How Reform Happens

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Abstract

What determines whether and how regulations are reformed? We use a newly constructed data set of 3,590 successful and failed regulatory reforms in 189 countries, between 2005 and 2022, to address this question. We document that regulations have become more business friendly in some regulatory domains but not others. We also show that regulations are more business friendly in richer than in poorer countries, and that holding initial regulatory levels constant, richer countries also reform more. We present a model in which the successful passage of reforms is shaped by the number of veto points in the approval process, the social returns to reform, and the cost of compensating losers from reform, and then test it using our new data set. We find that richer countries have both higher reform attempt and success rates, but less impact of individual reforms on regulation than poorer countries. These findings are consistent with the model if richer countries are better at reform, perhaps because they can compensate losers more efficiently. Across the world, reform attempt rates are strongly correlated with reform success rates but not with reform impact levels. Within countries, a higher share of technological reform attempts is successful, compared to administrative or legal reforms, consistent with the importance of veto points.

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I. Introduction

What determines the pace of regulatory reform around the world? A tradition associated with Coase (1960) holds that reforms raising aggregate wealth will generally be implemented because losers from such reforms can be compensated by the winners (Acemoglu 2003). According to this view, reforms occur when their social benefits are high and when compensation of losers is efficient, as might be particularly true in richer countries (Lipset 1959). A different tradition, associated with Olson (1982), holds that welfare-improving reforms are frustrated by interest groups with an ability to veto change and that such groups proliferate as countries develop, which makes reforms harder. In this view, reforms are rare, especially in rich countries, occurring only when they can bypass this veto power.[2] But which view is correct?

We evaluate the Coase-Olson divide empirically, using a newly constructed data set of 3,590 regulatory reform attempts in 189 countries. This dataset includes reforms that were proposed but not passed. We measure changes in government regulations from 2005 to 2022 in six domains: contract enforcement, labor, insolvency, minority shareholder rights, tax collection, and business entry. The data specifies which branch of the government proposed the reform, and which branch stopped failed reforms. We further collect data on whether reforms are legal, administrative, or technological, which relates to the veto points they face. We build a model of reform with both Coasean and Olsonian elements and use our data to investigate its predictions.

Section II reports three initial findings. First, over the sample period, across the world, it has become much easier to start a business, for businesses to pay their taxes, and – at least on

[2] We associate Coase with costless bargaining making institutional barriers to reform irrelevant, but that is too extreme for Coase. We associate Olson with the view that institutional roadblocks, rather than the returns to reform, drive the amount of reform. We believe that view can be fairly called Olsonian.

paper – for minority investors to receive legal protection against insiders. There has been little global change in the ease of resolving insolvency or enforcing contracts. In labor regulation many countries, particularly the richer ones, became more stringent. We use our reform data set to understand why these differences exist.

Second, during our period the raw trend toward pro-market regulations is often stronger in countries that started (and finished) with the heaviest regulations, which are the poorer ones.

Third, not only are regulations more market-friendly in richer countries, but when we control for the initial levels, the pro-business movement in regulations is also stronger. This finding is more in line with Lipset's (1959) modernization theory than with Olson's hypothesis of high-income sclerosis, which occurs because "stable societies with unchanged boundaries tend to accumulate more collusions and organizations with vested interests," and "the accumulation of distributional coalitions leads to economic inefficiency and political rigidity."

In Section III we present a model of regulatory reform combining elements of Coasean heterogeneity in the effectiveness of bargaining with an Olsonian focus on the political barriers to reform. In our model, a reformer – who could be the executive, the legislature, or the highest court – proposes a reform, accompanied by a plan to compensate some of the losers from this reform. The efficiency of such compensation is a key parameter of the model. The proposed reform must then pass several "veto points." This captures the idea that after a proposal, a reform must be approved by various parties, including the executive branch as a whole (especially in coalition governments), but possibly also the legislature and courts.

The model highlights three parameters that determine the frequency of reform attempts, the success rate of these attempts, and the impact of reform conditional on success. As in

Tsebelis (1995), the number of veto points shapes the probability of success and the attractiveness of attempts, especially across domains within countries. All sample countries have an executive branch, a legislature, and a judiciary with at least some powers. However, some types of reform, which we measure as administrative or technological reforms, are vulnerable to fewer vetoes than legal ones. We keep track of these differences empirically.

We measure two further parameters indicated by the model that vary across countries: the returns to reform and the ease of compensating losers from reform, which we take to capture the efficiency of Coasean bargaining. Higher returns to reform lead to more frequent reform attempts, a lower reform success rate, and a stronger link between reforms and subsequent changes in regulations. More efficient Coasean bargaining also leads to more reforms attempts, but those reforms have a higher success rate and less impact conditional on success.

While our model focuses on Coasean efficiency, we have no way to empirically distinguish such efficiency from other variables that make reform easier. Consequently, we see our empirical work as mostly distinguishing between the returns to reform and the ability to reform. Standard economic analysis emphasizes the welfare benefits from reform, but we instead ask whether the ability to complete reforms is more important for explaining cross-national differences in the amount of reform.

We next present our empirical analysis of reforms. Our primary data is a list of 3,590 attempted regulatory reforms across the 6 domains in 189 countries between 2005 and 2022. We group reform efforts into three broad categories: legal (requiring new laws), administrative (requiring agreements within a branch of government), and technological (reapplying existing legislation). We also categorize reform attempts based on which branch of government initiates them (executive, legislature, or judiciary) and which branch stops any failed reform.

Our first empirical tests check whether our reforms actually capture regulatory change. Event study analyses show that our indicators of market-friendliness of regulations improve after a successfully passed reform, controlling for country and year fixed effects. We find that reforms impact the index of outcomes in every domain, but the effects of reform are higher in starting a business, paying taxes, and protecting minority shareholder rights than in the other three areas, especially labor regulation.

Second, we turn to within-country analysis of the effect of veto points, which clarifies the patterns of change across domains documented in Section II. Reforms in almost all categories, except for enforcing contracts, are most likely to be initiated by the executive. The executive has the highest probability of success with reforms in starting a business (90%) and resolving insolvency (81%), followed by labor regulation, paying taxes, and investor protection (between 63% and 72%). Enforcing contracts, an area where courts exercise their veto, is a low of 39%.

Across types of reform, technological reforms have a high success rate in every domain, but these mostly occur in starting a business, paying taxes, and resolving contract disputes. Administrative and legal reforms, which presumably face more veto points, are also generally successful but not as successful as technological ones.

The substantial regulatory changes observed for starting a business and paying taxes can be explained by the prevalence of technological reforms in these domains. We also see large changes for minority shareholder protections despite the legal nature of reforms. In labor and resolving insolvency reforms are almost exclusively legal and have low average impact.

Third, we turn to cross-country variation in the prevalence of reform, starting with the fact that richer countries experience more improvement in their index values, holding the initial

values constant. Following the model, we ask whether this pattern reflects higher returns to reform in richer countries or a better ability to get reforms passed. We find that richer countries attempt more reforms, have a higher success rate conditional on attempting, but reforms have less impact on regulation. In poor countries, 25 percent of technological reforms are stopped by the executive. The comparable rate for rich countries is 3 percent. These findings suggest that richer countries are more Coasean than poorer ones and are consequently better at getting reforms passed. They are not be more sclerotic.

We then turn to the convergence also documented in Section II. We again ask whether the greater regulatory change in countries with low initial index values (low market friendliness) is driven by higher returns to reform or a greater ability to enact reform. We find that the returns to reform are higher but reform rates are lower in countries with low initial index levels. Convergence reflects high returns to reform in countries with less market friendly initial regulations, rather than to more Coasean bargaining.

Finally, we look at the patterns of reform attempts and ask whether they are linked to higher returns to reforms or a greater facility at getting them passed. We see this exercise as akin to asking if the quantity of reform is driven more by demand, which should reflect the returns to reform, or the supply of reform. Across countries, we find a strong positive correlation between the number of reform attempts and their success rate, and either no or a negative relationship between the frequency of reforms and their impact on regulations. This evidence suggests that capacity rather than the need for reform largely shapes reform efforts.

Our results suggest that economists should consider the capacity to enact a policy change, as much as its impact of a policy. While the wealthy world is far more Coasean than Olson implies, even in rich countries it is easier to enact technological reforms (96 percent passed) than

legal ones (82 percent passed). Olson was wrong to believe that change stops in rich countries, but reform does take on definite forms shaped by veto points as much as by economic benefits.

The framework presented in this paper is complementary to recent work on the political economy of reforms. Closest work to our paper is Alesina et al (2006) and Alesina et al (2023), who ask when structural reforms happen and how their timing affects their reception by the public. The emphasis in their paper is on when reforms are implemented, whereas our paper focuses on mechanisms behind the successful passing of structural reforms, on the importance of veto players, and on the ability to compensate losers as predictors of successful reforms. Other work emphasizes factors such as the role of state and administrative capacity (Besley and Persson 2009) and the role of interest groups and lobbying in shaping policy (Grossman and Helpman 2001 for tariff policy; Mian et al 2010 for financial policy; Kaplan and Naidu 2025 for unions; Bombardini and Trebbi 2025 for firms). Hartley, Pallan, and Wheaton (2026) describe the effects of market reforms on economic growth.

II. Data and Initial Facts

We construct a new data set of regulatory reforms between 2005 and 2022. Reforms are grouped into six domains: starting a business, labor regulation, paying taxes, protecting minority investors, enforcing contracts, and resolving insolvency. We select these six domains of study due to the presence of already-collected data on actual regulations from the World Bank. Our empirical analysis makes use of the two data sets together, although they were collected independently of each other. This has an important implication: even a successful reform need not change regulatory scores that we measure because it deals with other matters.

The regulatory reform data set is constructed by a research team at the London School of Economics. A reform enters our data set when it first becomes public as it is announced or published in a governmental schedule or announced as a technology-driven improvement by the respective state institution. We refer to this date as reform initiation. Importantly, if a reform is attempted multiple times, we de-duplicate reform instances and use the latest date of initiation for that reform. Data on initiation are gathered from the websites of national parliaments and executive offices, as well as ministries that can initiate legal and administrative changes. We also included announcements by public agencies or their external partners, such as the European Commission. A reform only enters our data if it is proposed by an official branch of government, a ministry, or an executive agency. We do not count public pronouncements or campaign promises that are not officially announced. For each initiated reform, we keep track of which branch of government — executive, legislative, or judicial — had proposed it.[3] We classify reforms into the six domains discussed above.

We measure three distinct features of these reforms to assess the determinants of their passage. First, we classify reforms into successful and failed. A successful reform in our definition is one that ultimately enters into force, regardless of its impact. If a reform is tried repeatedly and is eventually adopted, we classify it as successful and do not count the previous failed attempts. If a reform is tried but not adopted at the end of the sample, we classify it as failed. If the reformers give up and do not retry to pass a reform, it is also classified as failed. A failed reform is thus one that is announced by the prime minister, Presidency, the justice

[3] One risk with this data is that early stage reforms are more likely to be announced in some countries than others, which might lead us to report higher failure rates in countries with more early stage announcements. This underreporting seems less likely to be problematic for legislature-initiated reforms, because legislation is typically announced in official legislative records. We repeated our major tables for legislature-initiated reforms and found few major differences relative to the whole sample.

minister, or the head of parliamentary committee, drafted as a legislative proposal or an administrative act at the ministry or government agency, but then is not approved by the administration or adopted by Parliament or the Presidency. This category also includes reforms that are quickly repealed by a new government or invalidated by a Constitutional Court ruling.

Second, the data set specifies which branch of the government proposed the reform, and which stopped it when it failed. Executive-proposed reforms are led by the head of state, government ministries, or presidential commissions, often culminating in strategies, executive orders, or decrees. Legislative-proposed reforms originate in parliament or congress and tend to focus on new laws or constitutional amendments through a legislative process involving debate, approval, and enactment. Judiciary-proposed reforms are led from within the judicial branch—by supreme courts, judicial councils, or chief justices—and typically result in judicial rulings, administrative orders, court circulars, practice directions, or internal guidelines and codes. The proposer and the approver of reform usually differ. Most frequently, the executive proposes legislation, or introduces bills and draft laws, while the legislative passes or enacts them.

Third, we further collect data on whether reforms are legal, administrative, or technological. Legal reforms require acts of parliament and are also often subject to presidential approval. Examples are new labor laws or revisions of company law. Administrative reforms may require a ministerial decree or internal order by the head of a government agency. Examples include the creation of a specialized large-payer unit in the Tax Office or the creation of an insolvency court in the judiciary. Technological reforms typically do not require a new legislative act or even a ministerial decision and instead use already-established legislation. Examples include the implementation of online submission of tax returns or the electronic registration of a business. These reforms can be directed by the respective administration.

A few examples illustrate how we use these definitions to characterize reforms. The government of Argentina's President Macri proposed a draft amendment of the Labor Code that aimed to reduce employment costs, deregulate the employment market, and promote investment. The proposal led to widespread street protests and failed to gain a majority in Parliament. We code this as a failed labor reform that was legal in nature and vetoed by legislature. In 2011, the Bangladeshi government tried to adopt a Tax Modernization Plan that would separate tax policy from tax collection and digitize the tax administration system. The reform was ultimately abandoned in 2012 due to rising political pressures from opposition parties. We code this as a failed legal reform proposed by the executive and vetoed by the legislature. In 2018, Togo attempted to make enforcing contracts easier by adopting a law that regulates all aspects of mediation as an alternative dispute resolution mechanism. This law took three years to pass and we code it as a successful 2021 enforcing contracts legal reform initiated by the executive. The Indonesian government initiated a bankruptcy law reform in 2004, promulgating Law Number 37 of 2004 on Bankruptcy and Suspensions, which empowered the Judicial Commission to supervise judges' conduct. Judicial resistance to this reform culminated in a 2006 Constitutional Court case that weakened the Judicial Commission's oversight powers. This is coded as a 2005 failed administrative Resolving Insolvency reform initiated by the Executive branch and stopped by the Judicial Branch.

The Doing Business Database

In addition to our new data on reforms, we use a set of available country-level indicators collected by the World Bank that measure the burden of regulation from 2005. The methodology for constructing these variables was developed by some of the present authors: starting a business by Djankov et al (2002); enforcing contracts in Djankov et al (2003); paying taxes in

Djankov et al (2010); indicators on resolving insolvency in Djankov, Hart and others (2008); employing workers in Botero et al (2004); and protecting minority investors in Djankov et al (2008). Previous research shows that these indicators have significant impact on economic outcomes, with lower regulations generally associated with better results. This data was last updated by the World Bank in 2021.

The regulatory indicators are based on fictitious case studies of a local business. Local respondents answer questions about the difficulties involved in different business tasks, and the World Bank averages these responses. We checked and extended these data to 2022. Each of the six domains has multiple measures, which we combine to form a single index for each domain. For comparability across domains and time periods, as well as for aggregation across domains, we convert regulatory scores into z-scores of market-friendliness. Table 1 presents the definitions of the variables we use.

In some domains, such as labor regulations and protecting minority shareholders, the Doing Business measures are based entirely on laws and regulations, not experiences. In others, such as starting a business or paying taxes, some of the variables reflect experience, such as the typical time it takes to start a business, some reflect laws, such as the number of procedures needed to start a business, and some reflect a hybrid or laws and experiences, such as the cost of resolving insolvency. In categories where we have both legal and experiential variables, we do not typically observe significant trend differences between the two types of variables.

Figures 1, 2, and 3 present regulatory trends for starting a business, paying taxes, and minority shareholder protection by income group. In these domains, significant change has occurred: average z-scores improved substantially for both rich and poor countries, with poor

countries narrowing the gap. For paying taxes, for example, the average z-score improved from 0 to 0.3 for rich countries and from -0.6 to 0 for poor countries. This corresponds to an average 3.1 days reduction in the time it takes to pay your taxes in rich countries and 3.9 days reduction for poor ones, and a 10.7 fall in the number of tax payments per year for rich countries and 12.1 fall for poor ones. However, convergence between rich and poor countries slows or reverses after 2019, with poor countries largely flattening and rich countries continuing to progress. Figures 4 and 5 show the domains with smaller changes, the enforcement of contracts and resolving insolvency. In these domains, poor countries made some gains over the period but saw a reversal after 2020, reflecting the impact of COVID-related court closures on the resolution of contract disputes and insolvency cases. Figure 6 describes labor regulation, where the richer countries have modestly increased regulatory burdens.

The rapid reform group includes two domains that relate to government-firm relations (paying taxes and starting a business), and one that relates to private relationships (minority shareholders). The low reform group only includes private relationships (firm-firm or firm-labor). Figure 7 shows the aggregate pattern across all six domains of business regulation: both rich and poor countries improve through 2019, but the gap between rich and poor remains or even widens with the advent of COVID.

Table 2 documents that richer countries generally experience more change, holding the initial levels constant. Both at the beginning and end of our sample, it was easier to do business in richer countries. The strong correlation between initial income and initial market friendliness of regulation explains why initial income is negatively correlated with subsequent reform when we do not control for the initial level of regulation. In Table 2, we regress across countries the

change in regulation (as a z score) for every regulatory feature in each domain on the initial level of that feature and the initial log of GDP per capita. There is convergence in regulatory indices.

Our evidence shows that reforms in richer countries, conditional on initial levels, improve regulatory scores. This is clear for starting a business and paying taxes, the two domains that show significant aggregate shifts, and for enforcing contracts and resolving insolvency, the two domains that do not display aggregate change. There is no correlation between income and change for minority shareholder protection and labor regulation. The evidence shows that richer countries start in a better place and reform more controlling for convergence. These results are not supportive of Olson's high-income-sclerosis hypothesis. Olson is right that rich countries do not change all that much, but wrong that they are trapped in particularly bad outcomes.

At the broadest level, these findings point to a world between Coase and Olson. In some domains, especially those relating to business-government interactions, there has been significant reform, while other areas have seen no change. There is substantial regulatory convergence, but controlling for initial regulation levels, richer countries pass more reforms. In the next section we present a model of economic reform aiming to explain these three facts and offer additional predictions. We then evaluate the predictions in Sections IV and V.

III. A Theory of Reform

We present a model compatible with the three facts we have documented. We assume that reform capability is affected by three important factors: two cross-country and one cross-domain. First, the returns to reform are higher in some countries than others, with higher returns for countries where initial regulations are more onerous. Second, some countries are better at

compensating losers than others. If richer countries have better functioning institutions, as in Lipset (1959), we can explain the income fact. Third, reforms facing more veto points are more likely to fail, even within the same country. This helps us understand why we see more reform in starting a business than in resolving insolvency or labor regulation.

The model also generates ancillary predictions about the reform process itself, which we test with our reform database. As countries differ in at least two major ways – the returns to reform and the ease of compensating the losers from reform – the model suggests tests of which variable is more important in driving the frequency of reform. Are reforms more common in countries where they are most needed or in countries better at getting them passed?

A reformer can initiate a reform, such as easing registration of a new business or enhancing shareholder rights. This reformer could come from the executive branch of government such as the president or a minister, or be a legislator or a judge. We assume that the reformer's benefit from initiating a reform is given by $B + \alpha \cdot Social\ Benefit$, where B in the net private benefit to the reformer from initiation, which would include any political support gained from an interest group from introducing the reform and would subtract the political costs of introducing the reform, including the leader's time and effort, α is the degree to which the reformer internalizes public benefits and *Social Benefit* is the expected utilitarian sum of benefits and losses from reforms across society as a whole. The assumption that the private benefit B accrues from attempting a reform rather than getting it passed helps explain why some welfare reducing reforms are attempted. It also simplifies the analysis.

We denote by R the social benefit of reform with no compensation, which we call "pre-compensation returns to reform." The reform creates L losers, who have costs from reform

uniformly distributed on the interval $[0,2|R|]$. As the average loss per loser is $|R|$, total expected losses equal $L|R|$. We are implicitly assuming that the both that the level of harm and the level of benefit scale up with the overall size of the reform. If the reform is enacted with no attempt to compensate the losers, the gross benefit to winners is given by $R + L|R|$. We expect R to be higher in countries with heavier regulations, and L to be higher for labor regulation than for reforming taxpaying. The benefits to the reformer are based on the social benefit of the reform after compensation, which means that, holding R constant, these benefits are independent of L if compensation is perfect. While for most reforms R is positive, with a high enough B reforms with a negative R can be proposed.

The reformer can craft the reform to compensate for the losses. For example, a reform that makes it easier for outsiders to compete with incumbent firms or workers can include credit guarantees or separation payments, and a reform that makes firing easier can include retraining benefits for unemployed workers. Reformers provide either zero or complete compensation to each loser, because losers who only receive partial compensation still oppose the reform. It is then optimal to fully compensate those who lose the least from the reform first.

We assume that providing 1 dollar of compensation costs $1 + \theta$ dollars. The variable $\theta > 0$ captures the inefficiency of bargaining and compensation. We think of θ as varying across countries, rather than domains or types of reform. Under Lipset's modernization hypothesis, θ is lower in richer countries.

Figure 8 describes how the costs of compensation increase with the share of the losers who are compensated (q). As losses are uniformly distributed on $[0,2|R|]$, the loss to the loser at the q'th percentile of the L losers is $2|R|q$. The green line in the figure captures loss

experienced by the marginal compensated loser. If everyone with lower losses than that is compensated, the average loss in that group is $|R|q$. The blue line shows this average loss as a function of the share of compensated losers, which has exactly one-half the slope of the green line. The orange line just multiplies this average loss ($|R|q$) by the share of losers who are compensated (q). Finally, the black line shows the total cost of compensation, which equals the average loss ($|R|q$) times the share of losers who are compensated (q) times the number of losers (L) times the inefficiency of compensation ($1+\theta$), which comes to $(1+\theta)q^2|R|L$. The benefits to the winners fall to $R+|R|L-(1+\theta)q^2|R|L$.

If everyone with losses greater than the q-th percentile loser (whose loss is $2|R|q$) is put in a group, then the average loss in that group is $(2|R|q+2|R|)/2$. The total losses experienced by this uncompensated group are (1-q)L (the number of uncompensated losers) times $(1+q)|R|$ (their average loss) or $(1-q)(1+q)|R|L=|R|L(1-q^2)$. If we subtract these losses from the benefits accrued to the winners, we get net social benefits of $R-\theta q^2|R|L$, which we refer to as the post-compensation social returns from reform. The second part of this term can be broken down as the average loss experienced by the compensated losers ($|R|q$) times the number of losers who are compensated (qL) times the social cost of compensation (θ). The results are similar if the initiator only values the benefits to the winners.

Reforms face N veto points, which can include formal entities, such as the Parliament or the Supreme Court, but also organized interests such as unions. After the reform process commences, each of the N veto points can potentially derail the reform. Each veto point is sensitive to losses from reform, which we operationalize by assuming that the probability that each veto point rejects the reform equals a constant (φ) times the number of uncompensated

losers, $(1-q)L$, where we assume that $\varphi L \leq 1$. Our core results would not change if we assumed that the probability of rejection is any increasing function of the number of uncompensated losers, but this would make the algebra more tedious.

To recapitulate the timing: (1) the reformer decides whether to initiate a reform, (2) the reformer can craft the reform to reduce the share of people who lose from it, which reduces the net social benefit from successful reform from R to $R - \theta q^2|R|L$, (3) the reform is considered by N veto points each of whom rejects it with probability $\varphi(1-q)L$ While we focus on variation in R and θ across countries, we focus on the variation in N across types of reform within a country. We assume that N is higher for reforms that involve legislation than for reforms that involve technological or administrative changes within the executive branch.

Our propositions follow with these assumptions (proofs are in the appendix). The first describes the reformer's optimal breadth of compensation, conditional on initiating a reform.

Proposition 1: If R<0, then no losers are compensated. If R>0 and $\theta < \frac{\varphi N}{2+\varphi NL}$, then all losers are compensated. If $\theta > \frac{\varphi N}{2+\varphi NL}$ then conditional on initiating a reform, the reformer proposes to compensate a positive fraction $q^* < 1$ of the losers. The breadth of compensation and total compensation (q^*) is declining with θ and increasing with N and φ. The breadth of compensation (q^*) is increasing with L if and only if $\theta > \frac{(2+N)N\varphi}{4}$, but the total amount of compensation ($q^{*2}L$) is always increasing with L. If R>0, the expected social returns to initiating reform are always decreasing in L and θ and increasing in R. They are independent of N and φ when $\theta < \frac{\varphi N}{2+\varphi NL}$, and decreasing with N and φ when $\theta > \frac{\varphi N}{2+\varphi NL}$.

When the social returns to reform R are negative, the reformer is only initiating because B (the returns to initiating reform) is sufficiently high. Neither the society nor the reformer benefits if the reform is actually enacted, so the reformer does not craft the reform to increase its

probability of being enacted. Still, such reforms may occasionally pass. This result would change if B was the private benefit conditional on enactment, rather than just initiation.

If R is positive and compensation is efficient enough ($\theta < \frac{\varphi N}{2+\varphi NL}$) then all losers are fully compensated.[4] If R>0 and $\theta > \frac{\varphi N}{2+\varphi NL}$, then there will be partial compensation. The breadth of compensation is independent of R because we have assumed that both the benefits and the costs of compensation scale with R. With partial compensation, the scale of compensation increases with the number of veto points (N), because veto points magnify the power of the losers and consequently increase the returns to buying them off. The post-compensation returns from reform also fall with N if and only if $\theta > \frac{\varphi N}{2+\varphi NL}$, which shows that institutional structure is more likely to impact outcomes when efficient side-payments are not available. Unsurprisingly, the amount of compensation is declining with θ which raises the effective cost of compensation. The amount of compensation increases with φ, because φ strengthens the link between compensation and the probability of reform success.

As the number of losers (L) increases, the amount of compensation always rises, but the share of losers receiving compensation can either rise or fall. Increasing L means that the cost of compensating the same share of losers rises, but the benefit of doing so rises as well. As the benefit scales with the share of people compensated but the cost scales with the share squared, the benefits effect outweighs the cost effect when the share of compensated losers is small.

[4] The condition $\theta < \frac{N}{2+NL}$ seems to imply that full compensation is likely. For example, if N=2 and L=.5, then full compensation occurs whenever $\theta < .7$, which seems likely to hold in most places. However, this result is quite sensitive to the assumption that the probability of any veto point rejecting equals the number of uncompensated losers. If the link between the number of uncompensated losers and the probability of rejection is weaker, then the upper bound for θ would be lower.

Assuming $\theta > \frac{(2+N)N\varphi}{4}$ guarantees that result. The post-compensation returns to reform are always decreasing in L and θ, because these parameters determine the costs of compensation. This result may shed light on the limited change in labor regulation.

Figure 9 captures the intuition of Proposition 1. The lower line, the "Reform Possibilities Frontier," reflects the tradeoff between the probability of success $(1-\varphi(1-q)L)^N$ and the returns conditional on success $R - R\theta q^2 L$. As the reformer offers a higher q, the probability of success rises, but the payoff from reform conditional on success falls. This curve is the "budget set" available to a would-be reformer. The top line is the indifference curve of the reformer. Since the reformer is an expected welfare maximizer, the curve sets the probability of success equal to the utility level implied by the indifference curve divided by the return conditional on success. The optimal breadth of compensation (q) occurs at the point of tangency. We have assumed that *B* is the net private benefit of initiating reform rather than achieving reform, so this parameter does not impact the *ex post* returns to reform.[5]

The Reform Possibilities Frontier is shaped by national political institutions as long as $\theta > 0$ and especially if $\theta > \frac{\varphi N}{2+\varphi NL}$. If the reformer is proposing a reform outside of their own domain of activity (for example, the Finance Minister suggesting a reform of the judicial procedure), the probability of success shifts downward as shown in Figure 10, because the number of veto points increases. This moves the Reform Possibilities Frontier closer from the origin, meaning that either the probability of success falls or the compensation of losers increases, or both. The curve shifts far more when the returns from a successful reform are

[5] As we have assumed that the B represents private returns to initiating rather than achieving reform, it does not impact that choice of how to design the reform. If there were also private benefits from enacting reforms, then the indifference curve would shift, which would lead to more compensation of losers.

higher, because in that case the losers are not compensated and an extra veto point substantially increases their ability to stop the reform. If the losers are completely compensated, which occurs when the returns to a successful reform are high, then the extra veto point has not impact on the probability of success.

If compensating the losers from the reform is particularly easy (θ is low), then the Reform Possibilities Frontier shifts upward as is also shown in Figure 10. This shift no impact on the curve when returns to a successful return are high, because the level of compensation is zero. A decline in compensation costs makes it possible to achieve a probability of success of one with far less of a reduction in the social benefit of a successful reform.

At this optimal level of compensation, we can calculate comparative statics for the probability of success conditional on attempting reform.

Proposition 2: If R<0, the probability of success is falling with φ , L and N and independent of all other parameters. If R>0, the probability of success is equal to one if $\theta < \frac{\varphi N}{2+\varphi NL}$. If R>0 and $\theta > \frac{\varphi N}{2+\varphi NL}$, then the probability of success is declining with θ and independent of R. There exists a value of $\theta > \frac{\varphi N}{2+\varphi NL}$, denoted θ^*, such that the probability of success is decreasing with N if and only if $\theta > \theta^*$. There exists another value of $\theta > \frac{\varphi N}{2+\varphi NL}$, denoted θ^{**}, such that the probability of success is decreasing with φ if and only if $\theta > \theta^{**}$.

Proposition 2 describes the determinants of successful reform, conditional on attempting it. If R<0, the initiator does not actually want the reform to succeed, and is only proposing it to gain the private benefit B. No losers are then compensated, and the probability of success is low and depends only on the number of losers and the number of veto points. If R>0 and bargaining is efficient ($\theta < \frac{\varphi N}{2+\varphi NL}$), then all losers are compensated and the reform succeeds with

probability 1. If R>0 and bargaining is relatively inefficient, the probability of success depends on the inefficiency of compensation (θ), which always reduces it.

The number of veto points (N) and the impact of losers on the probability of rejection by any given veto point (φ) have a negative direct impact on the probability of success, but a positive indirect effect because they increase the amount of compensation given to the losers. When θ is low and compensation is relatively efficient, this indirect effect dominates and both variables (N and φ) actually increase the probability that a reform succeeds. When θ is high and compensation is relatively inefficient, the indirect effect becomes less important and both variables reduce the probability of a successful reform.

The proposition suggests that richer countries, with lower values of θ, are likely to have more successful reforms. With a low enough θ, the probability of success can actually increase with N, because the overall amount of compensation rises. In countries with costlier Coasean bargaining, veto points decrease the probability of success. Reforms are more likely to succeed when they face fewer veto points. While political theorists such as Tsebelis (1995) have focused on veto points associated with constitutional design, their logic also sheds light on types of reforms within countries. A legislative reform in a fractious democracy inevitably faces more veto points than a technological one enacted by a single agency.

Proposition 2 also predicts that institutional details matter more in some settings than in others. Just as in Coase (1960), in highly functional systems where compensating losers is easy ($\theta < \frac{\varphi N}{2+\varphi NL}$), reforms go through even when N or φ are high. Legal reforms are more likely to be successful in highly functional polities.

We next turn to the patterns of reform across countries. In the next section, we try to understand empirically whether differences in the pace of reform are driven more by differences in the impact of reform (R) or in the political capacity for reform (θ). Propositions 3a and 3b characterize cross-national differences in these two variables and their effects.

We assume (every period) that a reformer can try a reform but only does so if the the private expected payoff exceeds zero. In countries with a weak alignment between the incentives of the reformer and the social benefits of reform (captured by a low α), the private benefits of reform initiation (captured by B) determine the propensity to attempt reforms. In such cases, we would expect to see little correlation between the impact of reform and the propensity to reform. If α is sufficiently low, then reforms may be just as likely to cause harm as to increase welfare (negative R). We interpret cases in which our outcomes get worse after a reform to imply that there were other benefits from initiating the reform.

All reforms are assumed to be identical, except for the number of losers L, which can range from 0 to $1/\varphi$. The distribution of L across reforms is characterized by a density function g(L), which is positive everywhere on its support, with a cumulative distribution function G(L). We focus on socially beneficial reforms (R>0), and assume that $R > \frac{-B}{\alpha} > R \cdot Max\left[1 - \frac{\theta}{\varphi}, \frac{2}{2+N}\left(\frac{\varphi N}{\theta(2+N)}\right)^{\frac{N}{2}}\right]$. This condition guarantees that the reformer tries reforms with no losers and eschews those with $L = 1/\varphi$. Monotonicity of returns with respect to L then implies that there exists a value of L^* in the support at which the reformer is indifferent between trying and not.

Proposition 3a yields predictions for patterns across countries if differences in reform are driven by differences in the returns to reform. Proposition 3b instead focuses on a country's

ability to efficiently compensate the losers from reform. These two propositions yield predictions about whether patterns of reform are driven by the actual benefits of reform or by the capacity of the political system to let reforms happen. We expect that a high R corresponds to a low initial level of the regulatory variable, and that θ is lower in richer countries.

Proposition 3a: A rise in returns from reform (R) (1) raises the frequency of reform attempts and the frequency of successful reforms, (2) reduces the average probability of success conditional on starting a reform, (3) has no impact on the probability of success conditional on L, and (4) raises the returns both pre-compensation and post-compensation holding L constant. An increase in R can either increase or decrease the average returns post-compensation, but if $g(L^*)$ is sufficiently low, the average post-compensation returns from reform rise.

The model predicts that countries with higher returns from reform make more reform attempts and have more successful reforms. It also predicts that the average probability of success weakly falls with R because higher returns from reform mean that reformers attempt more marginal reforms (those with higher L). If one could control for the share of losers from reform, then higher returns are predicted to have no impact on the probability of success.

The connection with average returns from reform is ambiguous, because of selection effects. Higher aggregate returns to reform encourage attempts of more marginal reforms, which have lower returns than the infra-marginal reforms. If $g(L^*)$ is sufficiently small, then the selection effects are overwhelmed by the direct effect of R on the returns from reform.

Figure 11 shows the impact of returns on the frequency of reform, average success rate, and returns conditional on attempts when $g(L^*)$ is sufficiently small. As R rises, the frequency of reform and the average post-compensation return conditionally on attempt rise. The success rate falls, and the pre-compensation return rises mechanically. We test this by looking at whether countries with initially low regulatory scores attempt more reforms, and whether they succeed less conditionally on initiation.

Proposition 3b illustrates what we should expect when countries differ in the efficiency of their political process, as captured by the inefficiency of compensation (θ).

Proposition 3b: A decrease in the inefficiency of compensation (θ) (1) increases the frequency of reform attempts and successful reforms, (2) increases the probability of success holding reform type (L) constant, (3) increases post-compensation returns holding L constant, and has no impact on the pre-compensation returns to reform, and (4) can either increase or decrease the probability of success conditional upon attempting reform, but will always increase the probability of success if $g(L^*)$ is sufficiently small. A decrease in the inefficiency of compensation can either increase or decrease the average after compensation returns conditional on success, but if $g(L^*)$ is sufficiently small and $L^* < \frac{\varphi N - 2\theta}{\varphi N \theta}$, then the average after compensation returns rises as the inefficiency of compensation falls.

An improvement in the efficiency of compensation generates more reforms and more successful reforms. We will test whether richer countries reform more and more successfully. The impact on the average probability of success is ambiguous, again because a more efficient country undertakes more marginal reforms. If $g(L^*)$ is small enough to make selection effects small, the probability of success rises. Controlling for the share of losers, the prediction that the probability of success is higher in countries with more efficient compensation is unambiguous.

Compensation efficiency has no impact on pre-compensation returns. Holding L fixed, post-compensation returns rise because costs of compensation fall. Average post-compensation returns could fall, because of two types of selection effects: undertaking more marginal reforms and changing the value of q at different levels of L which changes the composition of successful reforms conditional on attempting. If $g(L^*)$ is small (which limits the impact of attempting marginal reforms) and $L^* < \frac{\varphi N - 2\theta}{\varphi N \theta}$ (there is no impact of θ on q), the direct effect dominates.

Figure 12 shows the effects of reform efficiency, defined as $(1/\theta)$, on the frequency of reform, the probability of success contingent on reform, and the pre-compensation returns

contingent on success. The first two measures both increase with efficiency. Pre-compensation returns contingent on success are independent of efficiency.

The contrasts between Figures 11 and 12 point to one of our key empirical exercises. If in the data we see that countries with higher rates of reform have higher returns from reform despite lower probabilities of success, then national differences are likely driven by the underlying economic benefits of reform. But if we instead see that countries with more reform have higher probabilities of success despite lower returns to reform, then national differences are driven by institutional factors that make compensating losers (or other costs) lower. As we show in Section IV, the data favor the latter view.

IV. The Effects of Reforms

Do Reforms Matter?

Before testing the theories of reform, we ask whether reforms matter. We do this by connecting the regulatory indices, which are our measure of "outcomes" discussed in Section II, with the reforms. Table 3 examines in an event study format regulatory measures two years after vs two years before the reform. We do this for each individual domain of regulation, but also for a composite measure. We control for year and country fixed effects. The dependent variable is the average of all the subcategories described in Table 1. In all cases, we transform the variable into a z-score (subtracting the mean and dividing by the standard deviation) before averaging.

Columns (1), (2) and (5) show strong effects in several domains. A reform is associated with a .202 standard deviation increase in the starting a business index, a .18 standard deviation increase in paying taxes index, and a .213 standard deviation improvement in the minority

investors index. By contrast, an average reform is only associated with a .022 standard deviation increase in the enforcing contracts index, a .056 increase in the resolving insolvency index, and .025 standard deviation increase (insignificant) for the labor regulation index. Pooling all the reforms, the average improvement is .131 of a standard deviation. Reforms are on average generating business-friendly change, although labor reforms are often an exception.

Indeed, many reforms do not improve regulatory scores. Because the data sets of reforms and of regulatory indices are constructed independently, reforms do not necessarily change the indexes, and indexes can change without reforms. Table 3 illustrates this phenomenon. While overall 59% of reforms make regulations more market friendly, 30% do not change the indexes, and 11% reduce them. In the domains of starting a business and paying taxes most reforms improve regulations, but in other domains they often do not change the index, perhaps because they deal with the features of regulation not covered by the World Bank measures. In a few cases, but not a trivial number in labor regulation and enforcing contracts, reforms reduce regulatory indexes, perhaps because of the political benefits of initiating reforms. We next turn to some facts about reform and relate them to the model.

Veto Points and the Nature of Reform

In the model, we thought about the returns to reform (R) and the inefficiency of compensation (θ) as national variables, and Section V use cross-national data to shed light on the theory. Here we focus on N, which differs across types of reform. If a reform is initiated by one branch of government, then we expect it to be stopped (vetoed) by another branch of government. Moreover, Proposition 2 predicts that the executive branch will be particularly

successful with administrative and technological reforms that do not require legislation. Proposition 3 predicts that such reforms will be favored by the executive branch.

Table 4 presents our basic tabulation of which branch of government proposes a reform, what share is successful, and which branch stops it if it is not. We do that for our six domains, and for the aggregate. There are 3,590 total reforms in our sample, of which 2,551 or 71% are successful (or eventually successful, since we count reforms that were tried several times and passed eventually as one observation). The majority of reforms are proposed by the executive branch, and these have roughly the same success rate as reforms overall.

Reforms are usually but not always stopped by a different branch of government. The executive branch proposes 2,368 reforms in our sample, and 27 percent of those fail. Eight percent of the executive-initiated reforms are turned down in the executive branch itself. The rest are stopped either by the judiciary (9 percent) or by the legislature (10 percent). There are far fewer judiciary-initiated reforms (419), and only 19 percent of them fail. Of these 78 failed judiciary reforms, 29 are blocked by the executive, 22 by the legislature, and 27 by the judiciary itself. There were 803 legislature-initiated reforms and 39 percent of them failed. The executive branch is responsible for nearly two-thirds of these failures, with the legislature blocking more than the judiciary. In the data, veto points matter.

Table 4 also shows the distribution of reforms across domains. Paying taxes and starting a business, where reforms are almost always initiated by the executive and generally have few organized opponents, together represent almost 50 percent of attempted reforms and almost 50 percent of successful ones. As Table 5 shows, many of these reforms are administrative and

technological, and as such do not need legislation. Courts rarely stop them. The success rate for executive-initiated reforms is 90 percent for starting a business and 68 percent for paying taxes.

A surprisingly high 23 percent of executive-led reforms for paying taxes are stopped in the executive branch. An even higher 49 percent of legislature-started reforms are stopped by the executive. Attempts to simplify tax payments are typically made by the finance ministry, but an independent tax authority must implement the change. It then becomes a separate veto point within the executive branch. Although paying taxes is not an outlier in the incidence of initiation by the executive, with a 66% share, starting a business with 85% executive share is an outlier. Reform in this domain is vulnerable to the fewest veto points.

The executive is also extremely successful with resolving insolvency reforms, but these are much rarer. It may be difficult to improve the resolution of insolvency without harming either creditors or debtors. If the number of losers is higher in these domains, the executive may only initiate relatively incremental reforms. Executive-initiated reforms in this area are most likely to be blocked by the judiciary.

The executive is much less successful in the other three domains. Twenty-seven percent of labor regulation and 16 percent of minority investor reforms are blocked by the legislature. As Table 5 shows, reforms in these domains are overwhelmingly legislative. Fifty-five percent of enforcing contracts reforms are blocked by the judiciary.

The executive branch may have a comparative advantage at reform, perhaps because compensation of losers is easier from the executive branch, or because these reforms are often administrative or technological. Still, 22 percent of reforms begin in the legislature and 12 percent begin in the judiciary. The judiciary is both the most selective and the most successful

reform initiator. Sixty-five percent of the judiciary led reforms relate to contract enforcement, and these reforms are successful 84 percent of the time. It may be hard for either the executive or the legislative branch to veto choices made by judges about the procedures followed by courts.

Legislative reforms are both more common and spread across a wider range of areas. They are most often blocked by the executive branch, again illustrating the power of veto points. Tax-related reforms are the most common type of legislature-initiated reform. Forty-nine percent of these reforms are blocked by the executive branch. Again, the entity proposing the reform (often a finance ministry) is distinct from the entity responsible for collecting taxes, and the tax authority can and does often veto administrative reforms related to taxation.

Several other findings emerge looking across the domains of reform. Reforms that are not predominantly proposed by the executive are disproportionately proposed by the branches of government that have some expertise in these areas. Thus, contract enforcement reforms are predominantly proposed by the judiciary, while the laws on minority investor protection are often proposed by legislature. In the domains that truly are the prerogative of the executive, such as starting a business and tax collection, reforms are predominantly started by the executive.

Branches of government are less likely to stop reforms that originate in the same branches and more likely to stop those originating in other branches. Reforms proposed by the executive are most likely to be stopped by legislature, although in some domains the judiciary pulls the plug. There is some deference to the domain of specialization of each branch of government. Most successful reforms by the judiciary are concerning contract enforcement and minority investor rights.

Table 5 provides an alternative perspective on the same reform outcomes by looking at the success rate depending on whether the reform is technological, administrative, or legislative. Intuitively, technological reforms are less likely to face external veto points than legislative ones, and that appears to be confirmed in the data. The ratio of successful to unsuccessful reforms is by far the highest for technological reforms (6:1), with administrative and legal reforms at the more modest (2:1).

Table 5 shows that 53 percent of failed reforms are legal, and 55 percent of those are stopped by the legislature. The remainder of failed reforms are mostly administrative, and occasionally technological. The failures of administrative reforms are largely due to the executive itself. For example, 97 percent of administrative reform failures related to paying taxes (115 out of 119) are due to the executive branch blocking reforms.

The starting a business category is dominated by technological and administrative reforms, which succeed 98 and 80 percent of the time respectively. Legal reforms in this domain succeed 86 percent of the time. In every category, technological reforms succeed more than 70 percent of the time, but opportunities for such reforms mostly appear for starting a business, paying taxes, and enforcing contracts.

Administrative reforms are also most common in these three domains, although there are also some administrative reforms related to resolving insolvency and minority investors. There are almost no administrative reforms in the domains of labor regulation. However, the success rate for administrative reforms is relatively high for starting a business (80 percent), but lower for resolving insolvency (63 percent), enforcing contracts (57 percent), paying taxes (43 percent), and minority investor protections (42 percent). Administrative reforms related to

enforcing contracts, resolving insolvency and minority shareholders are typically blocked by the judiciary, which enjoys veto power over the administrative reforms related to legal processes. Administrative reforms related to paying taxes are disproportionately blocked by the executive.

The success rates for legal reforms in all domains except enforcing contracts and paying taxes range from 71 to 86 percent. In these areas, legislatures are most likely to stop a reform, and in labor regulation, they do so at a higher rate (24%) than for any other domain. Legislatures also stop 20% of reforms related to paying taxes, but in that domain the executive itself dooms more (25%). Legal reforms related to enforcing contracts succeed only 58 percent of the time. In this domain, courts reject more reforms (23 percent) than legislatures (11 percent).

Veto points do not stop all reforms, but they make success less likely, which potentially explains why reform has been more common for starting a business than for labor regulation. We next turn to cross country evidence and the difference between rich and poor countries.

V. Cross Country Differences

We take up three questions about reform across countries. We first ask why rich countries experience more change than poor ones holding initial levels of regulation constant. We then turn to convergence and ask what explains change for countries that begin with less market-friendly regulations. Finally, we ask whether the overall reform variation across countries is associated more with differences in the impact or in the capacity for reform.

National Income and Reform

Following the model, we think of the overall amount of change as reflecting the product of the probability of initiating a reform, the probability of the reform succeeding, and the impact of the reform on regulations. To this end, we ask whether income or initial regulation outcomes impact the probability of initiating reform, the probability of reform succeeding, and the impact of the reform. We interpret a combination of more attempted reforms, more reform success, and lower reform impact as being driven by more efficient compensation, i.e. a lower θ. We interpret a combination of fewer reform attempts, a lower rate of success, and a higher impact as being driven by higher returns from reform, i.e. a higher R. While R can differ across domains, we think of θ as specific to the country as a whole.

Table 6 asks whether the impact of reforms is different in rich countries. In all cases, we use the same event study structure as in Table 3. We also include log of GDP per capita (GDP) and interact it with the indicator that takes on a value of 1 for the two years after a reform.

For four of the six domains, reforms have less impact on regulations in richer countries. Across all domains, one extra log point of income is associated with a slightly less than 10 percent reduction in the average impact of reform on regulatory indices. There is an approximately ten to one ratio of the main coefficient on post-reform to the interaction with income for starting a business, paying taxes, enforcing contracts and protecting minority investors. The coefficients on the interactions are the highest for minority investors, but so are the main effects. We find no significant interactions for resolving insolvency and labor regulations, but no significant effect of post on those variables either.

Table 7 reports the relationship between income and the propensity to initiate reform. We repress the time dimension and use as the dependent variable the number of attempted

reforms, for each domain, for each type of initiator (executive, legislature and judiciary), for each country, over the entire sample period. We do not report coefficients for some extremely sparse cells in Table 7 and 8. Specifically, we exclude judicial reforms related to paying taxes or labor regulation and technological reforms in resolving insolvency or labor regulation. For GDP per capita, we use the average for each country over the sample period.

In Table 7, the executive branch is more likely to initiate reforms. Overall, legal reforms are the most common, which we interpret as reflecting the budget set of feasible reforms. If the executive could do more technological reforms, it presumably would, since these are easier to pass. In the starting a business domain, such reforms are more common.

Table 7 shows that higher log GDP per capita is associated with more reform initiations in all domain. The relationship is strongest in starting a business, labor regulation and minority investor protection. The effect of reforms in richer countries is smaller but they start more reforms. This finding is inconsistent with the view that R (returns to reform) is higher in rich countries but may be explained by their lower θ (the waste from Coasean compensation).

Table 8 examines the probability that a reform succeeds conditional on initiation. Each observation is an initiation attempt. The first two rows correlated the initiating branch of government with the success rate. This is a useful check, but since the decision to initiate reforms is endogenous, we cannot separate selection from treatment. Technological reforms have a higher probability of success in every specification, except starting a business and enforcing contracts. Administrative reforms have a lower probability of success in all domains. These reforms mostly fail because of executive opposition, but also because of the judiciary.

The impact of income on success rates is strong and positive in every regression. Rich countries start and implement more reforms but they are less impactful. They are better turning reform attempts into reforms, as in Proposition 3. They are more Coasean.

To understand better the differences between rich and poor countries, Tables 9a and 9b reproduce parts of Table 4 and 5 for rich and poor countries separately. In these tables, we split the sample of countries in half, based on the initial income levels. We do not show results by domain. Table 9a looks at the identity of who initiates and who stops reforms. Table 9b looks at the type of reform and how it is stopped.

In Table 9a, the overall reform success rate is 84 percent in rich countries and 51 percent in poor ones. As rich countries also initiate more reforms, this leads to a large disparity in total numbers of reforms between rich and poor countries. In rich countries, the legislature dooms the most reforms. In poor ones, the executive branch vetoes most often. Sixteen percent of reforms initiated by the executive branch are also stopped by the executive branch in the poorer half of countries. The comparable number in rich countries is 3 percent. This points to a massive divergence in the ability to close deals even within the executive branch of government.

In Table 9b, we look at the different types of reform. In rich countries, 95 percent of technological reforms and 79 percent of administrative ones succeed. The success rates for these types of reform in poor countries are 56 and 43 percent respectively. Moreover, technological and administrative reforms fail mostly within the executive branch, especially in poor countries. Legal reforms are also less successful in poorer countries, although the gap is smaller, largely because legislatures often stop reforms in both rich and poor countries.

Table 10 repeats Table 7, but looks at the rate of successful reforms, rather than reform initiation, by country, initiator and domain. The executive branch successfully passes more reforms in most of the domains and overall. There are more successful attempts in the technological and administrative categories within the starting a business and enforcing contracts domain, but not in the other domains and not overall. GDP is strongly positively associated with successful reforms in every category and overall.

In sum, reforms are much more likely to succeed in rich countries, which also attempt more reforms. The model links these two phenomena. Proposition 3 predicts that countries that are better at compensating losers from reforms would see both more reform initiations and more successes. The typical reform could have a lower impact, as is also the case in richer countries. Overall, rich countries are less likely than poor ones to get trapped, contrary to Olson's view that sclerosis accompanies prosperity.

Understanding Convergence

We next explore the pattern of convergence documented in Section 2, using the same approach. Table 11 looks at the estimated impact of reforms on outcomes using the same event study structure as in Tables 3 and 6. We interact the event with initial level of regulation, rather than with income. These interactions are not causal but describe whether the returns to reform ("R") are higher in countries that begin with less market-friendly regulations.

In all regressions, the coefficients are significant and negative. The impacts of reforms are uniformly higher in countries that started with less business-friendly regulations, and the magnitudes are especially large in regressions (1), (2), (5) and (6). Regression (1) shows that a reform event is associated with a .19 standard deviation higher change in the starting a business

index if the country's 2005 starting a business measure is one standard deviation lower. Regression (2) shows that reform is associated with a .25 standard deviation larger change if the paying taxes measures is one standard deviation lower in 2005. In regression (5) we find that reforms generate a .17 standard deviation larger increase in the protecting minority investors index in countries that begin with a one standard deviation lower value of that index. For labor regulation, a one standard deviation lower initial index value corresponds to a .11 standard deviation larger change overall.

In regressions (3) and (4), we see much smaller interactions for the other categories. Countries that begin with a one standard deviation lower value of the enforcing contracts index experience a .08 larger increase in that index after a reform. When the initial value of the resolving insolvency measure is one standard deviation lower, then reforms are associated with a .07 standard deviation higher increase. Regression (7) shows that across all categories, a one standard deviation lower initial index value is associated with a .12 standard deviation higher impact of reform.

The patterns in Table 11 suggest two results. First, the effects of reform are higher, and sometimes much higher, in countries with lower initial index values. We interpret this as suggesting that "R" is higher in countries with less market-friendly regulations initially. Second, the interaction with initial levels is higher in domains where there has been significant aggregate change: starting a business, paying taxes, and protecting minority shareholders. Aggregate change is easier when individual reforms have big effects in initially market-hostile countries.

Table 12 shows a low correlation between the initial level of regulations and the probability of starting a reform. The coefficients on the initial level of regulations are

statistically distinct from zero in regressions (1), (5), and (7), and weakly significant in (3). The other coefficients are statistically insignificant and small. The coefficients in (1), (3), (5) and (7) are positive: countries with more market-friendly regulations are more likely to reform.

Table 13 examines the effect on the probability of success of the initial index value. In six of the seven specifications, the coefficient on the initial index value is statistically significant and positive. Countries with more pro-business regulations are better at getting reforms through. The one exception is labor regulation. These findings suggest that countries with initially low levels of our regulatory indices have higher returns to reform ("R") but also higher costs of compensating the losers from reform ("θ").

Table 14a looks at the initiation and success rates of reforms in countries with high and low index values by initiator. Whether the initiator is in the executive, legislature or judiciary, the success rates are higher in countries with a higher initial index value. The executive is far more likely to stop reforms in countries with initially lower index values, even reforms initiated by itself. In high index countries, only 5 percent of the reforms initiated by the executive get stopped by the executive. In low index countries, 12 percent of executive-initiated reforms are stopped by the executive. The executive branch stops a remarkable 38 percent of the reforms initiated in the legislature in countries with an initially low index.

Table 14b splits the reforms by type. Here again the differences between high and low initial index countries is clear. An astonishing 92 percent of technological reforms proposed in high index countries succeed. That rate drops to 74 percent in low index countries, with 16 percent of technological reforms in low index countries blocked by the executive branch.

Table 15 looks at the overall number of successful attempts. In this case, the initial index level is positively related to the number of successful attempts except for labor regulation. A one unit increase in the initial index is associated with .14 more successful reforms for starting a business, .11 more successful reforms related to minority investor rights and .99 more reforms overall. The initial index is more predictive of the overall reform rate than the different sub-indices in Table 15 or of the propensity to initiate reforms in Table 12 since averaging across categories reduces noise in the independent variable.

The evidence suggests that variations in R and in θ often go together. Richer countries have lower values of both R and θ, which means that they are good at passing reforms, but their reforms have a lower impact on our indices. Countries with low initial index values have higher values of R and θ, which means that reforms are rarer but more impactful. We next turn to the broad question of whether R or θ explains more about the patterns of reform across countries.

Are Reform Attempt Rates Driven by the Returns to Reform or the Ability to Reform

We last examine whether the frequency of reform attempts is driven more by the heterogeneity in returns to reform (R) or the heterogeneity in the capacity to reform (θ). The logic of proposition 3a is that if the frequency of reforms is driven primarily by differences in R, then countries that initiate reform more often should have lower success rates, but higher returns from reforming. Proposition 3b implies instead that if heterogeneity in θ drives the frequency of reforms, then countries that attempt more reform have a higher reform success rate and possibly a lower return to reform. These predictions are analogous to looking at prices to see whether a quantity increase is driven by robust demand or easy supply. The returns to reform, R, are akin to demand for reform, while the capacity to reform, θ, is akin to the supply.

Figures 12 and 13 show the relationship between three variables: reform attempt rates, reform success rates and the index changes after reform. Attempt rates and success rates are essentially raw data. For each country, we calculate the average number of reforms per year to form attempt rates and the average probability those reforms succeed to form the success rate. The average reform effect is defined as the difference between the average regulatory index in the three years after the reform and that in the three years before.

Figure 13 shows the striking positive relationship between reform attempt and success rates. As the number of attempts per year rises by 1, the average success rate rises by around 26 percent. Countries with success rates below 60 percent initiate fewer than 2 reforms per year. This pattern is not causal but consistent with the view likely success rates shape attempt rates.

Figure 14 shows the relationship between the regulatory effects of reform and the propensity to initiate them. We define the effect of reform as the difference between the average regulatory index in the three years after the reform and the three years before. The estimated relationship is slightly negative, but statistically insignificant. Some of the countries with the lowest numbers of reform attempts have extremely high reform effects, but this might be because reform effects are measured with more error when reform attempts are fewer.[6]

To test the relationship between attempt frequency and reform effects, we estimate an event-study in which we interact our indicator for post-reform with the average frequency of reform in the country as a whole. Including country and year fixed effects, we estimate:

[6] The same non-relationship appears if we take regression coefficients and apply Bayesian shrinkage to the average estimated regression coefficient.

$$(1)\ Outcome = \underset{(.027)}{.247} * Post - \underset{(.014)}{.074} * Post * Frequency + Dummy\ Variables$$

The regression has 4546 observations and the R-squared is .437. This regression again shows that countries with more reform attempts do not have more consequential reforms.

In a Coasean world, the frequency of reform attempts would be driven by the benefits of reform. We see little evidence for that perspective. Instead, countries that are good at implementing reform are more likely to attempt reform. Olson proposed that reform is particularly difficult in rich countries. The opposite is true. But his emphasis on the importance of the political process and its role in blocking reform finds solid support in the data.

VI. Conclusion

Economists support policies that they believe do the most good if implemented. Significant tools of our profession, including cost-benefit analysis and structural models that simulate policy counterfactuals, estimate how much a change would help or harm population subgroups. Typical policy discussions are free of questions such as whether this change can be implemented administratively or whether legislation is needed.

A central point of our paper is that it is a mistake to focus on the returns to reform at the expense of the feasibility of reform. Across types of reform, technological reforms have higher success rates than administrative and legislative ones. Reforms initiated by the executive but require approval by another branch of government are less likely to succeed. Veto points matter and it is far easier to enact reforms that face fewer entities who can say no.

We found substantially larger changes in the market-friendly indices for countries that started with lower levels of these indices. This finding is driven by the higher returns to reform in these countries, not by a higher probability of initiating or passing reforms. But the positive relationship between national income and improved regulations obtains despite reforms having less impact in richer countries. Richer countries are just better at reform.

Finally, the prevalence of reform around the world seems much more correlated with the ability to achieve reform than with the returns to reform. Economists have spent decades investigating the impact of policy changes. We should spend more time understanding what determines whether and how these changes occur at all.

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Table 1: Definitions

Variable	Definition
Panel A. Reform Characteristics	
Attempt	Reform attempt refers to a proposed regulatory change initiated by an official branch of government, a ministry, or an executive agency. Public pronouncements or campaign promises that are not formally announced through official channels are excluded. A reform enters the dataset when it is first officially announced or published in a governmental schedule.
Success	Successful reform refers to a proposed reform that is adopted, either through new regulation or new technology or organizational restructuring.
Failure	Failed reform refers to a proposed reform that was not adopted by 2022.
Initiator	Initiator refers to the branch of government—Executive, Legislature, or Judiciary—that formally proposed the reform.
Stopper	Stopper refers to the branch of government—Executive, Legislature, or Judiciary—that prevented the reform from being adopted.
Type	Reform type is categorized into three groups: technological, administrative, and legal reforms. Technological reforms refer to changes that do not involve the legislature and are limited to updates in the technology used to implement existing regulations, without any accompanying policy, organizational, or structural changes. Administrative reforms are implemented by executive decree, a minister, or an agency, and do not require legislative approval. Legal reforms involve changes to laws that must go through the legislature.
Panel B. Outcome Indicators[7]	
Starting a Business	
Procedure	Procedure is defined as any interaction of the company founders with external parties (for example, government agencies, lawyers, auditors or notaries) or spouses (if legally required).
Time (days)	Time is recorded in calendar days. The measure captures the median duration that incorporation lawyers or notaries indicate is necessary in practice to complete a procedure with minimum follow-up with government agencies and no unofficial payments.
Cost (% of income per capita)	Cost is recorded as a percentage of the economy's income per capita. It includes all official fees and fees for legal or professional services if such services are required by law or commonly used in practice.
Paying Taxes	
Tax payments per year	Tax payments indicator reflects the total number of taxes and contributions paid, the method of payment, the frequency of payment, the frequency of filing and the number of agencies involved for the standardized case study company during the second year of operation.
Time (hours per year)	Time is recorded in hours per year. The indicator measures the time taken to prepare, file and pay three major types of taxes and

[7] Source: The World Bank's Doing Business database (URL: https://archive.doingbusiness.org/en/data) and extended through desk research at LSE. The coverage is 189 countries over the period 2005-2022.

	contributions: the corporate income tax, value added or sales tax, and labor taxes, including payroll taxes and social contributions.
Total tax and contribution rate	Total tax and contribution rate measures the amount of taxes and mandatory contributions borne by the business in the second year of operation, expressed as a share of commercial profit.
Enforcing Contracts	
Time (days)	Time is recorded in calendar days, counted from the moment seller decides to file the lawsuit in court until payment. This includes both the days when actions take place and the waiting periods in between.
Cost (% of contract)	Cost is recorded as a percentage of the claim value. Three types of costs are recorded: average attorney fees, court costs and enforce-ment costs.
Resolving Insolvency	
Time (years)	Time for creditors to recover their credit is recorded in calendar years. The period of time measured is from the company's default until the payment of some or all of the money owed to the bank.
Cost (% of estate)	Cost of the proceedings is recorded as a percentage of the value of the debtor's estate. The cost is calculated on the basis of questionnaire responses and includes court fees and government levies; fees of insolvency administrators, auctioneers, assessors and lawyers; and all other fees and costs.
Minority Investors	
Extent of disclosure index	Extent of disclosure index has five components: assuming that Mr. James proposes a transaction in which he owns 60% of the buyer and 90% of the seller, and the transaction harms the buyer, (i) whether corporate body can provide legally sufficient approval for the transaction; (ii) whether an external body (an inde-pendent auditor, for example) must review the transaction before it takes place; (iii) whether disclosure by Mr. James to the board of directors or the supervi-sory board is required; (iv) whether immediate disclosure of the transaction to the public, the regulator or the shareholders is required; and (v) whether disclosure in periodic filings (for example, annual reports) is required. The index ranges from 0 to 10, with higher values indicating greater disclosure.
Extent of director liability index	Extent of director liability index has seven components: assuming that Mr. James proposes a transaction in which he owns 60% of the buyer and 90% of the seller, and the transaction harms the buyer, (i) whether shareholders can sue directly or derivatively for the damage the transaction causes to the company; (ii) whether a shareholder plaintiff can hold Mr. James liable for the damage the Buyer-Seller transaction causes to the company; (iii) whether a shareholder plaintiff can hold other executives and directors liable for the damage the transaction causes to the company; (iv) whether Mr. James pays damages for the harm caused to the company upon a successful claim by the shareholder plaintiff; (v) whether Mr. James repays profits made from the transaction upon a successful claim by the shareholder plaintiff; (vi) whether Mr. James is disqualified upon a successful claim by the shareholder plaintiff; and (vii) whether a court can void the trans-action upon a successful claim by a shareholder plaintiff. The index ranges from 0 to 10, with higher values indicating greater liability of directors.
Labor Regulation	
Difficulty of hiring index	Difficulty of hiring index measures five components: (i) whether fixed-term contracts are prohibited for permanent tasks; (ii) the maximum

	cumulative duration of fixed-term contracts; (iii) the length of the maximum probationary period (in months) for permanent employees; (iv) the minimum wage for a cashier, age 19, with one year of work experience; and (v) the ratio of the minimum wage to the average value added per worker.
Rigidity of hours index	Rigidity of hours index measures eight components: (i) the maximum number of working days allowed per week; (ii) the premium for night work (as a percentage of hourly pay); (iii) the premium for work on a weekly rest day (as a percentage of hourly pay); (iv) the premium for overtime work (as a percentage of hourly pay); (v) whether there are restrictions on night work; (vi) whether there are restrictions on weekly holiday work; (vii) whether there are restrictions on overtime work; and (viii) the average paid annual leave for workers with one year of tenure, five years of tenure and 10 years of tenure.
Difficulty of redundancy index	Difficulty of redundancy index measures eight components: (i) whether redundancy is allowed as a basis for terminating workers; (ii) whether the employer needs to notify a third party (such as a government agency) to terminate one redundant worker; (iii) whether the employer needs to notify a third party to terminate a group of nine redundant workers; (iv) whether the employer needs approval from a third party to terminate one redundant worker; (v) whether the employer needs approval from a third party to terminate a group of nine redundant workers; (vi) whether the law requires the employer to reassign or retrain a worker before making the worker redundant; (vii) whether priority rules apply for redundancies; and (viii) whether priority rules apply for reemployment.
Panel C. Development Indicators	
GDP per capita	GDP per capita is gross domestic product divided by midyear population. GDP is the sum of gross value added by all resident producers in the economy plus any product taxes and minus any subsidies not included in the value of the products. It is calculated without making deductions for depreciation of fabricated assets or for depletion and degradation of natural resources. Data are in current U.S. dollars. The primary data source is the World Bank's World Development Indicators; where unavailable, values are supplemented using the International Monetary Fund's World Economic Outlook and United Nations' UNdata.

Figure 1: Trends in Starting a Business Regulatory Outcomes by Income Group

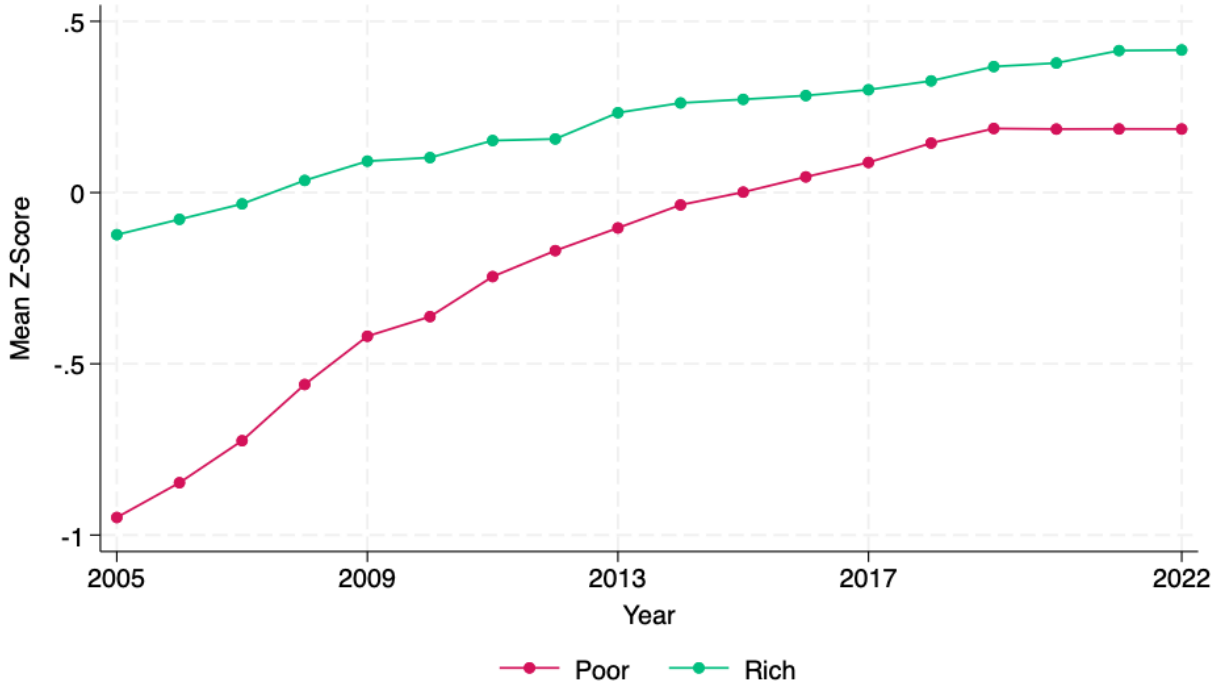


Figure notes: This figure shows average standardized regulatory outcomes in the "Starting a Business" domain by income group over time. The y-axis reports the mean z-score of three component indicators: the number of procedures, time (in days), and cost (as a percentage of income per capita) required to start a business. Each outcome is standardized across all countries and years, with higher values indicating more market friendly doing-business environments. The lines represent average scores for countries classified as Rich (green) and Poor (red). The sample includes countries with non-missing index data over 18 years, covering 173 countries from 2005 to 2022. Outcome data are sourced from the World Bank and extended through desk research at the London School of Economics. Countries are classified as rich if their GDP per capita in 2005 is above or equal to the median, and poor if it is below the median.

Figure 2: Trends in Paying Taxes Regulatory Outcomes by Income Group

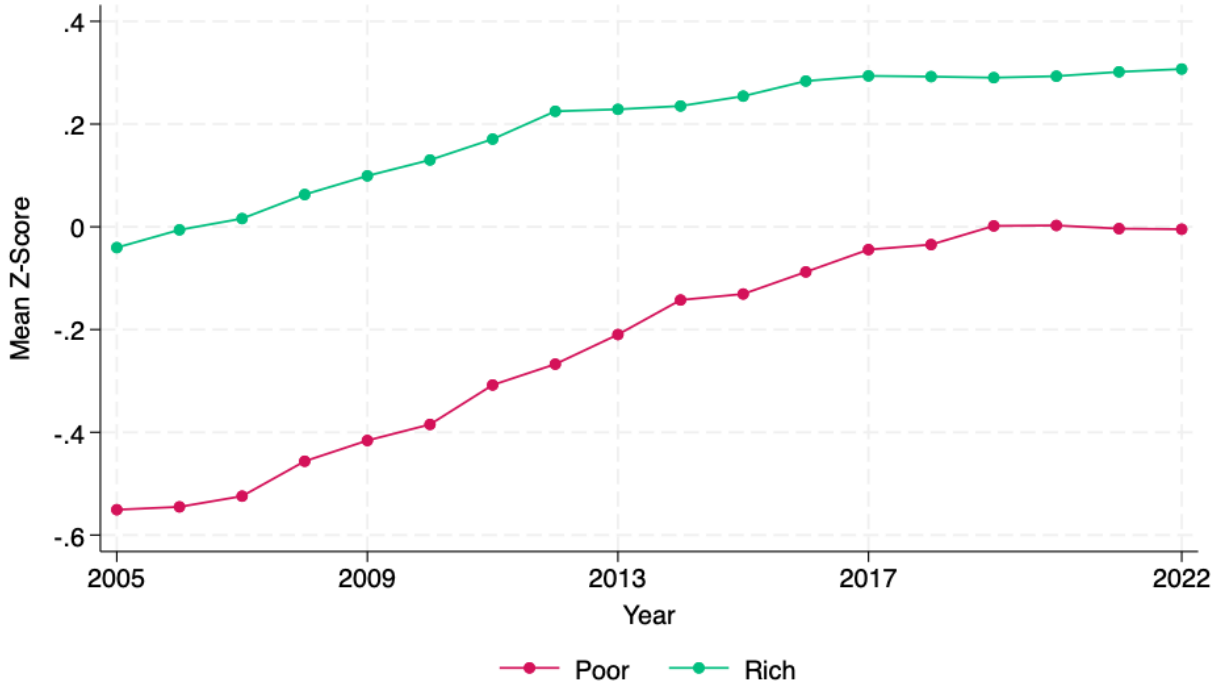


Figure notes: This figure shows average standardized regulatory outcomes in the "Paying Taxes" domain by income group over time. The y-axis reports the mean z-score of three component indicators: the number of tax payments per year, time required to prepare, file and pay taxes (hours per year), and total tax and contribution rate (as a percentage of profit). Each outcome is standardized across all countries and years, with higher values indicating more market friendly doing-business environments. The lines represent average scores for countries classified as Rich (green) and Poor (red). The sample includes countries with non-missing index data over 18 years, covering 173 countries from 2005 to 2022. Outcome data are sourced from the World Bank and extended through desk research at the London School of Economics. Countries are classified as rich if their GDP per capita in 2005 is above or equal to the median, and poor if it is below the median.

Figure 3: Trends in Minority Investors' Protection by Income Group

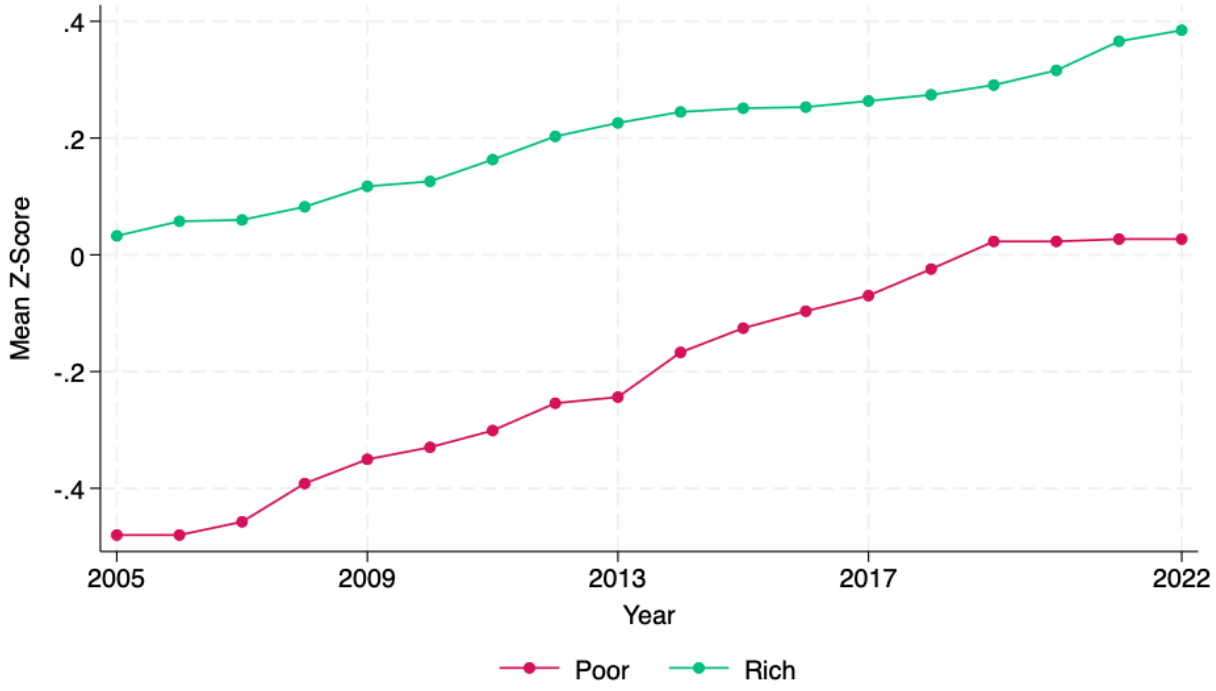


Figure notes: This figure shows average standardized outcomes in the "Minority Investors" domain by income group over time. The y-axis reports the mean z-score of two component indicators: Extent of disclosure index (0-10) and Extent of director liability index (0-10), which measure the strength of minority shareholder protections. Each outcome is standardized across all countries and years, with higher values indicating more market friendly environments. The lines represent average scores for countries classified as Rich (green) and Poor (red). The sample includes countries with non-missing index data over 18 years, covering 189 countries from 2005 to 2022. Outcome data are sourced from the World Bank and extended through desk research at the London School of Economics. Countries are classified as rich if their GDP per capita in 2005 is above or equal to the median, and poor if it is below the median.

Figure 4: Trends in Resolving Insolvency Regulatory Outcomes by Income Group

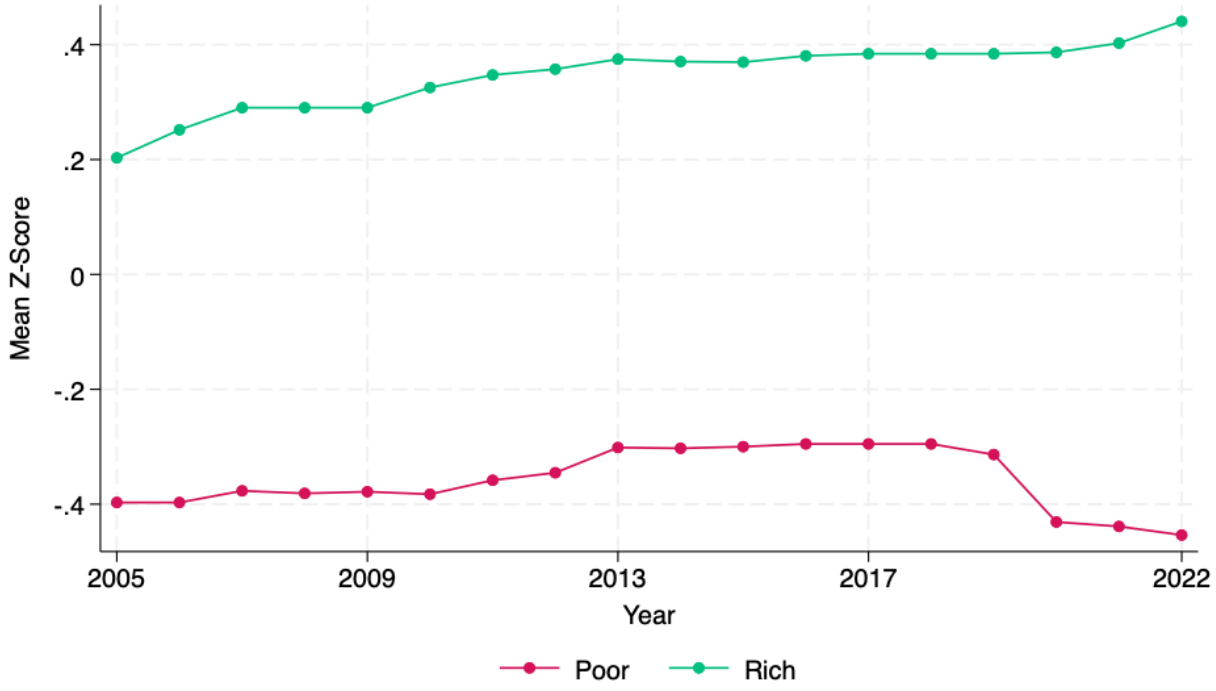


Figure notes: This figure shows average standardized outcomes in the "Resolving Insolvency" domain by income group over time. The y-axis reports the mean z-score of two component indicators: the time required to resolve insolvency (in years) and the associated cost (as a percentage of the estate value). Each outcome is standardized across all countries and years, with higher values indicating more market friendly environments. The lines represent average scores for countries classified as Rich (green) and Poor (red). The sample includes countries with non-missing index data over 18 years, covering 152 countries from 2005 to 2022. Outcome data are sourced from the World Bank and extended through desk research at the London School of Economics. Countries are classified as rich if their GDP per capita in 2005 is above or equal to the median, and poor if it is below the median.

Figure 5: Trends in Enforcing Contracts Regulatory Outcomes by Income Group

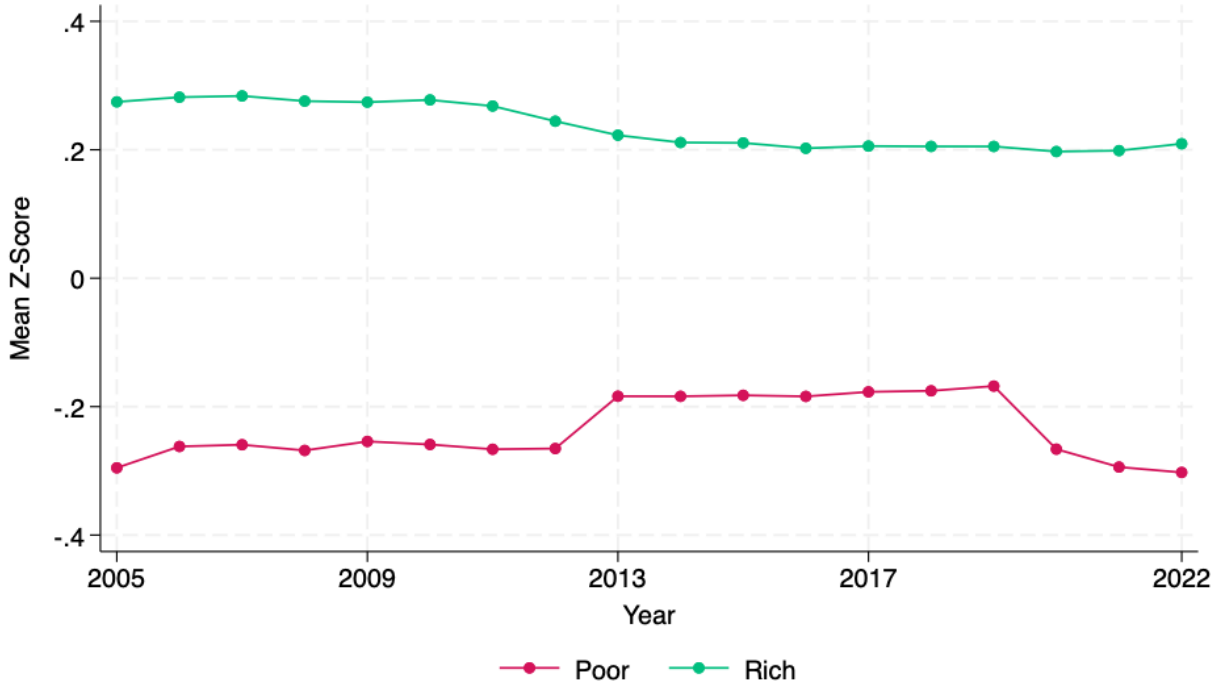


Figure notes: This figure shows average standardized outcomes in the "Enforcing Contracts" domain by income group over time. The y-axis reports the mean z-score of two component indicators: the time required to resolve a commercial dispute (in days) and the cost (as a percentage of the claim value). Each outcome is standardized across all countries and years, with higher values indicating more market friendly environments. The lines represent average scores for countries classified as Rich (green) and Poor (red). The sample includes countries with non-missing index data over 18 years, covering 173 countries from 2005 to 2022. Outcome data are sourced from the World Bank and extended through desk research at the London School of Economics. Countries are classified as rich if their GDP per capita in 2005 is above or equal to the median, and poor if it is below the median.

Figure 6: Trends in Labor Regulation Regulatory Outcomes by Income Group

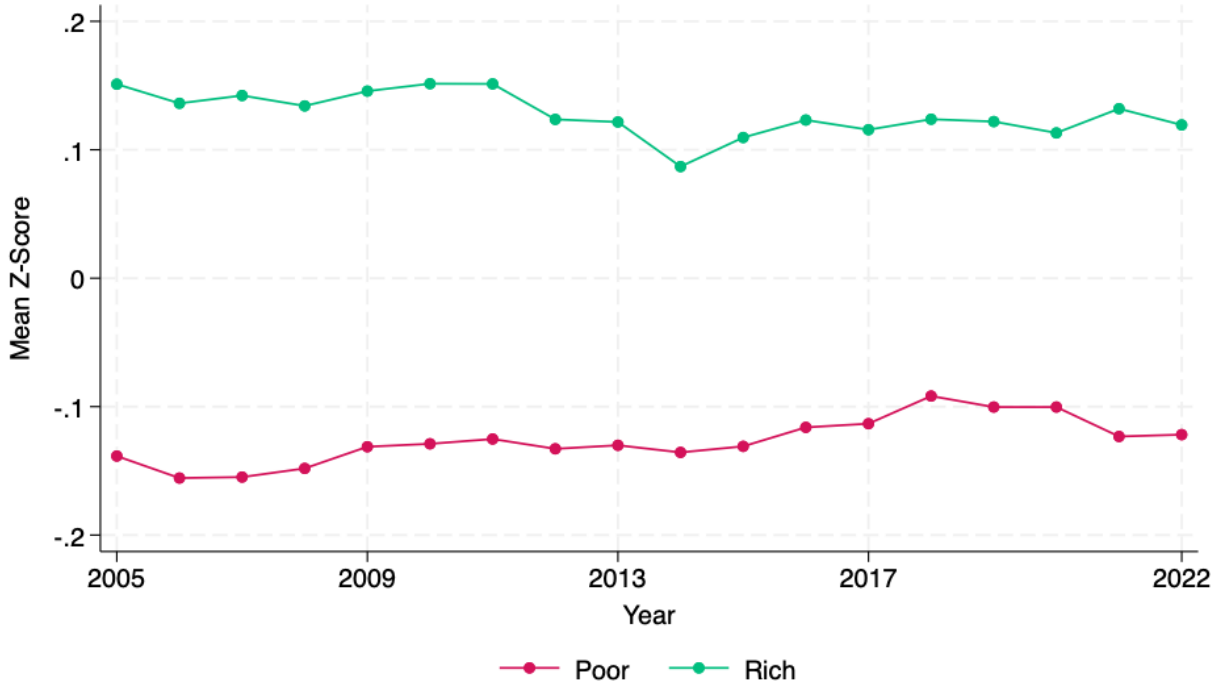
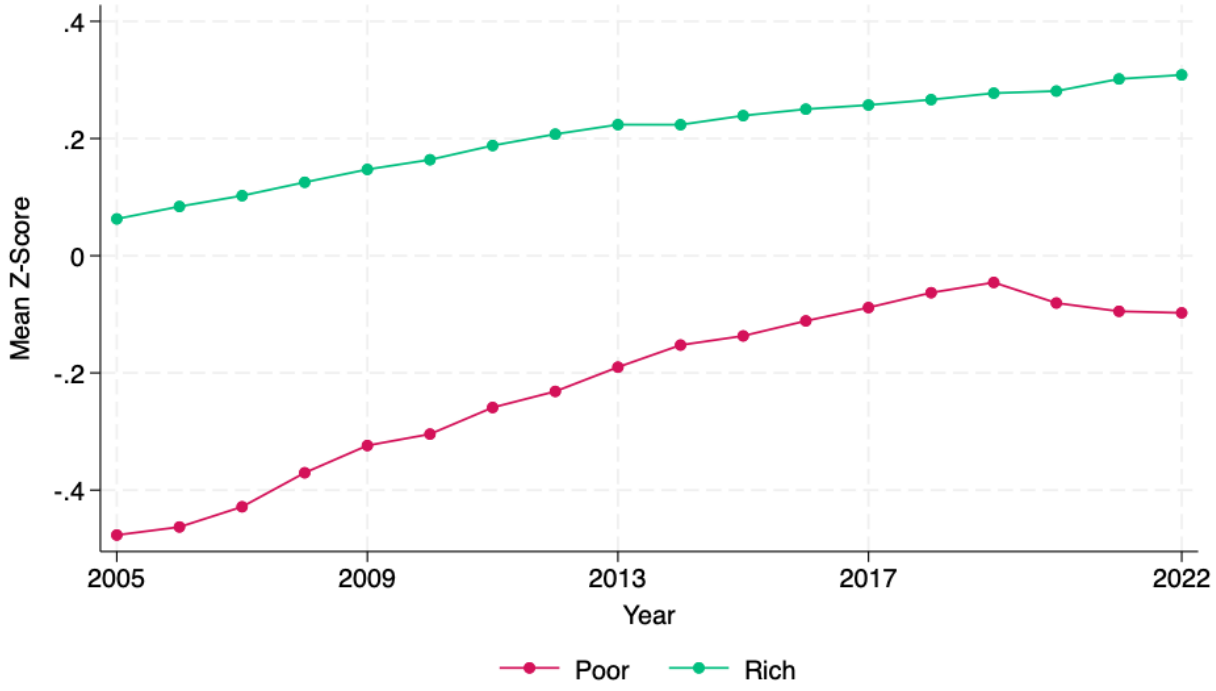


Figure notes: This figure shows average standardized outcomes in the "Labor Regulation" domain by income group over time. The y-axis reports the mean z-score of three component indicators: the Difficulty of Hiring Index, the Rigidity of Hours Index, and the Difficulty of Redundancy Index, which measure the flexibility of labor market regulations. Each outcome is standardized across all countries and years, with higher values indicating more market friendly environments. The lines represent average scores for countries classified as Rich (green) and Poor (red). The sample includes countries with non-missing index data over 18 years, covering 189 countries from 2005 to 2022. Outcome data are sourced from the World Bank and extended through desk research at the London School of Economics. Countries are classified as rich if their GDP per capita in 2005 is above or equal to the median, and poor if it is below the median.

Figure 7: Trends in Regulatory Outcomes Across All Domains by Income Group



This figure shows average standardized outcomes across six domains by income group over time. The y-axis reports the mean z-score of fifteen component indicators spanning six domains. Each outcome is standardized across all countries and years, with higher values indicating more market friendly environments. The lines represent average scores for countries classified as Rich (green) and Poor (red). The sample includes countries with non-missing index data over 18 years, covering 189 countries from 2005 to 2022. Outcome data are sourced from the World Bank and extended through desk research at the London School of Economics. Countries are classified as rich if their GDP per capita in 2005 is above or equal to the median, and poor if it is below the median.

Table 2: Change in Regulation on Initial Income and Initial Regulation Levels

Starting a Business

	(1) Procedure	(2) Time (days)	(3) Cost (% of income per capita)
Log of GDP per capita (2000)	.046	.053*	.052**
	(.044)	(.029)	(.021)
Initial "Procedure"	-.6***		
	(.067)		
Initial "Time (days)"		-.851***	
		(.031)	
Initial "Cost (% of income per capita)"			-.951***
			(.017)
_cons	.332	-.106	-.136
	(.356)	(.227)	(.169)
Observations	173	173	173
R-squared	.342	.815	.956

Paying Taxes

	(4) Tax payments per year	(5) Time (hours per year)	(6) Total tax and contribution rate
Log of GDP per capita (2000)	.205***	.059**	-.007
	(.044)	(.027)	(.032)
Initial "Tax payments per year"	-.867***		
	(.058)		
Initial "Time (hours per year)"		-.601***	
		(.033)	
Initial "Total tax and contribution rate"			-.8***
			(.036)
_cons	-1.269***	-.241	.206
	(.348)	(.215)	(.25)
Observations	173	173	173
R-squared	.574	.667	.76

Enforcing Contracts

	(7) Time (days)	(8) Cost (% of contract)
Log of GDP per capita (2000)	.055**	.054*
	(.028)	(.028)
Initial "Time (days)"	-.171***	
	(.041)	
Initial "Cost (% of contract)"		-.41***
		(.036)
_cons	-.544**	-.383*
	(.215)	(.218)
Observations	173	173
R-squared	.1	.447

Resolving Insolvency

	(9) Time (years)	(10) Cost (% of estate)
Log of GDP per capita (2000)	.254***	.096***
	(.044)	(.022)
Initial "Time (years)"	-.617***	
	(.056)	
Initial "Cost (% of estate)"		-.16***
		(.032)
_cons	-1.876***	-.737***
	(.35)	(.168)
Observations	152	152
R-squared	.459	.175

Minority Investors

	(11) Extent of disclosure index	(12) Extent of director liability index
Log of GDP per capita (2000)	-.015	.001
	(.036)	(.032)
Initial "Extent of disclosure index"	-.345***	
	(.058)	
Initial "Extent of director liability index"		-.197***
		(.051)
_cons	.58**	.258
	(.289)	(.253)
Observations	189	189
R-squared	.17	.087

Labor Regulation

	(13) Difficulty of hiring index	(14) Rigidity of hours index	(15) Difficulty of redundancy index
Log of GDP per capita (2000)	.043	-.037	.015
	(.029)	(.031)	(.028)
Initial "Difficulty of hiring index"	-.238***		
	(.046)		
Initial "Rigidity of hours index"		-.252***	
		(.048)	
Initial "Difficulty of redundancy index"			-.188***
			(.044)
_cons	-.308	.22	-.088
	(.23)	(.243)	(.217)
Observations	189	189	189
R-squared	.129	.135	.091

Table notes: This table presents ordinary least squares (OLS) regressions. Each observation in this regression corresponds to a country. The dependent variable is the change in outcomes between 2005 and 2022. Each outcome is standardized across all countries and years, where a higher value reflects a more market friendly environment. Regressors include the log of GDP per capita in 2000 (for South Sudan, where 2000 data are unavailable, the 2008 value is used) and the initial level of the corresponding outcome in 2005. The table is organized into six panels, each corresponding to a reform domain (e.g., Starting a Business, Paying Taxes). Each panel includes two to three outcome variables specific to that domain. The number of observations reflects the set of countries with non-missing data for the outcome change, initial income, and initial outcome. Standard errors are shown in brackets under each coefficient. *p<.1; **p<.05; ***p<.01.

Figure 8: Size of Loss, Share of Losers and Cost of Compensation

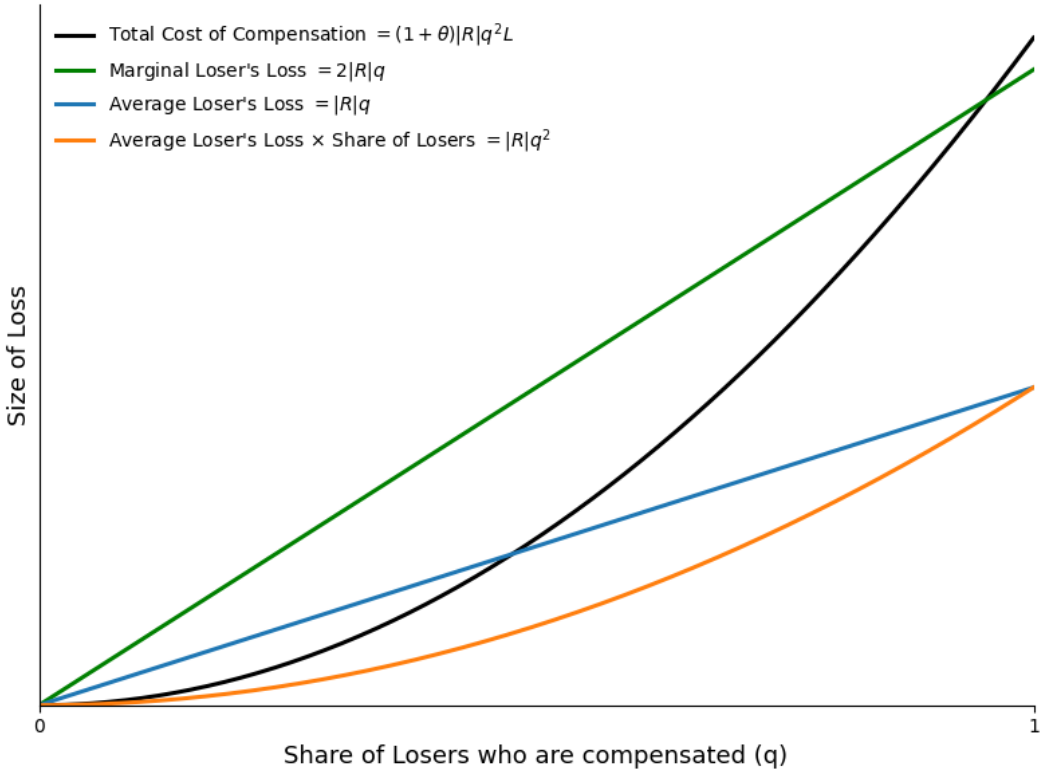


Figure 9: Reform Possibilities Frontier and Indifference Curve

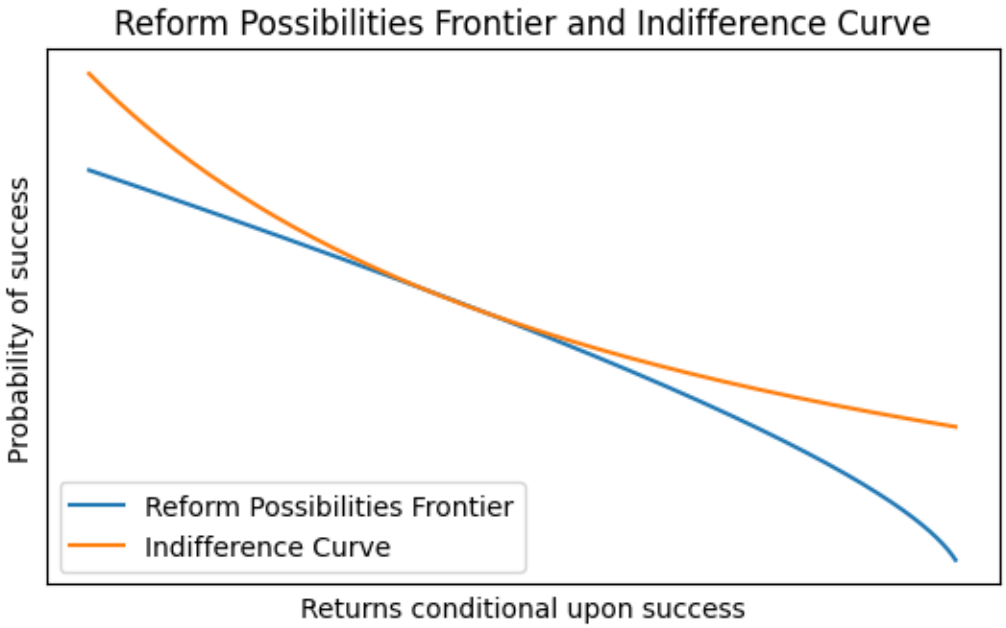


Figure 10: Shifts in the Reform Possibilities Frontier

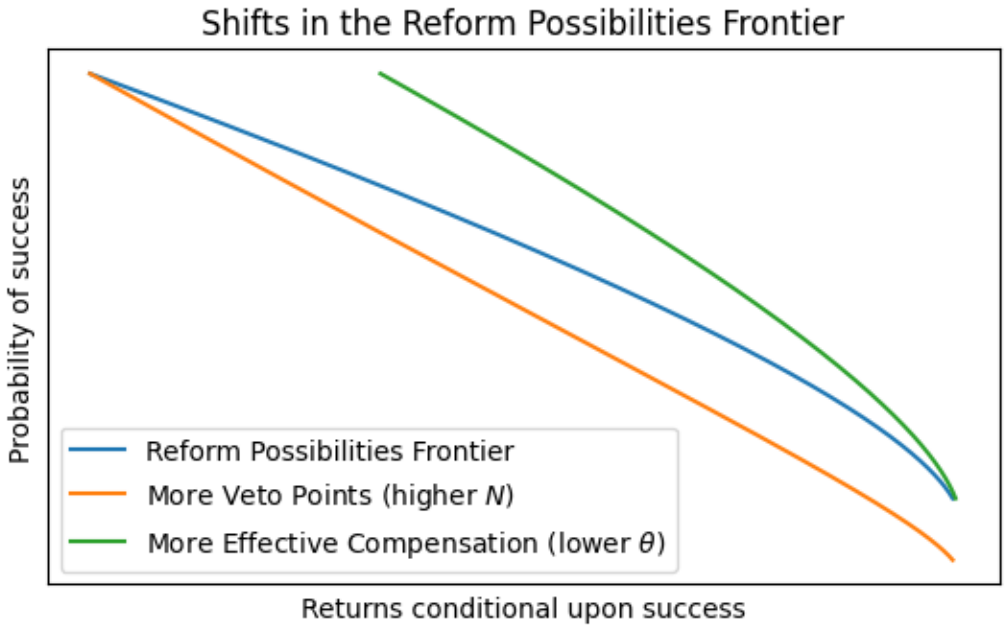


Figure 11: Impact of Returns to Reform on the Amount of Reform and Probability of Success

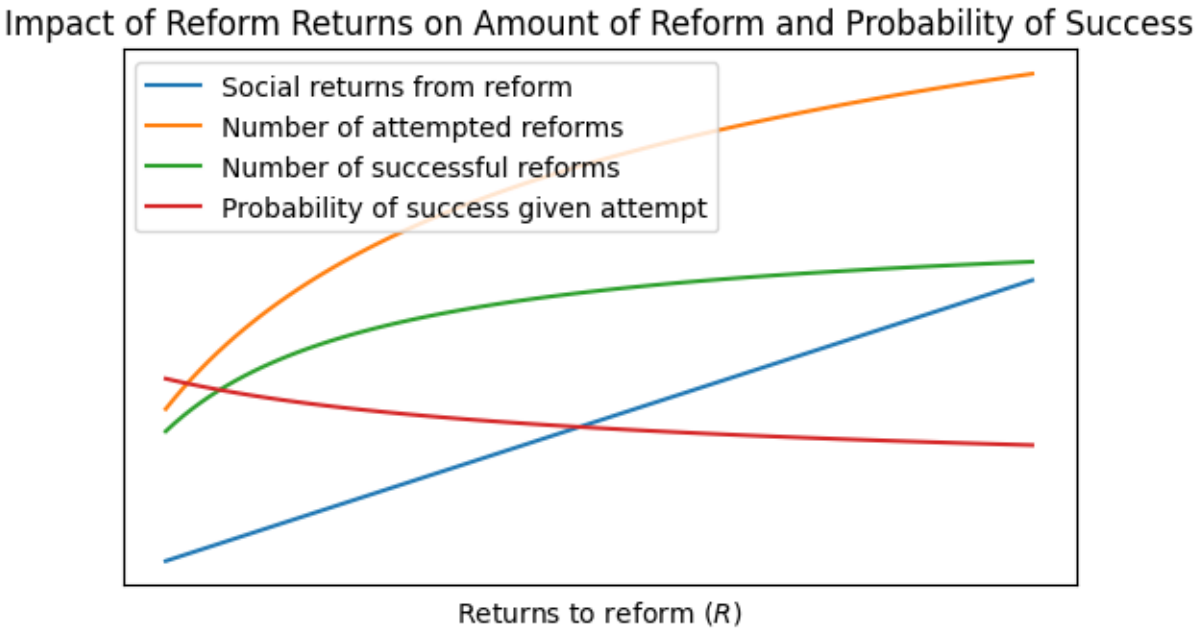


Figure 12: Impact of Reform Efficiency on the Amount of Reform and Probability of Success

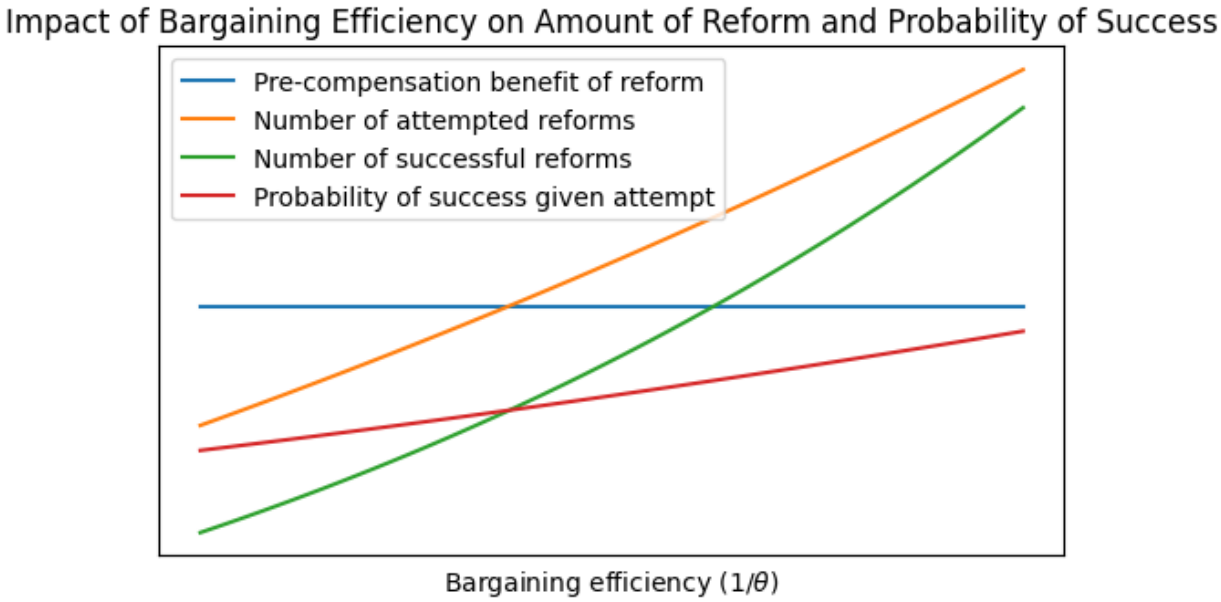


Table 3: Event Study

	(1) Starting a Business	(2) Paying Taxes	(3) Enforcing Contracts	(4) Resolving Insolvency	(5) Minority Investors	(6) Labor Regulation	(7) All Domains
Post	.202***	.18***	.022*	.056**	.213***	.025	.131***
	(.019)	(.027)	(.011)	(.023)	(.031)	(.021)	(.01)
_cons	-.077	-.033	.151*	.231***	.053	.009	.049
	(.053)	(.043)	(.081)	(.032)	(.171)	(.067)	(.05)
Observations	1270	892	644	532	560	660	4558
R^2	.771	.812	.953	.932	.884	.933	.437
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
% increase	93%	85%	34%	24%	48%	22%	59%
% same	1%	1%	45%	67%	52%	60%	30%
% decrease	6%	14%	21%	10%	0%	18%	11%

Table notes: This table presents ordinary least squares (OLS) regressions.
Specifications. Each observation corresponds to a successful reform and a two-year period, either pre- or post-reform. For each reform implemented in country c in year t, we compute two-period averages of the relevant index: one for the pre-reform period (years t–2 and t–1) and one for the post-reform period (years t+1 and t+2). These averages are then regressed on an indicator variable equal to 1 for the post-reform period and 0 for the pre-reform period. All specifications control for country and year fixed effects.
Regulatory indices. All outcomes are first standardized across all countries and years (z-score), with higher values indicating more market friendly environments, and then averaged within each domain (Columns 1–6), or across all domains (Column 7). For example, the index for Starting a Business (Column 1) is calculated as the mean of the z-scores for the three indicators in that domain: number of procedures, time (days), and cost (as a percentage of income per capita). The table reports separately by reform domain in Columns 1–6 (e.g., Starting a Business, Paying Taxes), with Column 7 pooling all domains.
Observations. Each reform contributes two observations: one for the pre-reform period and one for the post-reform period. The number of observations reflects only successful reforms with non-missing index data in both periods. For example, 485 paying-taxes reforms would generate 970 observations. However, 37 reforms occur in 2005 or 2022, the first and last years of our index data (2005–2022), so for these reforms either the pre- or post-reform observation is mechanically missing. In addition, 2 reforms are dropped due to missing index data. Accounting for these cases explains the observed count of 892 observations for paying taxes.
Percentages. The last three rows report the share of reforms with higher, unchanged, or lower two-year post-reform averages relative to the pre-reform period.
Standard errors are clustered by country, shown in brackets under each coefficient. *p<.1; **p<.05; ***p<.01.

Table 4: Reforms by Initiator and Stopper

Initiation	Total	Total (Successful)	Total (Failed)	Stop		
				Executive	Judiciary	Legislature
Starting a Business						
Executive	665 (85%)	598 (90%)	67 (10%)	49 (7%)	7 (1%)	11 (2%)
Judicial	36 (5%)	31 (86%)	5 (14%)	3 (8%)	0 (0%)	2 (6%)
Legislative	86 (11%)	66 (77%)	20 (23%)	18 (21%)	0 (0%)	2 (2%)
Total	787	695 (88%)	92 (12%)	70 (9%)	7 (1%)	15 (2%)
Paying Taxes						
Executive	556 (66%)	378 (68%)	178 (32%)	127 (23%)	2 (0%)	49 (9%)
Judicial	0 (0%)	0 (0%)	0 (0%)	0 (0%)	0 (0%)	0 (0%)
Legislative	283 (34%)	107 (38%)	176 (62%)	139 (49%)	1 (0%)	36 (13%)
Total	839	485 (58%)	354 (42%)	266 (32%)	3 (0%)	85 (10%)
Enforcing Contracts						
Executive	255 (43%)	100 (39%)	155 (61%)	2 (1%)	139 (55%)	14 (5%)
Judicial	271 (46%)	228 (84%)	43 (16%)	12 (4%)	27 (10%)	4 (1%)
Legislative	64 (11%)	33 (52%)	31 (48%)	16 (25%)	14 (22%)	1 (2%)
Total	590	361 (61%)	229 (39%)	30 (5%)	180 (31%)	19 (3%)
Resolving Insolvency						
Executive	264 (68%)	214 (81%)	50 (19%)	7 (3%)	28 (11%)	15 (6%)
Judicial	28 (7%)	14 (50%)	14 (50%)	7 (25%)	0 (0%)	7 (25%)
Legislative	94 (24%)	83 (88%)	11 (12%)	6 (6%)	2 (2%)	3 (3%)
Total	386	311 (81%)	75 (19%)	20 (5%)	30 (8%)	25 (6%)
Minority Investors						
Executive	242 (51%)	153 (63%)	89 (37%)	10 (4%)	40 (17%)	39 (16%)
Judicial	80 (17%)	66 (83%)	14 (18%)	7 (9%)	0 (0%)	7 (9%)
Legislative	148 (31%)	100 (68%)	48 (32%)	7 (5%)	36 (24%)	5 (3%)
Total	470	319 (68%)	151 (32%)	24 (5%)	76 (16%)	51 (11%)
Labor Regulation						
Executive	386 (75%)	279 (72%)	107 (28%)	1 (0%)	1 (0%)	105 (27%)
Judicial	4 (1%)	2 (50%)	2 (50%)	0 (0%)	0 (0%)	2 (50%)
Legislative	128 (25%)	99 (77%)	29 (23%)	8 (6%)	3 (2%)	18 (14%)
Total	518	380 (73%)	138 (27%)	9 (2%)	4 (1%)	125 (24%)
All Reform Types						
Executive	2368 (66%)	1722 (73%)	646 (27%)	196 (8%)	217 (9%)	233 (10%)
Judicial	419 (12%)	341 (81%)	78 (19%)	29 (7%)	27 (6%)	22 (5%)
Legislative	803 (22%)	488 (61%)	315 (39%)	194 (24%)	56 (7%)	65 (8%)
Total	3590	2551 (71%)	1039 (29%)	419 (12%)	300 (8%)	320 (9%)

Table notes: This table presents the number of successful and failed reforms by initiator, i.e. Executive, Legislative, and Judicial. For failed reforms, we further identify the stopper, i.e. Executive, Legislative, and Judicial. Percentages are reported in parentheses. In the *Total* column, the percentage reflects the share of reforms initiated by each branch within the category (column percentage). In the *Total (Successful)* and *Total (Failed)* columns, the percentages represent the share of successful or failed reforms within that initiator (row percentage). In the three *Stop* columns, the percentages represent the share of failed reforms stopped by each branch within that initiator (row percentage), which together sum to the percentage in the *Total (Failed)* column. The top six panels present numbers for each domain separately, and the final panel pools all six domains together.

Table 5: Reforms by Type and Stopper

Initiation	Total	Total (Successful)	Total (Failed)	Stop		
				Executive	Judiciary	Legislature
Starting a Business						
Technological	326 (41%)	319 (98%)	7 (2%)	7 (2%)	0 (0%)	0 (0%)
Administrative	336 (43%)	268 (80%)	68 (20%)	60 (18%)	6 (2%)	2 (1%)
Legal	125 (16%)	108 (86%)	17 (14%)	3 (2%)	1 (1%)	13 (10%)
Total	787	695 (88%)	92 (12%)	70 (9%)	7 (1%)	15 (2%)
Paying Taxes						
Technological	219 (26%)	171 (78%)	48 (22%)	47 (21%)	0 (0%)	1 (0%)
Administrative	207 (25%)	88 (43%)	119 (57%)	115 (56%)	1 (0%)	3 (1%)
Legal	413 (49%)	226 (55%)	187 (45%)	104 (25%)	2 (0%)	81 (20%)
Total	839	485 (58%)	354 (42%)	266 (32%)	3 (0%)	85 (10%)
Enforcing Contracts						
Technological	159 (27%)	116 (73%)	43 (27%)	3 (2%)	40 (25%)	0 (0%)
Administrative	299 (51%)	169 (57%)	130 (43%)	17 (6%)	109 (36%)	4 (1%)
Legal	132 (22%)	76 (58%)	56 (42%)	10 (8%)	31 (23%)	15 (11%)
Total	590	361 (61%)	229 (39%)	30 (5%)	180 (31%)	19 (3%)
Resolving Insolvency						
Technological	1 (0%)	1 (100%)	0 (0%)	0 (0%)	0 (0%)	0 (0%)
Administrative	49 (13%)	31 (63%)	18 (37%)	4 (8%)	14 (29%)	0 (0%)
Legal	336 (87%)	279 (83%)	57 (17%)	16 (5%)	16 (5%)	25 (7%)
Total	386	311 (81%)	75 (19%)	20 (5%)	30 (8%)	25 (6%)
Minority Investors						
Technological	36 (8%)	34 (94%)	2 (6%)	0 (0%)	1 (3%)	1 (3%)
Administrative	84 (18%)	35 (42%)	49 (58%)	13 (15%)	30 (36%)	6 (7%)
Legal	350 (74%)	250 (71%)	100 (29%)	11 (3%)	45 (13%)	44 (13%)
Total	470	319 (68%)	151 (32%)	24 (5%)	76 (16%)	51 (11%)
Labor Regulation						
Technological	0 (0%)	0 (0%)	0 (0%)	0 (0%)	0 (0%)	0 (0%)
Administrative	6 (1%)	2 (33%)	4 (67%)	4 (67%)	0 (0%)	0 (0%)
Legal	512 (99%)	378 (74%)	134 (26%)	5 (1%)	4 (1%)	125 (24%)
Total	518	380 (73%)	138 (27%)	9 (2%)	4 (1%)	125 (24%)
All Reform Types						
Technological	741 (21%)	641 (87%)	100 (14%)	57 (8%)	41 (6%)	2 (0%)
Administrative	981 (27%)	593 (60%)	388 (40%)	213 (22%)	160 (16%)	15 (2%)
Legal	1868 (52%)	1317 (71%)	551 (30%)	149 (8%)	99 (5%)	303 (16%)
Total	3590	2551 (71%)	1039 (29%)	419 (12%)	300 (8%)	320 (9%)

Table notes: This table presents the number of successful and failed reforms by type, i.e. Technological, Administrative, and Legal. For failed reforms, we further identify the stopper, i.e. Executive, Legislative, and Judicial. Percentages are reported in parentheses. In the *Total* column, the percentage reflects the share of reforms by type within the domain (column percentage). In the *Total (Successful)* and *Total (Failed)* columns, the percentages represent the share of successful or failed reforms within that type (row percentage). In the three *Stop* columns, the percentages represent the share of failed reforms stopped by each branch within that type (row percentage), which together sum to the percentage in the *Total (Failed)* column. The top six panels present numbers for each domain separately, and the final panel pools all six domains together.

Table 6: Event Study with GDP Interaction

	(1) Starting a Business	(2) Paying Taxes	(3) Enforcing Contracts	(4) Resolving Insolvency	(5) Minority Investors	(6) Labor Regulation	(7) All Domains
Post	.955***	.932***	.221**	-.08	1.187***	-.01	.549***
	(.138)	(.194)	(.092)	(.125)	(.252)	(.153)	(.073)
GDP	.113	.272**	-.006	.223	.083	-.047	.151**
	(.129)	(.126)	(.138)	(.14)	(.279)	(.092)	(.076)
Post # GDP	-.081***	-.083***	-.021**	.013	-.102***	.004	-.046***
	(.014)	(.018)	(.008)	(.013)	(.024)	(.016)	(.007)
Intercept	-1.106	-2.533**	.201	-1.791	-.734	.442	-1.33*
	(1.173)	(1.157)	(1.233)	(1.262)	(2.538)	(.857)	(.697)
Observations	1270	892	644	532	560	660	4558
R^2	.786	.827	.953	.933	.892	.933	.441
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table notes: This table presents ordinary least squares (OLS) regressions.
Specifications. Each observation corresponds to a successful reform and a two-year period, either pre- or post-reform. For each reform implemented in country c in year t, we compute two-period averages of the relevant outcome: one for the pre-reform period (years t–2 and t–1) and one for the post-reform period (years t+1 and t+2). These averages are then regressed on an indicator variable equal to 1 for the post-reform period and 0 for the pre-reform period. All specifications include the log of GDP per capita, matched to each country-year and averaged over the same two-year windows as the outcomes. An interaction term between the post-reform indicator and the log of GDP per capita is also included. All specifications control for country and year fixed effects.
Regulatory Indices. All outcomes are first standardized across all countries and years (z-score), with higher values indicating more market friendly environments, and then averaged within each domain (Columns 1–6), or across all domains (Column 7). For example, the outcome for Starting a Business (Column 1) is calculated as the mean of the z-scores for the three indicators in that domain: number of procedures, time (days), and cost (as a percentage of income per capita). The table reports separately by reform domain in Columns 1–6 (e.g., Starting a Business, Paying Taxes), with Column 7 pooling all domains.
Observations. Each reform contributes two observations: one for the pre-reform period and one for the post-reform period. The number of observations reflects only successful reforms with non-missing outcome data in both periods. For example, 485 paying-taxes reforms would generate 970 observations. However, 37 reforms occur in 2005 or 2022, the first and last years of our index data (2005–2022), so for these reforms either the pre- or post-reform observation is mechanically missing. In addition, 2 reforms are dropped due to missing index data. Accounting for these cases explains the observed count of 892 observations for paying taxes.
Standard errors are clustered by country, shown in brackets under each coefficient. *p<.1; **p<.05; ***p<.01.

Table 7: Number of Reform Attempts on Initiator, Reform Type, and GDP

	(1) Starting a Business	(2) Paying Taxes	(3) Enforcing Contracts	(4) Resolving Insolvency	(5) Minority Investors	(6) Labor Regulation	(7) All Domains
Initiator							
Judiciary	-1.109***		.028	-.416***	-.286***		-3.437***
	(.069)		(.046)	(.041)	(.041)		(.147)
Legislature	-1.021***	.009	-.337***	-.3***	-.166***	-.118**	-2.76***
	(.069)	(.052)	(.046)	(.041)	(.041)	(.051)	(.147)
Type							
Technological	.354***	-.342***	.048		-.554***		-1.988***
	(.069)	(.06)	(.046)		(.041)		(.147)
Administrative	.372***	-.363***	.295***	-.211***	-.469***	-.441***	-1.564***
	(.069)	(.06)	(.046)	(.036)	(.041)	(.051)	(.147)
GDP	.145***	.058***	.054***	.045***	.076***	.08***	.458***
	(.02)	(.017)	(.013)	(.012)	(.012)	(.017)	(.042)
Intercept	-.31*	.227	-.125	.149	.115	-.192	1.438***
	(.179)	(.152)	(.12)	(.105)	(.106)	(.147)	(.381)
Observations	1701	1701	1701	1701	1701	1701	1701
R^2	.194	.033	.077	.085	.151	.057	.355

Table notes: This table presents ordinary least squares (OLS) regressions. Each observation in this regression corresponds to a country–initiator–type combination, where initiators are Executive, Legislature, or Judiciary, and reform types are Technological, Administrative, or Legal. The dependent variable is the number of reform attempts in that combination, aggregated over the period 2005–2022. The data are collapsed accordingly, and combinations with no attempted reforms are recorded as zero. For example, in the Labor Regulation domain, the United States attempted five legal reforms: two successful reforms initiated by the executive branch, one failed reform initiated by the executive branch and two failed reforms by the legislative branch. This results in counts of 3 for Legal–Executive, 2 for Legal–Legislative, and 0 for Legal–Judicial. All Technological and Administrative combinations are also recorded as 0 due to no reform attempts. The table reports result separately by reform domain in Columns 1–6 (e.g., Starting a Business, Paying Taxes), with Column 7 pooling all domains. Regressors include dummy variables for reform initiator and reform type, as well as the average log of GDP per capita over 2005–2022. The number of observations is constant across columns: 1,701, calculated as 189 countries by 3 reform initiators by 3 reform types. Coefficients are not estimated where the number of observations is fewer than five. Standard errors are shown in brackets under each coefficient. *p<.1; **p<.05; ***p<.01.

Table 8: Reform Success on Initiator, Reform Type and Income

	(1) Starting a Business	(2) Paying Taxes	(3) Enforcing Contracts	(4) Resolving Insolvency	(5) Minority Investors	(6) Labor Regulation	(7) All Domains
Initiator							
Judiciary	-.056		.427***	-.297***	.126**		.079***
	(.053)		(.038)	(.074)	(.051)		(.022)
Legislature	-.178***	-.296***	.07	.03	-.009	.05	-.128***
	(.042)	(.032)	(.059)	(.045)	(.042)	(.044)	(.018)
Type							
Technological	-.075*	.087**	-.111**		.048		.044**
	(.041)	(.036)	(.053)		(.071)		(.019)
Administrative	-.153***	-.105***	-.129***	-.1*	-.273***	-.334*	-.087***
	(.037)	(.036)	(.046)	(.058)	(.048)	(.18)	(.017)
GDP	.067***	.157***	.1***	.076***	.153***	.057***	.112***
	(.008)	(.01)	(.012)	(.013)	(.013)	(.013)	(.005)
Intercept	.381***	-.704***	-.388***	.146	-.7***	.204*	-.267***
	(.076)	(.09)	(.108)	(.124)	(.118)	(.121)	(.044)
Observations	787	839	590	386	470	518	3590
R^2	.158	.352	.3	.143	.317	.048	.182

Table notes: This table presents ordinary least squares (OLS) regressions. Each observation in this regression corresponds to a single reform attempt, categorized by country, reform initiator (Executive, Legislature, or Judiciary), and reform type (Technological, Administrative, or Legal), over the period 2005–2022. The dependent variable is a binary indicator equal to 1 if the reform was successful and 0 otherwise. The data are used at the reform level without collapsing. Regressors include dummy variables for reform initiator and reform type, and the log of GDP per capita in the corresponding year. The table reports results separately by reform domain in Columns 1–6, with Column 7 pooling all domains. The number of observations reflects the number of attempted reforms across all countries and years within each specified domain. Coefficients are not estimated where the number of observations is fewer than five. Standard errors are shown in parentheses under each coefficient. *p<.1; **p<.05; ***p<.01.

Table 9a: Reforms by Initiator and Stopper: Rich vs. Poor Countries

Initiator	Total	Total (Successful)	Total (Failed)	Stop		
				Executive	Judiciary	Legislature
Rich						
Executive	1446 (65%)	1236 (85%)	210 (15%)	44 (3%)	66 (5%)	100 (7%)
Judiciary	280 (13%)	249 (89%)	31 (11%)	10 (4%)	10 (4%)	11 (4%)
Legislature	488 (22%)	371 (76%)	117 (24%)	64 (13%)	19 (4%)	34 (7%)
Total	2214	1856 (84%)	358 (16%)	118 (5%)	95 (4%)	145 (7%)
Poor						
Executive	922 (67%)	486 (53%)	436 (47%)	152 (16%)	151 (16%)	133 (14%)
Judiciary	139 (10%)	92 (66%)	47 (34%)	19 (14%)	17 (12%)	11 (8%)
Legislature	315 (23%)	117 (37%)	198 (63%)	130 (41%)	37 (12%)	31 (10%)
Total	1376	695 (51%)	681 (49%)	301 (22%)	205 (15%)	175 (13%)

Table notes: This table presents the number of successful and failed reforms by initiator, i.e. Executive, Legislative, and Judicial, separately for Rich and Poor countries. For failed reforms, we further identify the stopper, i.e. Executive, Legislature, and Judiciary. Countries are classified as rich if their GDP in 2005 is above or equal to the median, and poor if it is below the median. For South Sudan, which has missing 2005 GDP data, the classification is based on its 2008 GDP; as it falls below the median, it is classified as poor. Percentages are reported in parentheses. In the *Total* column, the percentage reflects the share of reforms initiated by each branch within the domain (column percentage). In the *Total (Successful)* and *Total (Failed)* columns, the percentages represent the share of successful or failed reforms within that initiator (row percentage). In the three *Stop* columns, the percentages represent the share of failed reforms stopped by each branch within that initiator (row percentage), which together sum to the percentage in the *Total (Failed)* column. The upper panel present numbers for all domains in Rich countries, and the lower panel present numbers for all domains in Poor countries.

Table 9b: Reforms by Type and Stopper: Rich vs. Poor Countries

Initiation	Total	Total (Successful)	Total (Failed)	Stop		
				Executive	Judicial	Legislature
Rich						
Technological	579 (26%)	550 (95%)	29 (5%)	16 (3%)	13 (2%)	0 (0%)
Administrative	475 (21%)	374 (79%)	101 (21%)	46 (10%)	50 (11%)	5 (1%)
Legal	1160 (52%)	932 (80%)	228 (20%)	56 (5%)	32 (3%)	140 (12%)
Total	2214	1856 (84%)	358 (16%)	118 (5%)	95 (4%)	145 (7%)
Poor						
Technological	162 (12%)	91 (56%)	71 (44%)	41 (25%)	28 (17%)	2 (1%)
Administrative	506 (37%)	219 (43%)	287 (57%)	167 (33%)	110 (22%)	10 (2%)
Legal	708 (51%)	385 (54%)	323 (46%)	93 (13%)	67 (9%)	163 (23%)
Total	1376	695 (51%)	681 (49%)	301 (22%)	205 (15%)	175 (13%)

Table notes: This table presents the number of successful and failed reforms by type, i.e. Technological, Administrative, and Legal, separately for Rich and Poor countries. For failed reforms, we further identify the stopper, i.e. Executive, Legislature, and Judiciary. Countries are classified as rich if their GDP in 2005 is above or equal to the median, and poor if it is below the median. For South Sudan, which has missing 2005 GDP data, the classification is based on its 2008 GDP; as it falls below the median, it is classified as poor. Percentages are reported in parentheses. In the *Total* column, the percentage reflects the share of reforms by type within the domain (column percentage). In the *Total (Successful)* and *Total (Failed)* columns, the percentages represent the share of successful or failed reforms within that type (row percentage). In the three *Stop* columns, the percentages represent the share of failed reforms stopped by each branch within that type (row percentage), which together sum to the percentage in the *Total (Failed)* column. The upper panel present numbers for all domains in Rich countries, and the lower panel present numbers for all domains in Poor countries.

Table 10: Number of Successful Attempts on Initiator, Reform Type, and Income

	(1) Starting a Business	(2) Paying Taxes	(3) Enforcing Contracts	(4) Resolving Insolvency	(5) Minority Investors	(6) Labor Regulation	(7) All Domains
Initiation							
Judiciary	-1***		.226***	-.353***	-.153***		-2.436***
	(.068)		(.038)	(.037)	(.035)		(.135)
Legislature	-.938***	-.145***	-.118***	-.231***	-.093***	-.073*	-2.176***
	(.068)	(.043)	(.038)	(.037)	(.035)	(.043)	(.135)
Type							
Technological	.372***	-.097*	.071*		-.381***		-1.192***
	(.068)	(.05)	(.038)		(.035)		(.135)
Administrative	.282***	-.243***	.164***	-.192***	-.379***		-1.277***
	(.068)	(.05)	(.038)	(.032)	(.035)		(.135)
GDP	.168***	.123***	.081***	.056***	.097***	.078***	.603***
	(.019)	(.014)	(.011)	(.01)	(.01)	(.014)	(.038)
_cons	-.602***	-.603***	-.594***	-.041	-.309***	-.424***	-1.307***
	(.175)	(.127)	(.098)	(.094)	(.09)	(.123)	(.348)
Observations	1701	1701	1701	1701	1701	1701	1701
R^2	.185	.061	.086	.086	.14	.02	.31

Table notes: This table presents ordinary least squares (OLS) regressions. Each observation in this regression corresponds to a country–initiator–type combination, where initiators are Executive, Legislature, or Judiciary, and reform types are Technological, Administrative, or Legal. The dependent variable is the number of successful reform attempts in that combination, aggregated over the period 2005–2022. The data are collapsed accordingly, and combinations with no successful reforms are recorded as zero. For example, in the Labor Regulation domain, the United States attempted five legal reforms: two successful reforms initiated by the executive branch, one failed reform initiated by the executive branch and two failed reforms by the legislative branch. This results in counts of 2 for Legal–Executive, 0 for Legal–Legislative, and 0 for Legal–Judicial. All Technological and Administrative combinations are also recorded as 0 due to no reform attempts. The table reports result separately by reform domain in Columns 1–6 (e.g., Starting a Business, Paying Taxes), with Column 7 pooling all domains. Regressors include dummy variables for reform initiator and reform type, as well as the average log of GDP per capita over 2005–2022. The number of observations is constant across columns: 1,701, calculated as 189 countries by 3 reform initiators by 3 reform types. Coefficients are not estimated where the number of observations is fewer than five. Standard errors are shown in brackets under each coefficient. *p<.1; **p<.05; ***p<.01.

Table 11: Event Study with Initial Level of Regulation Interactions

	(1) Starting a Business	(2) Paying Taxes	(3) Enforcing Contracts	(4) Resolving Insolvency	(5) Minority Investors	(6) Labor Regulation	(7) All Domains
Post	.154***	.141***	.036***	.063***	.225***	.02	.12***
	(.008)	(.01)	(.013)	(.024)	(.028)	(.018)	(.007)
Post # Initial	-.185***	-.248***	-.08***	-.066*	-.167***	-.111***	-.164***
	(.041)	(.034)	(.024)	(.037)	(.037)	(.026)	(.02)
_cons	-.063	-.047	.17**	.23***	.053	.009	-.01
	(.049)	(.038)	(.08)	(.033)	(.169)	(.066)	(.035)
Observations	1194	840	598	506	560	660	4358
R^2	.809	.845	.953	.932	.892	.936	.885
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table notes: This table presents ordinary least squares (OLS) regressions.
Specifications. Each observation corresponds to a successful reform and a two-year period, either pre- or post-reform. For each reform implemented in country c in year t, we compute two-period averages of the relevant outcome: one for the pre-reform period (years t–2 and t–1) and one for the post-reform period (years t+1 and t+2). These averages are then regressed on an indicator variable equal to 1 for the post-reform period and 0 for the pre-reform period. All specifications include the average 2005 level of outcomes (z-scores) within the corresponding domain. An interaction term between the post-reform indicator and the initial outcome is also included. All specifications control for country and year fixed effects.
Regulatory indices. All outcomes are first standardized across all countries and years (z-score), with higher values indicating more market friendly environments, and then averaged within each domain (Columns 1–6), or across all domains (Column 7). For example, the outcome for Starting a Business (Column 1) is calculated as the mean of the z-scores for the three indicators in that domain: number of procedures, time (days), and cost (as a percentage of income per capita). The table reports separately by reform domain in Columns 1–6 (e.g., Starting a Business, Paying Taxes), with Column 7 pooling all domains.
Observations. Each reform contributes two observations: one for the pre-reform period and one for the post-reform period. The number of observations reflects only successful reforms with non-missing index data in both periods. For example, 485 paying-taxes reforms would generate 970 observations. However, 37 reforms occur in 2005 or 2022, the first and last years of our index data (2005–2022), so for these reforms either the pre- or post-reform observation is mechanically missing. In addition, 2 reforms are dropped due to missing index data: the Kosovo (2009) reform is missing its pre-period (2007–2008), and the Malta (2006) reform is missing both its pre- and post-period (2005 and 2007–2008). Additional 52 observations are lost due to missing initial index data in ten countries (Brunei, Cyprus, Kosovo, Liberia, Luxembourg, Malta, Montenegro, Myanmar, Qatar, and San Marino). Accounting for these cases explains the observed count of 840 observations for paying taxes.
Standard errors are clustered by country, shown in brackets under each coefficient. *p<.1; **p<.05; ***p<.01.

Table 12: Number of Attempts on Initiator, Type, and Initial Level of Regulation

	(1) Starting a Business	(2) Paying Taxes	(3) Enforcing Contracts	(4) Resolving Insolvency	(5) Minority Investors	(6) Labor Regulation	(7) All Domains
Initiation							
Judicial	-1.116***		.025	-.454***	-.286***		-3.437***
	(.074)		(.048)	(.049)	(.041)		(.15)
Legislative	-1.021***	.009	-.339***	-.318***	-.166***	-.118**	-2.76***
	(.074)	(.055)	(.048)	(.049)	(.041)	(.052)	(.15)
Type							
Technological	.355***	-.358***	.042		-.554***		-1.988***
	(.074)	(.064)	(.048)		(.041)		(.15)
Administrative	.364***	-.382***	.285***	-.239***	-.469***	-.441***	-1.564***
	(.074)	(.064)	(.048)	(.042)	(.041)	(.052)	(.15)
Initial	.124***	.028	.044*	.029	.084***	-.012	.813***
	(.029)	(.03)	(.024)	(.023)	(.023)	(.036)	(.109)
_cons	1.012***	.753***	.345***	.593***	.787***	.491***	5.53***
	(.069)	(.049)	(.044)	(.037)	(.038)	(.034)	(.139)
Observations	1557	1557	1557	1368	1701	1701	1701
R^2	.175	.029	.07	.084	.137	.044	.331

Table notes: This table presents ordinary least squares (OLS) regressions. Each observation in this regression corresponds to a country–initiator–type combination, where initiators are Executive, Legislative, or Judicial, and reform types are Technological, Administrative, or Legal. The dependent variable is the number of reform attempts in that combination, aggregated over the period 2005–2022. The data are collapsed accordingly, and combinations with no attempted reforms are recorded as zero. For example, in the Labor Regulation domain, the United States attempted five legal reforms: two successful reforms initiated by the executive branch, one failed reform initiated by the executive branch and two failed reforms by the legislative branch. This results in counts of 3 for Legal–Executive, 2 for Legal–Legislative, and 0 for Legal–Judicial. All Technological and Administrative combinations are also recorded as 0 due to no reform attempts. The table reports result separately by reform domain in Columns 1–6 (e.g., Starting a Business, Paying Taxes), with Column 7 pooling all domains. Regressors include dummy variables for reform initiator and reform type, as well as the initial level of regulation, defined as the average of outcome z-scores in 2005 within each domain (Columns 1–6), or across all domains (Column 7). Each index is standardized across all countries and years, where a higher value reflects a more market friendly environment. The number of observations is calculated as number of countries with non-missing initial level of regulation by 3 reform initiators by 3 reform types. Coefficients are not estimated where the number of observations is fewer than five. Standard errors are shown in brackets under each coefficient. *p<.1; **p<.05; ***p<.01.

Table 13: Reform Success on Initiation, Type and Initial Level of Regulation

	(1) Starting a Business	(2) Paying Taxes	(3) Enforcing Contracts	(4) Resolving Insolvency	(5) Minority Investors	(6) Labor Regulation	(7) All Domains
Initiation							
Judicial	-.048		.472***	-.358***	.141**		.083***
	(.055)		(.041)	(.076)	(.056)		(.023)
Legislative	-.147***	-.31***	.102	.07	.03	.073	-.127***
	(.044)	(.037)	(.064)	(.046)	(.046)	(.045)	(.018)
Type							
Technological	-.001	.105**	-.095		.14*		.076***
	(.042)	(.042)	(.058)		(.078)		(.02)
Administrative	-.132***	-.203***	-.135***	-.038	-.298***	-.441**	-.12***
	(.039)	(.042)	(.05)	(.061)	(.053)	(.181)	(.018)
Initial	.075***	.079***	.079***	.073***	.168***	-.069**	.198***
	(.014)	(.018)	(.023)	(.021)	(.027)	(.028)	(.014)
_cons	.98***	.732***	.464***	.832***	.697***	.72***	.763***
	(.038)	(.03)	(.044)	(.025)	(.032)	(.022)	(.012)
Observations	733	780	542	347	470	518	3590
R^2	.121	.165	.231	.111	.175	.025	.107

Table notes: This table presents ordinary least squares (OLS) regressions. Each observation in this regression corresponds to a single reform attempt, categorized by country, reform initiator (Executive, Legislative, or Judicial), and reform type (Technological, Administrative, or Legal), over the period 2005–2022. The dependent variable is a binary indicator equal to 1 if the reform was successful and 0 otherwise. The data are used at the reform level without collapsing. Regressors include dummy variables for reform initiator and reform type and the initial level of regulation, defined as the average of outcome z-scores in 2005 within each domain (Columns 1–6), or across all domains (Column 7). Each outcome is standardized across all countries and years, where a higher value reflects a more market friendly environment. The table reports result separately by reform domain in Columns 1–6 (e.g., Starting a Business, Paying Taxes), with Column 7 pooling all domains. The number of observations reflects the number of attempted reforms across all countries and years within each specified domain, excluding observations with missing initial level of regulation. Coefficients are not estimated where the number of observations is fewer than five. Standard errors are shown in brackets under each coefficient. *p<.1; **p<.05; ***p<.01.

Table 14a: Reforms by Initiation and Stopper: Initially More vs. Less Market-Friendly Countries

Initiation	Total	Total (Successful)	Total (Failed)	Stop		
				Executive	Judicial	Legislative
High initial index						
Executive	1283 (65%)	1052 (82%)	231 (18%)	62 (5%)	79 (6%)	90 (7%)
Judicial	262 (13%)	226 (86%)	36 (14%)	12 (5%)	13 (5%)	11 (4%)
Legislative	442 (22%)	328 (74%)	114 (26%)	58 (13%)	19 (4%)	37 (8%)
Total	1987	1606 (81%)	381 (19%)	132 (7%)	111 (6%)	138 (7%)
Low initial index						
Executive	1085 (68%)	670 (62%)	415 (38%)	134 (12%)	138 (13%)	143 (13%)
Judicial	157 (10%)	115 (73%)	42 (27%)	17 (11%)	14 (9%)	11 (7%)
Legislative	361 (23%)	160 (44%)	201 (56%)	136 (38%)	37 (10%)	28 (8%)
Total	1603	945 (59%)	658 (41%)	287 (18%)	189 (12%)	182 (11%)

Table notes: This table presents the number of successful and failed reforms by initiator, i.e. Executive, Legislative, and Judicial, separately for countries with higher or lower than median initial regulatory index scores. High initial index corresponds to more market friendly environments, as defined by the average of outcome z-scores in 2005 across all domains. For failed reforms, we further identify the stopper, i.e. Executive, Legislative, and Judicial. Countries with above-median initial level of market friendliness are defined as *High initial index* countries, while those with below-median initial level of market friendliness are defined as *Low initial index* countries. Percentages are reported in parentheses. In the *Total* column, the percentage reflects the share of reforms initiated by each branch within the domain (column percentage). In the *Total (Successful)* and *Total (Failed)* columns, the percentages represent the share of successful or failed reforms within that initiator (row percentage). In the three *Stop* columns, the percentages represent the share of failed reforms stopped by each branch within that initiator (row percentage), which together sum to the percentage in the *Total (Failed)* column. The upper panel present numbers for all domains in *High initial index* countries, and the lower panel present numbers for all domains in *Low initial index* countries.

Table 14b: Reforms by Type and Stopper: Initially More vs. Less Market-Friendly Countries

Initiation	Total	Total (Successful)	Total (Failed)	Stop		
				Executive	Judicial	Legislative
High initial index						
Technological	510 (26%)	470 (92%)	40 (8%)	19 (4%)	20 (4%)	1 (0%)
Administrative	446 (22%)	328 (74%)	118 (26%)	62 (14%)	51 (11%)	5 (1%)
Legal	1031 (52%)	808 (78%)	223 (22%)	51 (5%)	40 (4%)	132 (13%)
Total	1987	1606 (81%)	381 (19%)	132 (7%)	111 (6%)	138 (7%)
Low initial index						
Technological	231 (14%)	171 (74%)	60 (26%)	38 (16%)	21 (9%)	1 (0%)
Administrative	535 (33%)	265 (50%)	270 (50%)	151 (28%)	109 (20%)	10 (2%)
Legal	837 (52%)	509 (61%)	328 (39%)	98 (12%)	59 (7%)	171 (20%)
Total	1603	945 (59%)	658 (41%)	287 (18%)	189 (12%)	182 (11%)

Table notes: This table presents the number of successful and failed reforms by type, i.e. Technological, Administrative, and Legal, separately for countries with higher or lower than median initial level of regulation. For failed reforms, we further identify the stopper, i.e. Executive, Legislative, and Judicial. The initial level of regulation is defined as the average of outcome z-scores in 2005 across all domains. Countries with above-median initial level of market friendliness are defined as *High initial index* countries, while those with below-median initial level of market friendliness are defined as *Low initial index* countries. Percentages are reported in parentheses. In the *Total* column, the percentage reflects the share of reforms by type within the domain (column percentage). In the *Total (Successful)* and *Total (Failed)* columns, the percentages represent the share of successful or failed reforms within that type (row percentage). In the three *Stop* columns, the percentages represent the share of failed reforms stopped by each branch within that type (row percentage), which together sum to the percentage in the *Total (Failed)* column. The upper panel present numbers for all domains in *High initial index* countries, and the lower panel present numbers for all domains in *Low initial index* countries.

Table 15: Number of Successful Attempts on Initiator, Type, and Initial Level of Regulation

	(1) Starting a Business	(2) Paying Taxes	(3) Enforcing Contracts	(4) Resolving Insolvency	(5) Minority Investors	(6) Labor Regulation	(7) All Domains
Initiation							
Judicial	-1.008***		.229***	-.395***	-.153***		-2.436***
	(.073)		(.04)	(.044)	(.035)		(.14)
Legislative	-.94***	-.147***	-.114***	-.246***	-.093***	-.073*	-2.176***
	(.073)	(.047)	(.04)	(.044)	(.035)	(.044)	(.14)
Type							
Technological	.378***	-.106*	.066		-.381***		-1.192***
	(.073)	(.054)	(.04)		(.035)		(.14)
Administrative	.276***	-.258***	.16***	-.209***	-.379***		-1.277***
	(.073)	(.054)	(.04)	(.038)	(.035)		(.14)
Initial	.143***	.062**	.061***	.045**	.111***	-.029	.988***
	(.029)	(.026)	(.02)	(.02)	(.02)	(.03)	(.102)
_cons	.927***	.482***	.098***	.495***	.548***	.248***	4.066***
	(.068)	(.042)	(.036)	(.034)	(.033)	(.025)	(.13)
Observations	1557	1557	1557	1368	1701	1701	1701
R^2	.159	.024	.062	.078	.108	.002	.248

Table notes: This table presents ordinary least squares (OLS) regressions. Each observation in this regression corresponds to a country–initiator–type combination, where initiators are Executive, Legislative, or Judicial, and reform types are Technological, Administrative, or Legal. The dependent variable is the number of successful reform attempts in that combination, aggregated over the period 2005–2022. The data are collapsed accordingly, and combinations with no successful reforms are recorded as zero. For example, in the Labor Regulation domain, the United States attempted five legal reforms: two successful reforms initiated by the executive branch, one failed reform initiated by the executive branch and two failed reforms by the legislative branch. This results in counts of 2 for Legal–Executive, 0 for Legal–Legislative, and 0 for Legal–Judicial. All Technological and Administrative combinations are also recorded as 0 due to no reform attempts. The table reports result separately by reform domain in Columns 1–6 (e.g., Starting a Business, Paying Taxes), with Column 7 pooling all domains. Regressors include dummy variables for reform initiator and reform type, as well as the initial level of regulation, defined as the average of outcome z-scores in 2005 within each domain (Columns 1–6), or across all domains (Column 7). Each outcome is standardized across all countries and years, where a higher value reflects a more market friendly environment. The number of observations is calculated as number of countries with non-missing initial level of regulation by 3 reform initiators by 3 reform types. Coefficients are not estimated where the number of observations is fewer than five. Standard errors are shown in brackets under each coefficient. *p<.1; **p<.05; ***p<.01.

Figure 13: Success Rate vs. Attempts per Year

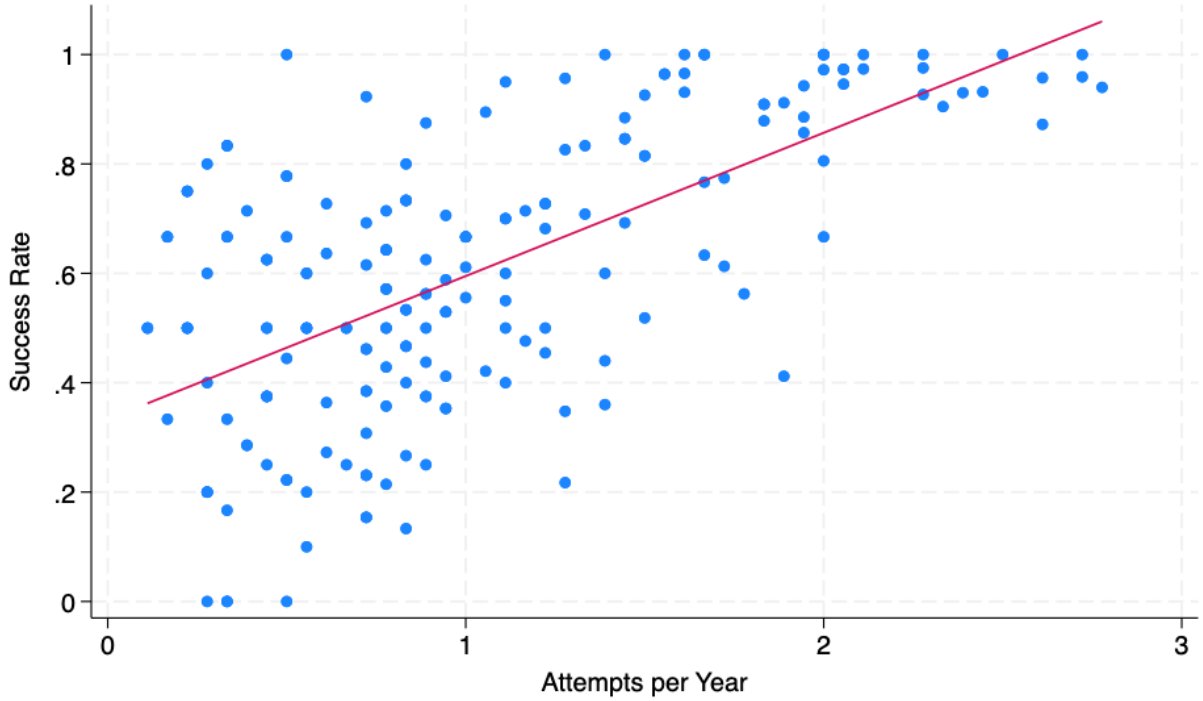


Figure notes: Each point represents a country (N = 189). The y-axis shows the success rate, defined as the number of successful reforms divided by total attempts over 2005-2022; the x-axis shows the average number of reform attempts per year over the same period. The red line plots the OLS fit, with the overall slope estimated at 0.262*** (SE = 0.023) from regressing the success rate on average number of reform attempts per year across all countries. *p<.1; **p<.05; ***p<.01.

Figure 14: Reform Effects vs. Attempts per Year

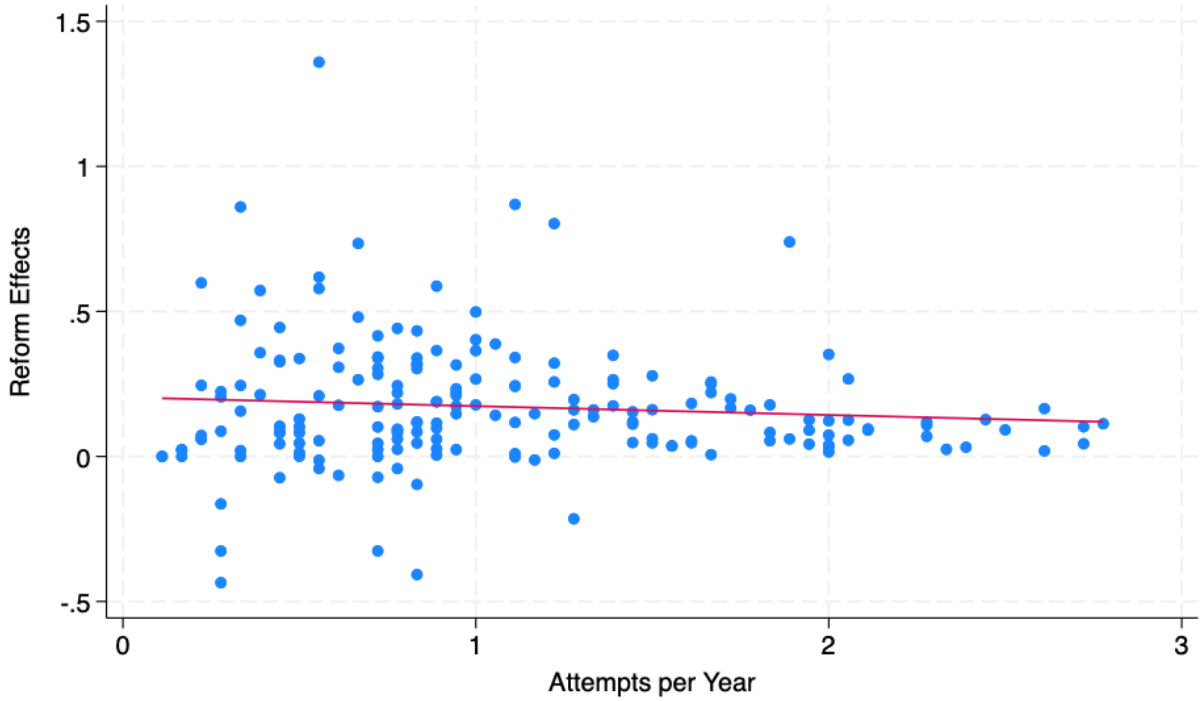


Figure notes: Each point represents a country (N = 185). The y-axis shows the estimated reform effects (successful reform only); the x-axis shows the average number of reform attempts per year over 2005–2022. For each country and reform domain, the reform effect is defined as the difference between the average outcome in the three years after the reform and the average outcome in the three years before the reform. All outcomes are first standardized across all countries and years (z-score), with higher values indicating more market friendly environments. The red line plots the OLS fit, with the overall slope estimated at -0.030 (SE = 0.025) from regressing reform effects on average number of reform attempts per year across all countries. Four observations are excluded because these countries have no successful reforms (Eritrea, Lebanon, Marshall Islands, and Somalia). *p<.1; **p<.05; ***p<.01.

Appendix Tables

Table A1: Change in Outcome on Initial Income and Initial Outcome (2022-2006)

Starting a Business

	(1) Procedure	(2) Time (days)	(3) Cost (% of income per capita)
Log of GDP per capita (2000)	.032	.056*	.068*
	(.042)	(.03)	(.038)
Initial "Procedure"	-.544***		
	(.065)		
Initial "Time (days)"		-.804***	
		(.033)	
Initial "Cost (% of income per capita)"			-.931***
			(.031)
_cons	.412	-.197	-.32
	(.342)	(.239)	(.305)
Observations	173	173	173
R-squared	.319	.779	.863

Paying Taxes

	(4) Tax payments per year	(5) Time (hours per year)	(6) Total tax and contribution rate
Log of GDP per capita (2000)	.177***	.058**	-.031
	(.044)	(.028)	(.037)
Initial "Tax payments per year"	-.829***		
	(.059)		
Initial "Time (hours per year)"		-.604***	
		(.034)	
Initial "Total tax and contribution rate"			-.74***
			(.041)
_cons	-1.084***	-.224	.375
	(.353)	(.22)	(.286)
Observations	173	173	173
R-squared	.547	.658	.68

Enforcing Contracts

	(7) Time (days)	(8) Cost (% of contract)
Log of GDP per capita (2000)	.054*	.05*
	(.027)	(.028)
Initial "Time (days)"	-.161***	
	(.041)	
Initial "Cost (% of contract)"		-.362***
		(.036)
_cons	-.549**	-.372*
	(.211)	(.216)
Observations	173	173
R-squared	.093	.391

Resolving Insolvency

	(9) Time (years)	(10) Cost (% of estate)
Log of GDP per capita (2000)	.194***	.095***
	(.046)	(.021)
Initial "Time (years)"	-.426***	
	(.058)	
Initial "Cost (% of estate)"		-.158***
		(.032)
_cons	-1.434***	-.731***
	(.363)	(.168)
Observations	152	152
R-squared	.279	.172

Minority Investors

	(11) Extent of disclosure index	(12) Extent of director liability index
Log of GDP per capita (2000)	-.027	-.01
	(.036)	(.032)
Initial "Extent of disclosure index"	-.339***	
	(.057)	
Initial "Extent of director liability index"		-.173***
		(.05)
_cons	.653**	.342
	(.284)	(.251)
Observations	189	189
R-squared	.175	.075

Labor Regulation

	(13) Difficulty of hiring index	(14) Rigidity of hours index	(15) Difficulty of redundancy index
Log of GDP per capita (2000)	.039	-.034	-.001
	(.027)	(.031)	(.024)
Initial "Difficulty of hiring index"	-.206***		
	(.042)		
Initial "Rigidity of hours index"		-.237***	
		(.048)	
Initial "Difficulty of redundancy index"			-.159***
			(.038)
_cons	-.265	.209	.058
	(.213)	(.242)	(.187)
Observations	189	189	189
R-squared	.115	.122	.092

Table notes: This table presents ordinary least squares (OLS) regressions. Each observation in this regression corresponds to a country. The dependent variable is the change in outcomes between the 2006 and 2022. Each outcome is standardized across all countries and years, where a higher value reflects a more market friendly environment. Regressors include the log of GDP per capita in 2000 (for South Sudan, where 2000 data are unavailable, the 2008 value is used) and the corresponding outcome in 2005. The table is organized into six panels, each corresponding to a reform domain (e.g., Starting a Business, Paying Taxes). Each panel includes two to three outcome variables specific to that domain. The number of observations reflects the set of countries with non-missing data for the outcome change, initial income, and initial outcome. Standard errors are shown in brackets under each coefficient. *p<.1; **p<.05; ***p<.01.

Table A2: Event Study (Failed)

	(1) Starting a Business	(2) Paying Taxes	(3) Enforcing Contracts	(4) Resolving Insolvency	(5) Minority Investors	(6) Labor Regulation	(7) All Domains
Post	.133	.049**	-.002	-.07	.182***	.005	.056***
	(.092)	(.02)	(.018)	(.065)	(.047)	(.021)	(.013)
_cons	-1.037***	-.386***	-.102	-.284***	.092	-.021	-.333***
	(.138)	(.089)	(.188)	(.081)	(.081)	(.099)	(.126)
Observations	158	640	370	100	286	202	1756
R^2	.841	.883	.947	.97	.868	.983	.507
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table notes: This table replicate table 3 with failed reforms. Standard errors are shown in brackets under each coefficient. *p<.1; **p<.05; ***p<.01.

Table A3: Event Study with GDP Interaction (Failed)

	(1) Starting a Business	(2) Paying Taxes	(3) Enforcing Contracts	(4) Resolving Insolvency	(5) Minority Investors	(6) Labor Regulation	(7) All Domains
Post	1.089	.152	.058	-.439	.903***	.074	.269**
	(1.046)	(.196)	(.132)	(.351)	(.293)	(.115)	(.112)
GDP	.368	.143**	.119	.295	.188	-.011	.17**
	(.223)	(.07)	(.121)	(.233)	(.195)	(.08)	(.079)
Post # GDP	-.128	-.014	-.009	.041	-.091**	-.008	-.028**
	(.126)	(.023)	(.015)	(.035)	(.036)	(.012)	(.013)
_cons	-3.693**	-1.44**	-1.019	-2.652	-1.344	.065	-1.623**
	(1.516)	(.571)	(1.02)	(1.831)	(1.483)	(.689)	(.629)
Observations	158	640	370	100	286	202	1756
R^2	.849	.885	.948	.972	.874	.983	.509
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table notes: This table replicate table 6 with failed reforms. Standard errors are shown in brackets under each coefficient. *p<.1; **p<.05; ***p<.01.

Table A4: Event Study with Initial Level of Regulation Interactions (Failed)

	(1) Starting a Business	(2) Paying Taxes	(3) Enforcing Contracts	(4) Resolving Insolvency	(5) Minority Investors	(6) Labor Regulation	(7) All Domains
Post	-.11	.003	-.007	-.092	.084**	.006	.015
	(.097)	(.015)	(.017)	(.078)	(.036)	(.023)	(.012)
Post # Initial	-.27*	-.121**	-.042	-.051	-.242***	-.019	-.128***
	(.145)	(.048)	(.031)	(.045)	(.06)	(.044)	(.037)
_cons	-1.047***	-.409***	-.089	-.273***	.092	-.021	-.242***
	(.199)	(.088)	(.188)	(.083)	(.081)	(.099)	(.073)
Observations	144	598	348	98	286	202	1676
R^2	.869	.884	.944	.971	.88	.983	.899
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table notes: This table replicate table 11 with failed reforms. Standard errors are shown in brackets under each coefficient. *p<.1; **p<.05; ***p<.01.

Annex: Proofs of Propositions

Proof of Proposition 1: If R is negative, then the initiator does not want the reform to pass, and consequently sets q=0, and for the remainder of this proof, we assume R>0. In that case, the leader chooses q to maximize $R(1-\theta q^2 L)(1-\varphi(1-q)L)^N$, and the derivative of this with respect to q is $R\big(\varphi N(1-\theta q^2 L)-2\theta q(1-\varphi L+\varphi qL)\big)L(1-\varphi(1-q)L)^{N-1}$ or $\frac{\varphi N}{\theta}-(2+N)\varphi L q^2-2(1-\varphi L)q$ times $RL\theta(1-\varphi(1-q)L)^{N-1}>0$.

The expression $\frac{\varphi N}{\theta}-(2+N)\varphi L q^2-2(1-\varphi L)q$ is obviously positive for small positive values of q and monotonically declining with q (when q is positive) and hence there is a unique value of q for which this is equal to zero, and that will deliver the maximum value of $R(1-\theta q^2 L)(1-\varphi(1-q)L)^N$ for positive q. The quadratic formula deliver that the maximum occurs when $q=q^*=\frac{\sqrt{(1-\varphi L)^2+\frac{\varphi^2(2+N)NL}{\theta}}-(1-\varphi L)}{(2+N)\varphi L}$, which is obviously independent of R. If $\frac{\varphi N}{\theta}>(2+N)\varphi L+2(1-\varphi L)$ and $\theta<\frac{\varphi N}{2+\varphi NL}$, then the derivative is still positive at q=1 and complete compensation is optimal. If $\theta>\frac{\varphi N}{2+\varphi NL}$ then q^* lies between zero and one. Note that returns are R when L=0, and when $\varphi L=1$, $q^*=\sqrt{\frac{N}{\theta L(2+N)}}$ if $\theta>\frac{\varphi N}{2+\varphi NL}$ in which case implies that expected returns are $\frac{2R}{2+N}\left(\frac{N}{\theta L(2+N)}\right)^{\frac{N}{2}}$ or $q^*=1$ if $\theta<\frac{\varphi N}{2+\varphi NL}$, and in that case expected returns are $R(1-\theta L)$

This also implies that $Lq^*=\frac{\sqrt{(1-\varphi L)^2+\frac{\varphi^2(2+N)NL}{\theta}}-(1-\varphi L)}{(2+N)\varphi}=\sqrt{\left(\frac{1-\varphi L}{(2+N)\varphi}\right)^2+\frac{NL}{(2+N)\theta}}-\frac{(1-\varphi L)}{(2+N)\varphi}$ and $1-\varphi L+\varphi Lq^*=\varphi\sqrt{\left(\frac{1-\varphi L}{(2+N)\varphi}\right)^2+\frac{NL}{(2+N)\theta}}+\frac{(1+N)(1-\varphi L)}{(2+N)}$.

The value of q^* also satisfies: $\frac{\varphi N}{\theta}=(2+N)\varphi Lq^{*2}+2(1-\varphi L)q^*$, and differentiating this yields $\frac{dq^*}{dN}=\frac{\frac{\varphi}{\theta}-\varphi Lq^{*2}}{2(1-\varphi L)+2(2+N)\varphi Lq^*}=\frac{(1-\varphi L+\varphi Lq^*)q^*}{N(1-\varphi L)+N(2+N)\varphi Lq^*}>0$, $\frac{dq^*}{d\theta}=\frac{-\frac{\varphi N}{\theta^2}}{2(1-\varphi L)+2(2+N)\varphi Lq^*}<0$, $\frac{dq^*}{d\varphi}=\frac{\frac{N}{\theta}-(2+N)Lq^{*2}+2Lq^*}{2(1-\varphi L)+2(2+N)\varphi Lq^*}=\frac{q^*}{\varphi(1-\varphi L)+\varphi(2+N)\varphi Lq^*}>0$ and $\frac{dq^*}{dL}=\frac{2\varphi q^*-(2+N)\varphi q^{*2}}{2(1-\varphi L)+2(2+N)\varphi Lq^*}$.

The value of total compensation $(q^{*2}L)$ is therefore also declining with θ, and increasing with N and φ.

We know that $\frac{dq^*}{dL} > 0$, if and only if $\frac{2}{2+N} > q^* = \frac{\sqrt{(1-\varphi L)^2+\frac{\varphi^2(2+N)NL}{\theta}}-(1-\varphi L)}{(2+N)\varphi L}$, or $\theta > \frac{(2+N)N\varphi}{4}$. The derivative of the total amount of compensation $(q^{*2}L)$ with respect to L is $q^{*2} + 2Lq^* \frac{dq^*}{dL} = q^{*2} + 2Lq^* \frac{2\varphi q^* - (2+N)\varphi q^{*2}}{2(1-\varphi L)+2(2+N)\varphi L q^*} = \frac{q^{*2}(1+\varphi L)}{(1-\varphi L)+(2+N)\varphi L q^*} > 0$.

Note that the derivative of $1 - L + Lq^*$ with respect to L is $-1 + L\frac{2\varphi q^* - (2+N)\varphi q^{*2}}{2(1-\varphi L)+2(2+N)\varphi L q^*} + q^*$, which as is clearly negative, as $(1 - q^*)\frac{2(1-\varphi L)+2(1+N)\varphi L q^*}{2(1-\varphi L)+2(2+N)\varphi L q^*} > \frac{-N\varphi L q^{*2}}{2(1-\varphi L)+2(2+N)\varphi L q^*}$.

If R<0, $q^* = 0$ and then returns equals $R(1 - \varphi L)^N$, which are increasing with R, L, N, and φ, and independent of all other parameters.

If R>0, expected returns are always $R(1 - \theta q^{*2}L)(1 - \varphi(1 - q^*)L)^N$, where q^* is optimally chosen.

If R>0, and $\theta < \frac{\varphi N}{2+\varphi NL}$, then $q^* = 1$ and expected returns are $R - \theta LR$ which is increasing in R and decreasing with L and θ, and independent of N.

If $\theta > \frac{\varphi N}{2+\varphi NL}$, returns are $R(1 - \theta q^{*2}L)(1 - \varphi(1 - q^*)L)^N$, but since q^* maximizes this quantity, the envelope theorem applies and expected returns are declining with θ, φ, N and L and increasing with R.

Proof of Proposition 2:

The probability of success is always $(1 - \varphi L + \varphi q^* L)^N$. If R<0, then this equals $(1 - \varphi L)^N$, which is falling with φ , L and N and independent of all other variables. If R>0 and $\theta < \frac{\varphi N}{2+\varphi NL}$, then this equals 1.

If $\theta > \frac{\varphi N}{2+\varphi NL}$, then the probability of success is declining with θ because q^* is declining with θ and increasing with R.

The derivative of $1 - \varphi L + \varphi q^* L$ with respect to L equals $-\varphi + \varphi q^* + \varphi L\frac{dq^*}{dL}$. If $\frac{dq^*}{dL} < 0$, then $1 - L + q^* L$ must be declining with L. The first order condition can be rewritten to be $\frac{\varphi N}{\theta q^*} + (1 - \varphi L)N = (2 + N)(1 - \varphi L + \varphi L q^*)$, and if $\frac{dq^*}{dL} > 0$ then the left hand side falls with L, and therefore the right hand side must fall with L, and if $1 - L + q^* L$ falls with L, the probability of success also falls.

The derivative of $1-\varphi L+\varphi q^{*}L$ with respect to φ equals $-L+q^{*}L+\varphi L\frac{dq^{*}}{d\varphi}=$ $\frac{Lq^{*}}{(1-\varphi L)+(2+N)\varphi Lq^{*}}-L(1-q^{*})$, which is positive if and if and only $\frac{q^{*}}{(1-\varphi L)+(2+N)\varphi Lq^{*}}>1-q^{*}$. The left-hand side of the inequality monotonically decreases with θ (because q^{*} is monotonically decreasing with θ), equal to $\frac{1}{(1-\varphi L)+(2+N)\varphi L}$ when $\theta=\frac{\varphi N}{2+\varphi NL}$ and goes to 0 as θ goes to infinity. The right-hand side of the inequality is monotonically increasing with θ, equal to 0 when $\theta=\frac{\varphi N}{2+\varphi NL}$ and goes to 1 as θ goes to infinity. Consequently, there is a unique value of θ, at which the inequality holds with equality, and for all values of θ above that value $\frac{q^{*}}{(1-\varphi L)+(2+N)\varphi Lq^{*}}<1-q^{*}$ and the probability of success falls with φ while for all values of of θ below that value $\frac{q^{*}}{(1-\varphi L)+(2+N)\varphi Lq^{*}}>1-q^{*}$and the probability of success rises with φ.

The probability of success declines with N if and only if $N(1-\varphi L+\varphi q^{*}L)^{N-1}\varphi L\frac{dq^{*}}{dN}+$ $(1-\varphi L+\varphi q^{*}L)^{N}ln(1-\varphi L+\varphi q^{*}L)<0$, which requires $-ln(1-\varphi L+\varphi q^{*}L)>$ $\frac{\varphi NL\frac{dq^{*}}{dN}}{1-\varphi L+\varphi q^{*}L}=\frac{q^{*}\varphi NL}{N(1-\varphi L)+N(2+N)\varphi Lq^{*}}$. The land hand side is increasing continuously with θ, equals 0 when $\theta=\frac{\varphi N}{2+\varphi NL}$ and goes to $-ln(1-\varphi L)>0$ as θ goes to infinity. The right-hand side is decreasing continuously with θ, equals $\frac{\varphi NL}{N(1-\varphi L)+N(2+N)\varphi L}>0$ when $\theta=\frac{\varphi N}{2+\varphi NL}$ and goes to 0 as θ goes to infinity. Consequently, there is a unique value of θ, at which the inequality holds with equality, and for all values of θ above that value $-ln(1-\varphi L+\varphi q^{*}L)>$ $\frac{q^{*}\varphi NL}{N(1-\varphi L)+N(2+N)\varphi Lq^{*}}$ and the probability of success falls with N while for all values of θ below that value $-ln(1-\varphi L+\varphi q^{*}L)<\frac{q^{*}\varphi NL}{N(1-\varphi L)+N(2+N)\varphi Lq^{*}}$and the probability of success rises with φ.

Proof of Proposition 3: We have assumed that prospective reforms differ in their value of L, where L is characterized by a distribution function G(L) which has mass everywhere on the unit interval. A reform attempt will be made whenever the expected social benefit of reform is greater than $-B/\alpha$, such that $R>\frac{-B}{\alpha}>R\cdot Max\left[1-\frac{\theta}{\varphi.},\frac{2}{2+N}\left(\frac{\varphi N}{\theta(2+N)}\right)^{\frac{N}{2}}\right]$, which ensures that there will be reform attempts when L=0 and no attempts when L=1. Using continuity and monotonicity in L, we know that there is an L such that

$R(1-\theta q^{*2}L)(1-\varphi(1-q^{*})L)^{N}=-B/\alpha$, and we define that L as L^{*}. Differentiation (and using the envelope theorem) then yields: $\frac{(1-\theta q^{*2}L)(1-\varphi(1-q^{*})L)}{R\theta q^{*2}(1-\varphi(1-q^{*})L)+\varphi(1-q^{*})NR(1-\theta q^{*2}L)}=\frac{dL^{*}}{dR}$ and $\frac{-Rq^{*2}L(1-\varphi(1-q^{*})L)}{R\theta q^{*2}(1-\varphi(1-q^{*})L)+\varphi(1-q^{*})NR(1-\theta q^{*2}L)}=\frac{dL^{*}}{d\theta}$.

We are interested in six objects beyond the probability of attempting reform: (1) probability of success conditional upon attempting reform, (2) probability of success conditional upon L, (3) rate of successful reform (which is the probability of attempting reform times the probability of success conditional upon reform, (4) gross returns to reform (which is just R and independent of L), (5) net returns conditional upon reform holding L constant (or $R(1-\theta q^{*2}L)$) and (6) average net returns conditional upon reform.

The frequency of reform will equal $G(L^*)$. The frequency of successful reform will equal $\int_{L=0}^{L^*}(1-\varphi(1-q^*(L)L)^N dG(L)$. The probability of success conditional upon reform is $\frac{1}{G(L^*)}\int_{L=0}^{L^*}(1-\varphi(1-q^*(L)L)^N dG(L)$ and the gross returns conditional upon successful reform equal R, while average net returns conditional upon reform equal $\frac{R\int_{L=0}^{L^*}(1-\theta q^{*2}(L)L)(1-\varphi(1-q^*(L)L)^N dG(L)}{\int_{L=0}^{L^*}(1-\varphi(1-q^*(L)L)^N dG(L)}$.

As gross returns are defined as R, they are increasing in R and independent of θ. Net returns conditional upon L equal $R(1-\theta q^{*2}L)$, which is also increasing in R since q^* is independent of R.

If $\theta < \frac{\varphi N}{2+\varphi NL}$ or $L < \frac{\varphi N-2\theta}{\varphi N\theta}$ and $q^* = 1$, then obviously net returns are always declining with θ.

If $L > \frac{\varphi N-2\theta}{\varphi N\theta}$, then net returns conditional upon L are increasing with θ if and only if $-Rq^{*2} - 2R\theta q^* \frac{dq^*}{d\theta} > 0$ or $-2\theta\frac{dq^*}{d\theta} > q^*$ or $\frac{2\frac{\varphi N}{\theta}}{2(1-\varphi L)+2(2+N)\varphi Lq^*} > q^*$ or $2\frac{\varphi N}{\theta} > 2(1-\varphi L)q^* + 2(2+N)\varphi Lq^{*2}$, and as $\frac{\varphi N}{\theta} = (2+N)\varphi Lq^{*2} + 2(1-\varphi L)q^*$ (the first order condition), the inequality holds.

The probability of success conditional upon L is 1 and independent of both R and θ if $L < \frac{\varphi N-2\theta}{\varphi N\theta}$. If $L > \frac{\varphi N-2\theta}{\varphi N\theta}$, then the probability of success conditional upon L is independent of R and falling with θ.

The derivative of the total number of successful reforms, $\int_{L=0}^{L^*}\left(1-\varphi(1-q^*(L)L)\right)^N g(L)dL$, with respect to R yields $\left(1-\varphi(1-q^*(L^*)L^*)\right)^N g(L^*)\frac{dL^*}{dR} > 0$. As $\frac{dL^*}{d\theta} < 0$ and $\frac{dq^*}{d\theta} < 0$, the derivative of the total number of successful reforms, $\int_{L=0}^{L^*}\left(1-\varphi(1-q^*(L,\theta)L)\right)^N g(L)dL$, with respect to θ yields $\left(1-\varphi(1-q^*(L^*)L^*)\right)^N g(L^*)\frac{dL^*}{d\theta} + \int_{L=0}^{L^*} N\left(1-\varphi(1-q^*(L,\theta)L)\right)^{N-1}\varphi L\frac{dq^*}{d\theta}g(L)dL < 0$.

The derivative of the average success rate with respect to R is $\int_{L=0}^{L^*}\left(\left(1-\varphi(1-q^*(L^*)L^*)\right)^N-\left(1-\varphi(1-q^*(L)L)\right)^N\right)g(L)dL\frac{g(L^*)}{\left(G(L^*)\right)^2}\frac{dL^*}{dR}$ and this is zero if $L^*<\frac{\varphi N-2\theta}{\varphi N\theta}$ (because $q^*(L^*)=1$) and negative if $L^*>\frac{\varphi N-2\theta}{\varphi N\theta}$, because $\left(1-\varphi(1-q^*(L)L)\right)$ is strictly declining with L for $L>\frac{\varphi N-2\theta}{\varphi N\theta}$ and weakly declining with L everywhere.

The derivative of the average success rate with respect to θ is $\int_{L=0}^{L^*}\left(\left(1-\varphi(1-q^*(L^*)L^*)\right)^N-\left(1-\varphi(1-q^*(L)L)\right)^N\right)g(L)dL\frac{g(L^*)}{\left(G(L^*)\right)^2}\frac{dL^*}{d\theta}+\frac{1}{G(L^*)}\int_{L=0}^{L^*}N\left(1-\varphi(1-q^*(L,\theta)L)\right)^{N-1}\varphi L\frac{dq^*}{d\theta}g(L)dL$. The first term is weakly positive (and strictly positive if $L^*>\frac{\varphi N-2\theta}{\varphi N\theta}$) and the second term is weakly negative (and strictly negative if $L^*>\frac{\varphi N-2\theta}{\varphi N\theta}$). If $L^*>\frac{\varphi N-2\theta}{\varphi N\theta}$ and $g(L^*)$ is sufficiently small, then the first term becomes arbitrarily small and the overall expression is negative.

To see that it can be positive. Consider a distribution of L with two mass points at zero and some value of $L>\frac{\varphi N-2\theta}{\varphi N\theta}$. An increase in θ that causes the reforms with mass L>0 not to be undertaken will necessarily increase the average success rate.

The derivative of the average return to successful reforms $\frac{R\int_{L=0}^{L^*}\left(1-\theta q^{*2}(L)L\right)(1-\varphi(1-q^*(L)L)^N dG(L)}{\int_{L=0}^{L^*}(1-\varphi(1-q^*(L)L)^N dG(L)}$ with respect to R is $\frac{\int_{L=0}^{L^*}\left(1-\theta q^{*2}(L)L\right)(1-\varphi(1-q^*(L)L)^N dG(L)}{\int_{L=0}^{L^*}(1-\varphi(1-q^*(L)L)^N dG(L)}+R\left(\frac{\int_{L=0}^{L^*}\left(\left(1-\theta q^{*2}(L^*)L^*\right)-\left(1-\theta q^{*2}(L)L\right)\right)(1-\varphi(1-q^*(L)L)^N dG(L)}{\int_{L=0}^{L^*}(1-\varphi(1-q^*(L)L)^N dG(L)}\right)\frac{g(L^*)\left(1-\varphi(1-q^*(L^*)L^*)\right)^N}{\int_{L=0}^{L^*}(1-\varphi(1-q^*(L)L)^N dG(L)}\frac{dL^*}{dR}$. The first term is always positive, and the second term is negative because $q^{*2}(L)L$ increases with L. If $g(L^*)$ is arbitrarily small, then the second term becomes arbitrarily small and the average returns to reform must go up with R.

To see that it can be negative, consider a distribution of L with two mass points at zero and some value of $L>\frac{\varphi N-2\theta}{\varphi N\theta}$. An increase in R that causes the reforms with mass L>0 to be undertaken will reduce the expected returns conditional upon success.

The derivative of the average return to successful reforms $\frac{R\int_{L=0}^{L^*}(1-\theta q^{*2}(L)L)(1-\varphi(1-q^*(L)L)^N dG(L)}{\int_{L=0}^{L^*}(1-\varphi(1-q^*(L)L)^N dG(L)}$ with respect to θ is $\frac{\mathrm{R}}{\int_{L=0}^{L^*}(1-\varphi(1-q^*(L)L)^N dG(L)}$ times $-\int_{L=0}^{L^*} q^{*2}(L)L(1-\varphi(1-q^*(L)L)^N dG(L) -$

$$\frac{\int_{L=0}^{L^*}(1-\theta q^{*2}(L)L)(1-\varphi(1-q^*(L)L)^N dG(L)\left(\int_{L=0}^{L^*}\varphi LN(1-\varphi(1-q^*(L)L)^{N-1}\frac{dq^*}{d\theta}dG(L)\right)}{\int_{L=0}^{L^*}(1-\varphi(1-q^*(L)L)^N dG(L)}$$

$$+\frac{\int_{L=0}^{L^*}\left(\left(1-\theta q^{*2}(L^*)L^*\right)-\left(1-\theta q^{*2}(L)L\right)\right)(1-\varphi(1-q^*(L)L)^N dG(L)\left(1-\varphi(1-q^*(L^*)L^*)\right)^N g(L^*)}{\int_{L=0}^{L^*}(1-\varphi(1-q^*(L)L)^N dG(L)}\frac{dL^*}{d\theta}.$$

The first effect is negative and it is direct. The second effect is positive and it represents the reduction in the denominator coming from changing values of q^*. The third represents selection and it is also positive.

To see that this can be positive, consider a distribution of L with two mass points at zero and some value of $L > \frac{\varphi N-2\theta}{\varphi N\theta}$. An increase in θ that causes the reforms with mass L>0 not to be undertaken will increase the expected returns conditional upon success. If $L^* < \frac{\varphi N-2\theta}{\varphi N\theta}$ and $g(L^*)$ is sufficiently close to zero, then only the first term remains and so the sign is negative.