis to countries that have promised JI in their constitutions. The gap measure is constructed as shown in Equation (1):

(1) JI Gap = 1 – (Judicial purges/5) – (Government attacks on judiciary/5) – (Court packing/5) – (High court independence/5) – (Compliance with high court/5)

A nonzero gap in a given year indicates that the country has constitutionalized *de jure* JI provisions, but does not deliver perfectly on the five items that make up the *de facto* indicator. A zero gap implies that the country has not only constitutionalized JI provisions, but has also enforced them perfectly (along the five items taken into consideration). This coding approach thus implies that the time variation in the gap is always due to the *de facto* variables.

Table A1 of the Appendix lists the countries and observation periods for *JI Gap*. In spite of the large cross-sectional sample, due to missing observations, our data are not representative of the world. Tables A2 and A3 contain detailed definitions of the variables and descriptive statistics, respectively.

Figure 1 illustrates the development of *JI Gap* based on its sample average in each year.

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[7] Over our sample period, there are more than 1,800 country year observations reflecting a positive JI gap.

Figure 1: *JI Gap* and number of sample countries across time
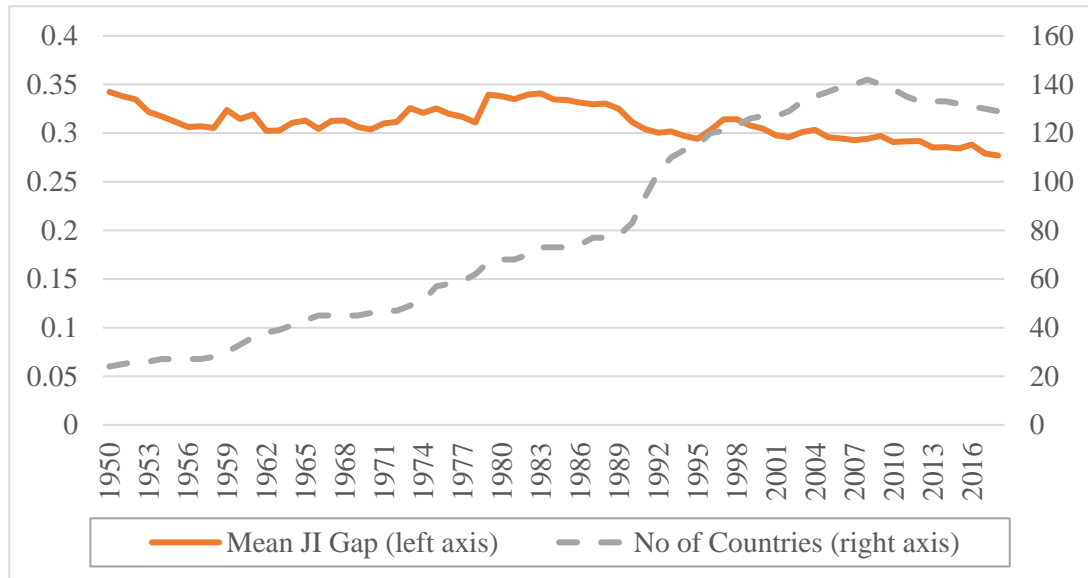
As shown by the right-hand-side axis of Figure 1, the sample commences with 24 countries in 1950, a number that rises to 142 in 2008 before falling back to 129 in 2018. In spite of these notable variations in the number of countries, the estimated average of *JI Gap* as given by the left-hand-side axis remains remarkably stable across time. It starts off at almost 34% in 1950 and then declines to about 28% in 2018. In other words, the gap between *de jure* and *de facto* JI has become only somewhat smaller over the last six decades.

Still, we find episodes of noteworthy variation, for instance, from 1978 to 1979, *JI Gap* shifts upwards by almost 3 percentage points. Generally, we observe a slight downward trend, especially from 1998 onwards. In light of the limited sample variation in the number of countries (minimum: 124 countries, maximum: 143 countries), this downward trend appears meaningful. However, over our sample period of almost 70 years, the gap has decreased by only about 6 percentage points, which is, on average, less than 0.1 percentage points per year.

In Table 1, we compare the gap across the main regions of the world. The averages accord well with our intuition: countries in the MENA region, in Africa, and East Asia score above the mean gap score, that is, these countries have a higher gap between *de jure* and *de facto* than the world average. The opposite holds for North

America/Western Europe and Oceania regions in which countries do significantly better than the world average when it comes to the JI gap.

Table 1: Observations on *JI Gap* across the world's regions

	JI Gap		
	No. of Obs.	Mean	St. Dev.
Africa	1,454	0.37	0.20
East Asia	514	0.36	0.19
Eastern Europe	942	0.31	0.17
Latin America & Caribbean	1,016	0.30	0.19
MENA	781	0.39	0.16
Oceania	143	0.12	0.08
South Asia	129	0.30	0.15
North America and Western Europe	640	0.07	0.06
Total sample	5,619	0.31	0.20

In our sample, the country with the largest gap is Peru in 1993. Alberto Fujimori became president in 1990 and staged a so-called *autogolpe* in 1992. He shut down Congress, suspended the constitution, and purged the judiciary (Levitsky 1999). Fujimori had a new constitution written (passed in 1993), which contained a formal recognition of JI. As a consequence of this coup, Peru was no longer considered a democracy (the Polity2 score switched from 8 in 1991 to –3 in 1992), media freedom was seriously curtailed, and corruption became rampant. Since Fujimori's secret police chief, Vladimiro Montesinos, not only made bribe recipients sign receipts, but also had the signing videotaped, the exact sums paid to politicians, judges, and the media are very well documented (McMillan and Zoido 2004). In 2000, during a trip to Asia, Fujimori resigned from being president. All in all, the very high negative gap values calculated for Peru appear highly plausible.

Other countries with very high negative gaps are Equatorial Guinea with 0.88 in 2018, Mauritania with 0.85 in 1991, Iran with 0.85 between 1980 and 1983, and Sudan with 0.83 in 1998. To us, these scores seem highly plausible. In 2018,

Equatorial Guinea was governed by the world's longest serving autocrat, Teodoro Mbasogo, who assumed power via a military coup in 1979.[8] In Mauritania, between September 1990 and March 1991, there were internal fights involving the armed forces. Between 1987 and 1991, thousands of people belonging to ethnic minorities were killed and some 80,000 were sent into exile (https://bti-project.org/en/reports/country-report/MRT#pos2). Ayatollah Khomeini came to power in Iran in 1979 and the years in which Iran scores the highest *JI Gap* are, perhaps unsurprisingly, the first years of the so-called Islamic Republic. While the civil war in Sudan had been going on for 15 years, 1998 marked a particularly bad year for that country as it was hit by a famine, which led to gross human rights violations (https://www.hrw.org/reports/1999/sudan/).

Using time-series methods and different indicators for *de jure* JI and *de facto* JI in a sample of 87 countries, Hayo and Voigt (2019) find evidence that *de jure* and *de facto* JI are co-integrated, thus creating a long-term equilibrium. Moreover, causality appears to run from *de jure* JI to *de facto* JI. Since the present sample is unsuitable for computing reliable stationarity tests, we simply assume that the co-integration result holds here, too, which allows us to use the difference between the two JI variables as our stationary dependent variable.

To avoid nonstationarity on the right-hand-side of the regression, we include the explanatory variables, except for dummies, in logarithms and first differences, that is, relative rates of change. Since there is notable persistence in *JI Gap*, we add a lagged dependent variable, which makes the estimators more (asymptotically) inefficient and avoids estimating a dynamically misspecified equation. We lag the explanatory variables by one year, which we believe is a reasonable period during which the *JI Gap* could change.[9]

We include an indicator for parliamentary political systems (Scartascini et al. 2018). To capture veto power in the political system, we rely on an indicator based on counting the number of veto players (Henisz 2017). We include a dummy variable measuring whether the constitution contains at least one provision for amending the constitution, based on Elkins et al. (2009). Press freedom is proxied by the Whitten-

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[8] A country report on Equatorial Guinea can be found at https://www.state.gov/reports/2018-country-reports-on-human-rights-practices/equatorial-guinea/.

[9] In a robustness test for any remaining deterministic trends, we include a yearly trend and decade dummies (1950s to 2000s). The additional variables are not significant and none of the results set out in Table 3 change in a noteworthy way.

Woodring et al. (2017) ordinal indicator. We include the Polity2 indicator from the Polity IV database to capture a country's degree of democracy (Marshall 2013). Our political corruption indicator is from the Variety of Democracies dataset (Coppedge et al. 2021) and can have a value between 0 and 1. We include a number of leader characteristics, as well as indicators measuring how these leaders obtained and yielded power (Goemans et al. 2009), in the form of dummy variables. Specifically, we code whether (i) leaders came into power or (ii) were removed through foreign intervention. More generally, we include additional dummies indicating whether leaders entered office via (iii) regular or (iv) irregular means. We measure whether they belong to (v) the military, also whether they are (vi) male. Finally, we control for the (vii) leader's age. In total, we have 13 explanatory variables plus five decade dummies on the right-hand side of our regression explaining *JI Gap*. To allow these variables to affect *JI Gap*, they are included with a one-year lag.

The estimated general model is given in Equation (2):

(2) JI Gap$_{it}$ = β_0 + β_1 *JI Gap*$_{it-1}$ + β_2 *ΔlnDemocracy*$_{it}$ + β_3 *LAmendment*$_{it-1}$ + β_4 *LParlPolSys*$_{it-1}$ + β_5 *ΔlnNoVetoPlayers*$_{it}$ + β_6 *ΔlnPressFree*$_{it}$ + β_7 *ΔlnCorruption*$_{it}$ + β_8 *LForeignMadeLeader*$_{it-1}$ + β_9 *LIrregLeader*$_{it-1}$ + β_{10} *LIrregExitLeader*$_{it-1}$ + β_{11} *LMilitaryLeader*$_{it-1}$ + β_{12} *LLeaderAge*$_{it-1}$ + β_{13} *LLeaderFemale*$_{it-1}$ + ε_{it}

The corresponding variable definitions are listed in Table 2; descriptive statistics can be found in Table A2.

Table 2: Variable definitions

Variable Name	Explanation
JI Gap	*De jure* JI – *de facto* JI (see Equation (1))
ΔlnDemocracy	Relative change of degree of democracy (source: Marshall 2013)
LAmendment	Lag of dummy = 1 when the constitution explicitly allows for an amendment (source: Elkins et al. 2009)
LParlPolSys	Lag of dummy = 1 when the country has a parliamentary political system (source: Scartascini et al. 2018)

ΔlnNoVetoPlayers	Relative change of number of veto players (source: Henisz 2017)
ΔlnPressFree	Relative change of degree of press freedom (source: Whitten-Woodring et al. 2017)
ΔlnCorruption	Relative change of degree of corruption (source: Coppedge et al. 2021)
LForeignMadeLeader	Lag of dummy = 1 for leader coming to power through foreign help (source: Goemans et al. 2009)
LIrregLeader	Lag of dummy = 1 for leader coming to power through irregular means (source: Goemans et al. 2009)
LIrregExitLeader	Lag of dummy = 1 for leader exiting power through irregular means (source: Goemans et al. 2009)
LMilitaryLeader	Lag of dummy = 1 for military leader (source: Goemans et al. 2009)
LLeaderAge	Lag of leader's age (source: Goemans et al. 2009)
LLeaderFemale	Lag of dummy = 1 for female leader (source: Goemans et al. 2009)

Given the dynamic specification, least-square dummy variable estimation suffers from the so-called Nickell bias (1981), which should actually be called "Nickell inconsistency," as, even under the best conditions, a lagged dependent variable estimator is only consistent but not unbiased. The problem arises when the estimator's consistency depends on the number of cross-sectional units rather than on the observations across time going to infinity. While it can be argued that in our sample, it is highly unlikely that consistency runs across countries, as surely the number of countries cannot become infinitely large, the cross-sectional dimension clearly dominates the sample.

Thus, to address consistency concerns, we estimate the relationship using a biased-corrected least-square dummy variable estimator. Kiviet (1995) suggests that, in small samples, a standard-error correction yields more reliable results than GMM and puts forward a consistent dynamic estimator. We employ Bruno's (2005) extension for unbalanced panels with Arellano Bond initialization, bootstrapped

standard errors, and a bias correction up to order $O(1/NT^2)$. Estimating a dynamic-panel-data model also implies that we can differentiate between short-term and long-term effects of changes in the explanatory variables.

To improve both estimation efficiency and interpretability of the results, we employ general-to-specific modeling (Hendry 1993). Including all theoretically relevant variables jointly takes into account both omitted variable bias and standard-error-decreasing complementarity (Hayo 2018). The general model contains 12 variables plus the lagged dependent variable. Striking a compromise between Type I and Type II errors, and in light of the substantial sample size (Leamer 1978), we test at a 5% nominal significance level. Finally, to test for the robustness of our estimates using out-of-sample observations, we re-estimate the reduced model using the expanded sample (an increase in sample size of 872 observations, or more than 60%), which becomes available due to eliminating the jointly insignificant variables from the model.

5. Empirical Analysis

Estimates of the general model are shown in the second column of Table 3. We test for the joint significance of the eight insignificant variables and cannot reject the restriction. Imposing this restriction, the third column contains the reduced model, which is based on the same sample as the general model. Re-estimating the reduced model using the larger sample available after dropping the insignificant variables results in the estimates presented in the fourth column of the table. A sign that this is a highly robust model is that the estimates are so consistent between the models, both in terms of significance as well as magnitude, especially in the third model, which is based on increasing the sample size by more than 60%.

In addition to the lagged dependent variable, we find four explanatory variables that are robustly significant across all three models.[10] A positive change in the number of veto players, degree of press freedom, and level of democracy all bring *de facto* JI closer to *de jure* JI and reduce the *JI Gap*. However, a positive change in the degree of corruption increases the *JI Gap*. These results are fully in line with the theoretical priors discussed above.

Table 3: Estimating the determinants of *JI Gap*

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[10] Note that the *p*-value for the reduced-model coefficient on $\Delta lnNoVetoPlayers_t$ in the small sample is 0.56, which is not quite significant at a 5% level.

	Models		
	General Model	Reduced Model (Small Sample)	Reduced Model (Large Sample)
Persistence			
JI Gap$_{t-1}$	0.865** (0.019)	0.874** (0.016)	0.863** (0.012)
Political factors			
ΔlnDemocracy$_t$	–0.023** (0.005)	–0.024** (0.005)	–0.031** (0.007)
LAmendment$_{t-1}$	–0.004 (0.003)		
LParlPolSys$_{t-1}$			
Environmental influences			
ΔlnNoVetoPlayers$_t$	–0.008** (0.004)	–0.008 (0.004)	–0.008** (0.003)
ΔlnPressFree$_t$	–0.032** (0.007)	–0.032** (0.007)	–0.025** (0.007)
ΔlnCorruption$_t$	0.054** (0.009)	0.055** (0.010)	0.042** (0.006)
Leader came into power			
LForeignMadeLeader$_{t-1}$	–0.015 (0.042)		
LIrregLeader$_{t-1}$	–0.001 (0.006)		
Leader lost power			
LIrregExitLeader$_{t-1}$	0.007 (0.011)		
LMilitaryLeader$_{t-1}$	0.004 (0.006)		
Leader characteristics			
LLeaderAge$_{t-1}$	–0.0001 (0.0002)		
LLeaderFemale$_{t-1}$	–0.001 (0.008)		

Country dummies included	Yes	Yes	Yes
Number of observations	1,391	1,391	2,263
Number of countries	80	80	95
Joint model reduction test of insignificant regressors	$Chi^2(8) = 13.4$	n.a.	n.a.

Notes: Biased-corrected least-square dummy variable estimator based on Kiviet (1995) and Bruno (2005). Bootstrapped standard errors in parentheses. * and **indicate significance at the 5% and 1% level, respectively.

Interpreting the coefficients is not straightforward: by themselves, they do not provide a clear idea about either the absolute or relative magnitude of the estimated effects. To illustrate the impact of the significant variables on *JI Gap*, we compute the average contribution a variable makes to closing the gap. Matters are further complicated by the dynamic nature of the model in the form of a lagged dependent variable, as we have to distinguish between the short- and the long-term effect of a variable. Given the notable degree of persistence in *JI Gap*, the long-terms effects are much stronger than the short-term ones.

Using the estimates from Table 3 for the reduced model based on the larger sample, which are the most efficient estimates, to compute the quantitative effects, we find that in the short term, a 1 percentage point (pp) increase in *ΔlnDemocracy* reduces *JI Gap* by 10% compared to its mean. Since it is unclear how relevant a change of this magnitude is in practice, a typical change in the explanatory variable could be proxied by its standard deviation (SD). Thus, it is useful to consider the resulting SD change in *JI Gap* after a 1 SD change in the explanatory variable. For *ΔlnDemocracy*, a 1 SD change is associated with a 2.5 SD drop in *JI Gap* in the short run and an 18 SD drop in the long run. In the case of *ΔlnNoVetoPlayers*, a 1 pp increase in the change in democracy reduces *JI Gap* by about 2.5% compared to its mean in the short term. The corresponding reductions in terms of SDs are about 2 in the short term and 13 in the long term. A 1 pp hike in *ΔlnPressFree* lowers *JI Gap* by about 8% in the short run, which is equivalent to a reduction of about 1.5 SD in *JI Gap* after a 1 SD increase in *ΔlnPressFree* (12 SD in the long term). Finally, a 1 pp increase in *ΔlnCorruption* widens the gap by almost 14% in the short term. In terms of SDs, a 1 SD hike in *ΔlnCorruption* is associated with a 3 (21) SD increase in the short term (long term).

Thus, all the estimated effects on *JI Gap* are substantial, and even more so in the long term. Measured in SDs over the short term (long term), the impact of the four significant

explanatory variables ranges from 1.5 to 3 (12 to 21), with the relatively strongest (weakest) impact estimated for corruption (press freedom).

6 Conclusion and Outlook

In this paper, we identify the dynamics contributing to the gap between the judicial independence guaranteed by a country's constitution and the judicial independence actually realized. We find that increases in corruption are associated with larger gaps, whereas a greater number of veto players and a higher degree of both freedom of the press and democracy are associated with a smaller gap.

Our interest is in the effects of *constitutional* constraints; we ignore any role statutory law may play in determining the *de facto* degree of judicial independence. Analyzing the relevance of statutory law in this context would be interesting, but data collection is extremely difficult. Moreover, due to missing data, we were not even able to test all the hypotheses derived from our theory section. Such must be left for the future when better data are available.

One potentially relevant dimension of the *de jure-de facto* gap is completely absent from this paper, namely, the relevance of other nation-state governments and international organizations. These could be relevant in a variety of ways. For example, a government that tinkers with the independence of its judiciary could damage its good reputation not only among other nation-states but also among potential foreign investors. These losses could, potentially, be large enough to garner renewed respect for judicial independence. International organizations based on treaties that provide for monitoring of and sanctioning of deviations in judicial independence are another relevant pathway for change in the JI gap. The European Union has such a mechanism as do various other international organizations, such as the Council of Europe and the Inter-American Human Rights System. The EU mechanism has been widely criticized, but it is not clear whether it has actually worked or not.

Finally, most of the theoretical conjectures discussed in this study are not confined to the judiciary. There are many ways in which the executive can renege on constitutional constraints and they are in no way confined to interfering with the judiciary.

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Table A1.1: *JI Gap* (de facto JI < de jure JI): List of 148 countries and observation periods

Country	Start	End	Country	Start	End	Country	Start	End
Afghanistan	1990	2000	Gabon	1960	2018	North Macedonia	1991	2018
Albania	1950	2018	Gambia	1996	2018	Oman	1996	2018
Algeria	1989	2018	German Democratic Republic	1950	1990	Pakistan	2002	2018
Angola	1975	2018	Germany	1950	2018	Panama	1950	2018
Argentina	1983	2018	Ghana	1992	2018	Papua New Guinea	1975	2018
Armenia	1995	2018	Greece	1975	2018	Paraguay	1992	2018
Austria	1950	2018	Guatemala	1965	2018	Peru	1979	2018
Azerbaijan	1991	2018	Guinea	1990	2018	Poland	1950	2018
Bahrain	1973	2018	Haiti	1964	2018	Portugal	1976	2018
Bangladesh	1986	2018	Honduras	1950	2018	Qatar	2003	2018
Barbados	1966	2018	Hungary	1950	2018	Romania	1991	2018
Belarus	1994	2018	Indonesia	2001	2018	Russia	1950	2018
Belgium	1999	2018	Iran	1979	2018	Rwanda	2003	2018
Benin	1979	2018	Iraq	2004	2018	Sao Tome and Principe	1975	2018
Bhutan	2005	2018	Ireland	1950	2018	Saudi Arabia	1992	2018
Bolivia	1967	2018	Italy	1950	2018	Senegal	1960	2018
Botswana	1966	2018	Ivory Coast	2000	2015	Seychelles	1993	2018
Brazil	1950	2018	Jamaica	1962	2018	Sierra Leone	1961	2018
Bulgaria	1950	2018	Japan	1950	2018	Slovak Republic	1993	2018
Burkina Faso	1991	2018	Jordan	1950	2018	Slovenia	1991	2018
Burundi	1992	2017	Kazakhstan	1993	2018	Solomon Islands	1978	2018
Cambodia	1993	2018	Kenya	1963	2018	Somalia	1979	2011
Cameroon	1961	2018	Kosovo	2008	2018	South Africa	1983	2018
Cape Verde	1980	2018	Kuwait	1992	2018	South Korea	1950	2018
Central African Republic	2004	2018	Kyrgyz Republic	2007	2018	Spain	1978	2018
Chad	1996	2018	Laos	1952	2018	Sri Lanka	1978	2018
Chile	1980	2018	Latvia	1991	2018	Sudan	1998	2018
China	1982	2018	Lebanon	1950	2018	Switzerland	1999	2018
Colombia	1991	2018	Lesotho	1993	2018	Syria	1973	2011
Comoros	1996	2018	Liberia	1950	1979	Taiwan	1950	2018
Congo	1992	2018	Libya	1951	2010	Tajikistan	1994	2018
Costa Rica	1950	2018	Lithuania	1992	2018	Timor	2002	2018
Croatia	1991	2018	Madagascar	1975	2009	Togo	1979	2018
Cuba	1959	2018	Malawi	1994	2018	Tunisia	1959	2010
Czech Republic	1993	2018	Maldives	2008	2018	Turkey	1950	2018
Dem. Rep. of the Congo	2003	2018	Mali	1974	2018	Turkmenistan	1992	2007
Denmark	1953	2018	Mauritania	1991	2018	Uganda	1995	2018
Djibouti	1977	2018	Mexico	1987	2018	Ukraine	1990	2018
Dominican Republic	1950	2009	Moldova	1991	2018	United Arab Emirates	1971	2018
Ecuador	1998	2018	Mongolia	1992	2018	Uzbekistan	1992	2018
Egypt	1964	2010	Montenegro	2006	2018	Vanuatu	1980	2018
El Salvador	1983	2018	Morocco	1962	2018	Venezuela	1961	2018
Equatorial Guinea	1982	2018	Mozambique	1975	2018	Vietnam	1960	1979
Eritrea	1997	2018	Myanmar	2008	2018	Yemen Arab Republic	1991	2018
Estonia	1991	2018	Namibia	1990	2018	Yemen People's Repub.	1970	1990
Eswatini	2005	2018	Nepal	2006	2018	Yugoslavia	1974	2018
Ethiopia	1987	2018	Nicaragua	1987	2018	Zambia	1991	2018
Fiji	1990	2008	Niger	1992	2008	Zimbabwe	1965	2014
Finland	1950	2018	Nigeria	1999	2018			
France	1958	2018	North Korea	1950	2018			

Table A2: Descriptive statistics of variables listed in Table 2

Variable	Obs.	Mean	Std. Dev.	Min.	Max.
JI Gap	4,946	0.98	0.78	0.00003	3.85
Parliamentary System	4,946	0.22	0.41	0	1
Number of Veto Players	4,946	0.23	0.22	0	0.73
Amendment of Constitution	4,946	0.55	0.50	0	1
Degree of Press Freedom	4,946	1.75	0.86	1	3
Degree of Democracy	4,946	1.13	7.63	-10	10
Degree of Corruption	4,946	0.44	0.31	0.01	0.97
Leader came into power through					
• Foreign Intervention	4,946	0.02	0.13	0	1
• Irregular Means	4,946	0.18	0.38	0	1
Leader lost power through					
• Foreign Intervention	4,946	0.01	0.08	0	1
• Irregular Means	4,946	0.14	0.35	0	1
Military Leader	4,946	0.20	0.40	0	1
Female Leader	4,946	0.97	0.16	0	1
Leader's Age	4,946	64	11.7	0	1
1950s	4,946	0.12	0.32	0	1
1960s	4,946	0.16	0.36	0	1
1970s	4,946	0.20	0.40	0	1
1980s	4,946	0.24	0.43	0	1
1990s	4,946	0.29	0.45	0	1