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Abstract

We use the time series variation in the World Bank's "distance to frontier" estimates of the ease of doing business to assess the effects of changes in this variable on real GDP per capita. The use of Vector Autoregression techniques allows us to identify shocks to the ease of doing business that are initially uncorrelated with GDP, thus addressing an important endogeneity problem that affects the cross-sectional literature on this topic. The results are surprising. We report a robust finding that improvements to the ease of doing business have at least a temporary negative impact on GDP and find little evidence for a positive effect in the years following these improvements.

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1. Introduction

In the huge literature on the determinants of economic development, the key goal for economists has been uncovering the policies and institutions that can help a country to achieve high levels of GDP per capita and living standards. In some cases, historical events such as the divisions of Germany and Korea have provided natural experiment that illustrate some of the key ingredients for successful economic development. Examples of this kind have generated widespread agreement among economists that economic development requires an environment in which private businesses can operate relatively freely. The exact roles played by private property rights, free markets, effective regulation and democracy are the subject of vigorous ongoing debates but it is clear that development agencies such as the World Bank and IMF place great weight on the idea that a business-friendly environment is important for promoting economic growth. Commentary about the need for "structural reforms" to boost growth features in many of their publications.

Assessing the link between a country's business environment and its level of economic development has traditionally been difficult because of the absence of comparable cross-country data on the policy and regulatory environment that businesses operate in. Over the past decade, however, the World Bank's *Doing Business* publication has emerged as an important source of information on business environments around the world. This annual publication, launched in 2003, stemmed from a series of academic studies sponsored by the World Bank examining the costs of starting a business and the ability to enforce contracts (see Djankov, La Porta, Lopes-de-Silanes, and Shleifer, 2002, and Djankov, La Porta, Lopez-de-Silanes, and Shleifer, 2003). This was followed over time by further detailed studies on other aspects of the business environment such as the ability to obtain credit, the efficiency of insolvency procedures, the costs and delays associated with exporting and importing and the efficiency of tax collection.[1]

The *Doing Business* project now combines information on ten different aspects of the business environment and publishes an annual ranking of the ease of doing business that attracts substantial global attention. For example, when Narendra Modi became Prime Minister of India, he set of a goal of reaching 50th place in the ease of doing business rankings.[2] Another example, noted by World Bank (2017), is Kenya, where an Ease of Doing Business delivery unit was set up to meet every two weeks, chaired by either the Deputy President or the Minister of Industrialization, to monitor progress on an action plan aimed at improving the country's *Doing Business* ranking.[3]

From a research perspective, the *Doing Business* publication has a number of important strengths. Substantial resources are devoted to collecting detailed and timely data for almost every country

[1] See Djankov, McLiesh, and Shleifer (2007), Djankov, Hart, McLiesh, and Shleifer (2008), Djankov, Freund, and Pham (2010) and Djankov, Ganser, McLiesh, Ramalho, and Shleifer (2010). See also Djankov (2016) for a discussion of how the Doing Business project began.

[2] See http://www.institutionalinvestor.com/article/3421742/banking-and-capital-markets-emerging-markets/prime-minister-narendra-modi-has-high-hopes-for-india

[3] See https://kepsa.or.ke/business-environment-reforms-2019-update-meeting/

in the world: For example, every year the World Bank collects information on the costs and delays associated with opening a specific standardised type of business in each country. It is unsurprising, then, that the data from the survey have been widely used to assess the linkages between the business environment and economic outcomes.

A large number of papers have focused on linking sub-components of the survey to specific outcome variables. For example, Djankov, Ganser, McLiesh, Ramalho, and Shleifer (2010) use the tax component of the survey to uncover negative effects of high corporate tax rates on entrepreneurship and investment. Lawless (2013) finds the complexity of the corporate tax system has a negative effect on inward foreign direct investment. Norback, Persson and Douhan (2014) find that better scores on the survey's measures of the ease of starting a business are associated with higher openness to trade while Freund and Bolasky (2008) find the benefits to economic development from openness are higher in economies with good scores on this measure. In relation to broader measures of economic development, Djankov, McLiesh and Ramalho (2006) find that countries with better ease doing business scores have consistently grown faster while Gillanders and Whelan (2014) find that the ease of doing business rank works better as an explanatory variable for GDP per capita than other popular institutional variables such as the rule of law index from the World Bank's Governance Matters dataset, which has been a popular explanatory variable in the literature on the link between institutions and development.

In this paper, we revisit the link between real GDP per capita and the ease of doing business, making a number of new contributions. Our first contribution is the use of a panel data set to explore the time dimension of the relationship between the ease of doing business and real GDP. Because of the relatively short time series available from the *Doing Business* survey, existing research has focused on pure cross-sectional relationships. With twelve years of data from the survey now available, we believe there is sufficient time dimension in the data now to warrant an investigation of the impact of variation over time in the *Doing Business* indicators. The use of panel data has a number of advantages over cross-sectional analysis. It can potentially expose relationships that exist in the cross-section as being spurious. In addition, it is unlikely that cross-sectional relationships can provide useful estimates of the short- or medium-term impact of policy changes. For example, if a poor country with significant restrictions on business introduces a major program of structural reforms, it is highly unlikely that GDP will immediately jump up to the level predicted by purely cross-sectional estimates. A time series approach is required to assess the dynamics of this relationship.

Our second contribution is to introduce a new approach to identifying the effect of the ease of doing business on GDP. It is well understood that the cross-sectional literature on the impact of policies and institutions on economic development is plagued by endogeneity problems. We may be able to identify a set of good policies that possibly encourage high levels of economic development but high-income countries are also more likely to have a social and political consensus that favors such

policies. Much of the previous research on this topic has attempted to deal with this problem using instrumental variables such as geographic or climate-related variables (as in Hall and Jones, 1999) or information on a country's colonial past (as in Acemoglu, Johnson and Robinson, 2001) to identify plausibly exogenous variations in variables that have had a likely influence on institutions and economic policy. This is the approach followed by Djankov, McLiesh and Ramalho (2006) and Gillanders and Whelan (2014) when analyzing the impact of the ease of doing business on GDP. While this type of research has been influential, it is affected by problems due to the limited amount of variation in institutions associated with the instruments (and thus weak first-stage fits), controversies over the true exogeneity of various instruments and collinearity between different plausible institutional or policy variables. Kelly (2019) also suggests many apparently significant findings in the cross-country literature on economic development may reflect spurious spatial noise.

In contrast to the cross-sectional approach, we use Vector Autoregression (VAR) techniques to identify the dynamic impact of shocks to the ease of doing business that are, by construction, uncorrelated with GDP during the period when they occur, thus avoiding the endogeneity issue that has been the principal focus of the cross-sectional literature. Because we would expect reforms to the ease of doing business to take a number of years to have an impact, we would still expect the shocks we identify to have an impact on GDP over time and these impacts can be assessed using a VAR.

A final contribution is the use of a new measure of the ease of doing business. Most previous research in this area has used the ease of doing business ranking as its explanatory variable. However, the ranking is constructed from a set of underlying scores and movements in a country's rank may reflect developments elsewhere rather than within the country. We also document that relatively small changes in the underlying indicators can result in large changes in a country's rank, depending on where the country sits in the overall distribution. In recent years, however, the *Doing Business* team have published a series of "distance to frontier" scores for the underlying indicators used to construct the ease of doing business rank. We construct a new measure of the ease of doing business using this data to provide a series constructed according to a single methodology that is available for the years 2006-2017. We believe this variable is more suitable than the rank variable for both cross-sectional analysis as well as the dynamic analysis that is undertaken in this paper.

Our results are somewhat surprising. We do not find evidence of a consistent positive effect of improvements in the ease of doing business. In fact, we report that improvements to the ease of doing business appear to have at least a temporary negative impact on GDP and the finding is robust across many different specifications. It holds for different sources for GDP data and we obtain similar (though somewhat weaker) findings when we look at the well-known sub-components of the overall Doing Business score. In our conclusions, we discuss possible explanations for the puzzling results.

2. Data

In this section, we describe the *Doing Business* project in more detail and present the data from the project that we use.

2.1. The Doing Business Project

The origins of the *Doing Business* project have been described by Djankov (2016). In the early 2000s, the World Bank had been running a project on the importance of institutions in development, at the instigation of then chief economist Joseph Stiglitz. World Bank economist Simeon Djankov turned to a research team lead by Andrei Shleifer to co-operate with the World Bank to produce a series of papers measuring business-related institutions and policies around the world. The work from these early papers, such as Djankov, La Porta, Lopes-de-Silanes, and Shleifer (2002, 2003) constructed a series of data sets that resulted in the first *Doing Business* rankings, based on five indicators: starting a business, enforcing contracts, getting credit, employing workers and resolving insolvency. In subsequent years, a number of other studies were done and the rankings are now also based on six additional indicators: dealing with construction permits, registering property, protecting minority investors, paying taxes, trading across borders and getting electricity.

While the project has been a tremendous success from a research perspective, with the underlying papers all published in leading journals and the data set used extensively by other researchers, the project has not been without controversy. In particular, the "Employing Workers" component of the survey was heavily criticized by international labor organisations on the grounds that the adoption of potentially exploitative labor practices could boost a country's ease of doing business rank. The World Bank responded to this criticism by removing the employing workers measure from the overall rankings but there remain concerns that the rankings push countries too much towards deregulating sectors without concern for the potential societal or environmental implications.

Another concern relates to the methodology underlying the survey. The project relies on contacting lawyers, accountants, architects, engineers and other professionals to get their estimates of the time and costs for a typical business in the largest business city in a country associated with undertaking various tasks.[4] World Bank (2017) reports that about 60 percent of the data collected for the project is based on a reading of the law. In this sense, the team behind the project argue that the data collected are largely objective and factual. The *Doing Business* team argue this approach is superior to surveying firms directly because they get more objective information from contacting specialist professionals, particularly as many of the issues being surveyed, such as getting hooked up to the electricity grid or going though an insolvency process, are things that only happen very occasionally

[4]Besley (2016) describes the typical firm assumptions in more detail. "The central case is a firm with at least 60 employees, which is located in the country's largest business city. It is a private, limited-liability company and does not operate in an export-processing zone or an industrial estate with special export or import privileges. It is 100 percent domestically owned, and exports constitute more than 10 percent of its sales."

for any individual firm. Still, Hallward-Driemeier and Pritchett (2015) use evidence from the World Bank's Enterprise Surveys of individual firms to show that there is a limited correlation between some of the measures produced by the *Doing Business* project and the experiences actually reported by firms while Kar, Pritchett, Roy and Sen (2019) suggest this correlation gets weaker for states with weak governance institutions. This points to potentially serious weaknesses with the process of data collection for *Doing Business*.

A final concern about *Doing Business* is that it may incentivize governments to "game" the indicators, focusing their reform efforts on items that are measured by the World Bank but which have a limited substantive impact on the economy. For examples, the 2018 report shows Rwanda with a rank of 41st in the world for ease of doing business, ahead of Belgium in 52nd. This may reflect gaming of the indicators rather than it being easier to do business in Rwanda than Belgium. While gaming of this sort may be harmless, some have argued that the project has been damaging because it can focus government energies on the wrong kinds of reforms. For example, Arrunada (2007) reviews the regulations and problems associated with starting a business and argues that *Doing Business* is "promoting sterile reforms while speeding up useless formalities."

The concerns and criticisms noted here may be worth keeping in mind when considering the potentially puzzling empirical results that we obtain in this paper

2.2. Data on Ease of Doing Business

Underlying the well-known rankings for the ease of doing business are a set of scores relating to 50 different indicators from the ten categories listed above. For each of these indicators, the scores run from 0 to 100, with 100 being the best possible value. The scores are constructed on a "distance to frontier" approach, so a score of 100 represents the best value for an indicator in the dataset. The underlying indicators are averaged for each of the ten categories and then an average is taken across the ten categories. It is this aggregate score that is used to construct the rankings. The World Bank *Doing Business 2017* report gives a description of how distance to frontier is constructed for one of the categories, Getting Electricity:

> *For the getting electricity indicators, for example, the frontier is set at three procedures, 18 days and no cost to obtain a new electricity connection in the economys largest business city. The worst for the same group of indicators is set at 9 procedures, 248 days and 81 times the economys income per capita as the cost. In addition, the getting electricity indicators measure the reliability of electricity supply and transparency of tariffs through an index ranging from 0 to 8; in this case 8 is the frontier score. For example, in the case of reliability and transparency, an economy with a score of 6 would be considered to be 75% of the way to the frontier and would have a distance to frontier score of that value.*

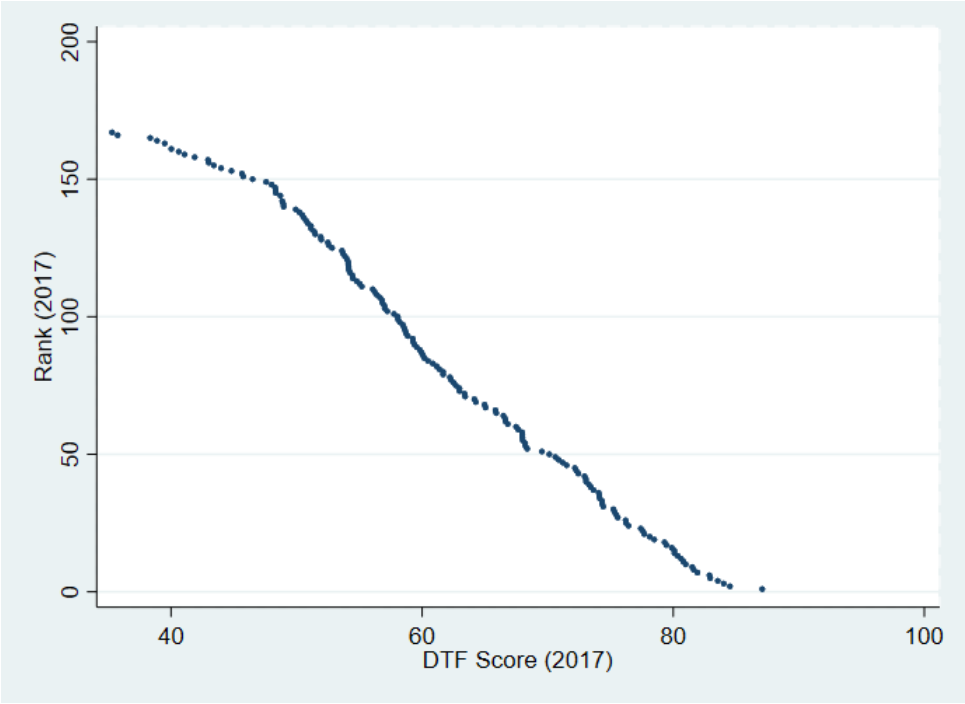
In recent years, the *Doing Business* team has made available all of the underlying distance to frontier (DTF) data as well as the aggregated DTF scores for each country. To give a sense of the magnitudes of the aggregated DTF scores, the average DTF score for 2017 was 62.07 and the standard deviation was 12.21.

Figure 1 illustrates the relationship between the underlying DTF score and the rank using 2017 data. Note that despite its name, the scoring system adopted by the World Bank means that a high "distance to frontier" score represents a good performance rather than a large distance from the frontier. The figure shows the relationship is relatively non-linear so a gain or drop of a particular size in the DTF score can translate into quite different changes in the rank depending on where a country starts out. For example, New Zealand is currently ranked first for ease of doing business with a DTF score of 87.01. If New Zealand's DTF score was to fall by 5, its rank would drop by 10 places. In contrast, China is currently ranked 87th with a DTF score of 64.28. If China's DTF score was to fall by 5, its rank would drop 27 places. We believe this is an argument in favour of using the underlying DTF scores in empirical analysis rather than the rank variable.

Another factor in favor of using the DTF score is that changes in a country's DTF score will be almost completely due to changes in the business environment of the particular country and not due to changes in the frontier. According to the World Bank both the best and the worst performance are established every five years based on the Doing Business data for the year for which they are established and remain at that level for the next five years. In contrast, a country's ease of doing business rank can move around from year to year due to developments elsewhere rather than factors related to its DTF score or underlying changes in the business environment. As we will see, also, the average DTF scores reported by the survey have improved over time. This means that countries where the ease of doing business is not changing are likely to see their rankings worsen over time.

There are, however, two practical problems when seeking to use the data on country-level aggregated DTF scores. The first is that the World Bank has only been published the aggregate DTF score since 2010, even though the DTF scores for the underlying indicators are available going back to as far as 2004 for some. The other problem is that the methodology used by the World Bank to put together the aggregate scores has changed a number of times because new sub-indicators have been added over time. So, for example, in our dataset, there are only three years available for scores constructed according to the most recent methodology.

Figure 1: DTF Score vs Doing Business Rank



To obtain an aggregated DTF score for each country that allows us to use a full panel with the same number of observations as the underlying scores, we constructed a DTF variable that mimics the methodology the World Bank used to construct the aggregate DTF for the three years from 2015 to 2017 inclusive. Specifically, we regressed the aggregate DTF scores as published by the World Bank on nine of the category variables that are available widely during this period (we excluded the Getting Electricity category because of limited data availability). The results of this regression are shown in Table 1. The fitted values from the regression produces a measure that essentially replicates the current global DTF scores, with the R^2 for the regression being about 0.98. We then used the coefficients from this regression to construct a DTF measure from the underlying category scores that is available from 2006 to 2017. This is the explanatory variable that we use in our analysis.

Summary statistics for this measure are shown in Table 2 while Figure 2 shows a histogram of all of values in our sample. An alternative approach of equally weighting each of the underlying categories produces essentially identical results to those we report here.

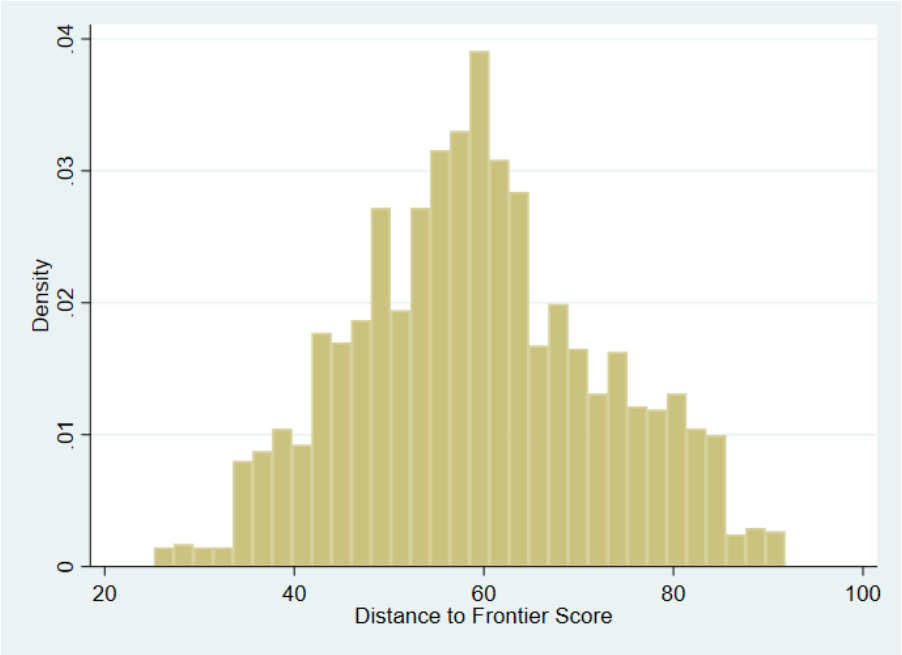
Table 1: Dependent Variable: Distance to Frontier

Variable	**Coefficient**
	(Std. Err.)
Starting a Business	0.098***
	(0.003)
Dealing with Construction Permits	0.127***
	(0.002)
Registering Property	0.105***
	(0.003)
Getting Credit	0.111***
	(0.002)
Protecting Minority Investors	0.090***
	(0.003)
Paying Taxes	0.114***
	(0.003)
Trade across Borders	0.126***
	(0.002)
Enforcing Contracts	0.116***
	(0.004)
Resolving Insolvency	0.099***
	(0.002)
Intercept	0.909***
	(0.260)
N	1320
R^2	0.98

Table 2: **Predicted Distance to Frontier by year**

Year	Mean	N
2006	54.02	165
2007	55.13	165
2008	56.15	165
2009	57.29	165
2010	58.56	165
2011	59.18	165
2012	60.25	165
2013	60.91	165
2014	62.57	165
2015	63.20	165
2016	61.94	165
2017	62.07	165
Total	59.27	1980

Figure 2: Distribution of Distance to Frontier



2.3. Data on Real GDP

Our analysis assesses the relationship between the DTF variable and real GDP per capita. The principal measure of real GDP that we use is taken from the World Bank's World Development Indicators (WDI) database. These data are collected from national statistical organizations and central banks by visiting and resident World Bank missions while data for selected high-income economies are taken from the OECD.

We also report some results using real GDP from the Penn World Tables (PWT) as well as the World Bank's PPP-based measures. Our panel data analysis employs country-specific fixed effects, so our identification of the relationship is based on within-country variation. For this reason, the kind of cross-country real income comparisons for which the PWT measures (and the World Bank's PPP-based measures) were designed are less relevant to our analysis. Johnson et al (2013) argue that the current PWT data are not well-suited to high-frequency analysis using annual data, which is the type of analysis that we are carrying out.

Figure 3 illustrates the relationship between distance to frontier and the WDI measure of real GDP per capita using data from 2017 while Figure 4 substitutes the ease of doing business rank for the DTF score. Figure 5 shows the relationship between the growth in GDP and DTF between 2006 and 2017. All scatter plots show the expected sign. The DTF score is positively correlated with real

GDP per capita, the ease of doing business rank is negatively correlated. The relationship between the cumulative growth in DTF and growth in GDP is weaker but still statistically significant.

Figure 3: GDP Level vs Ease of Business

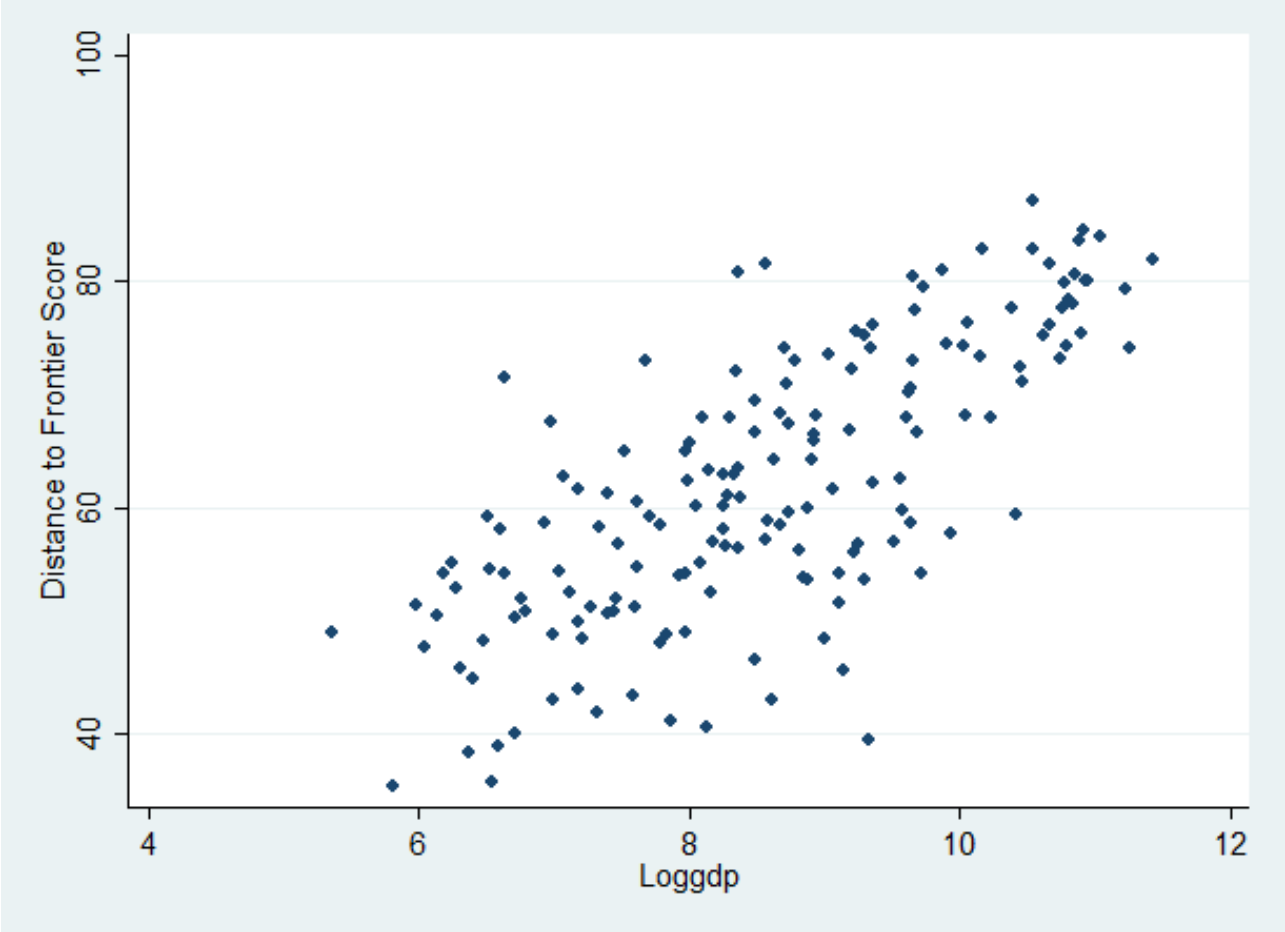


Figure 4: GDP Level vs Doing Business Rank

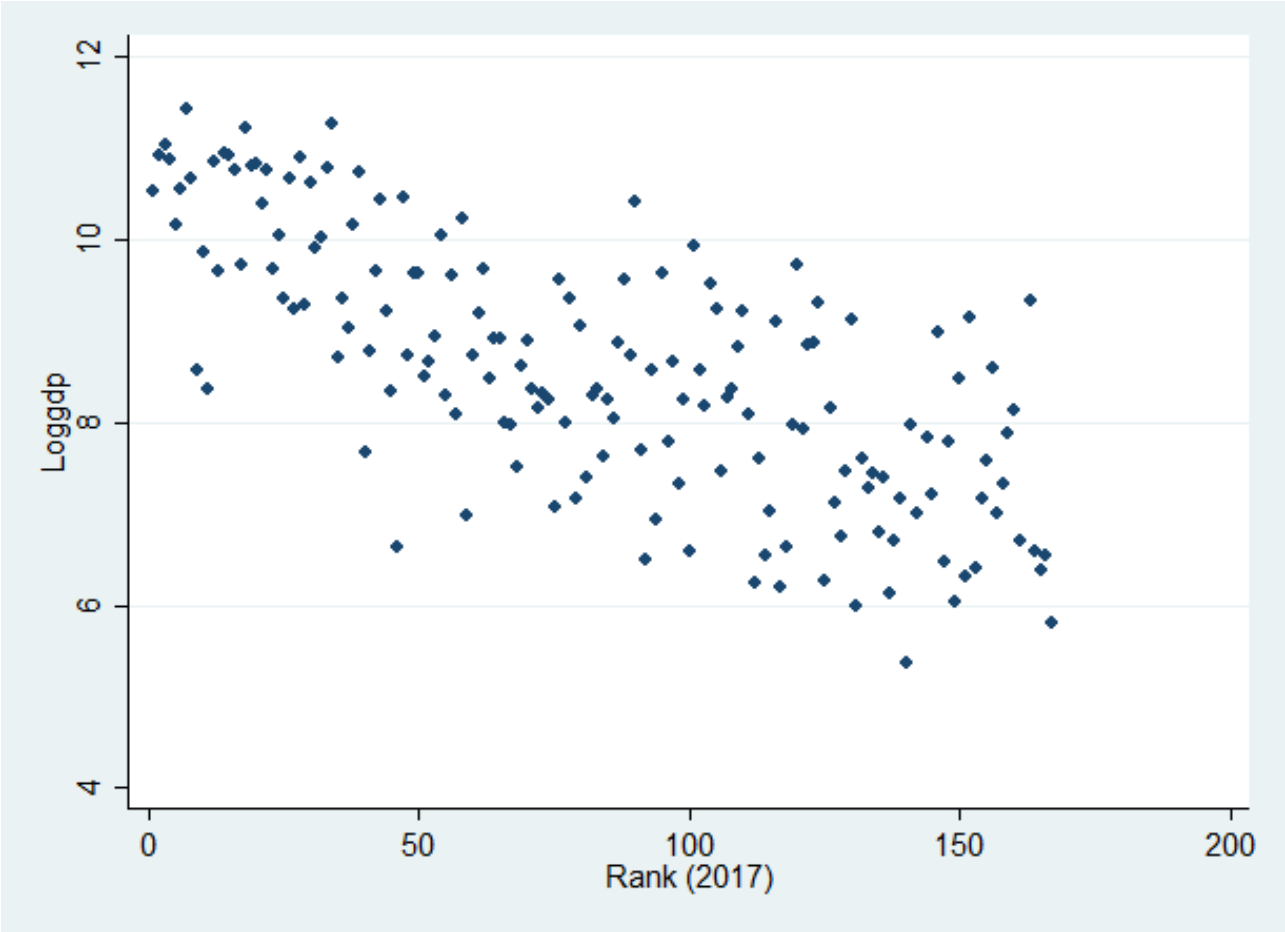
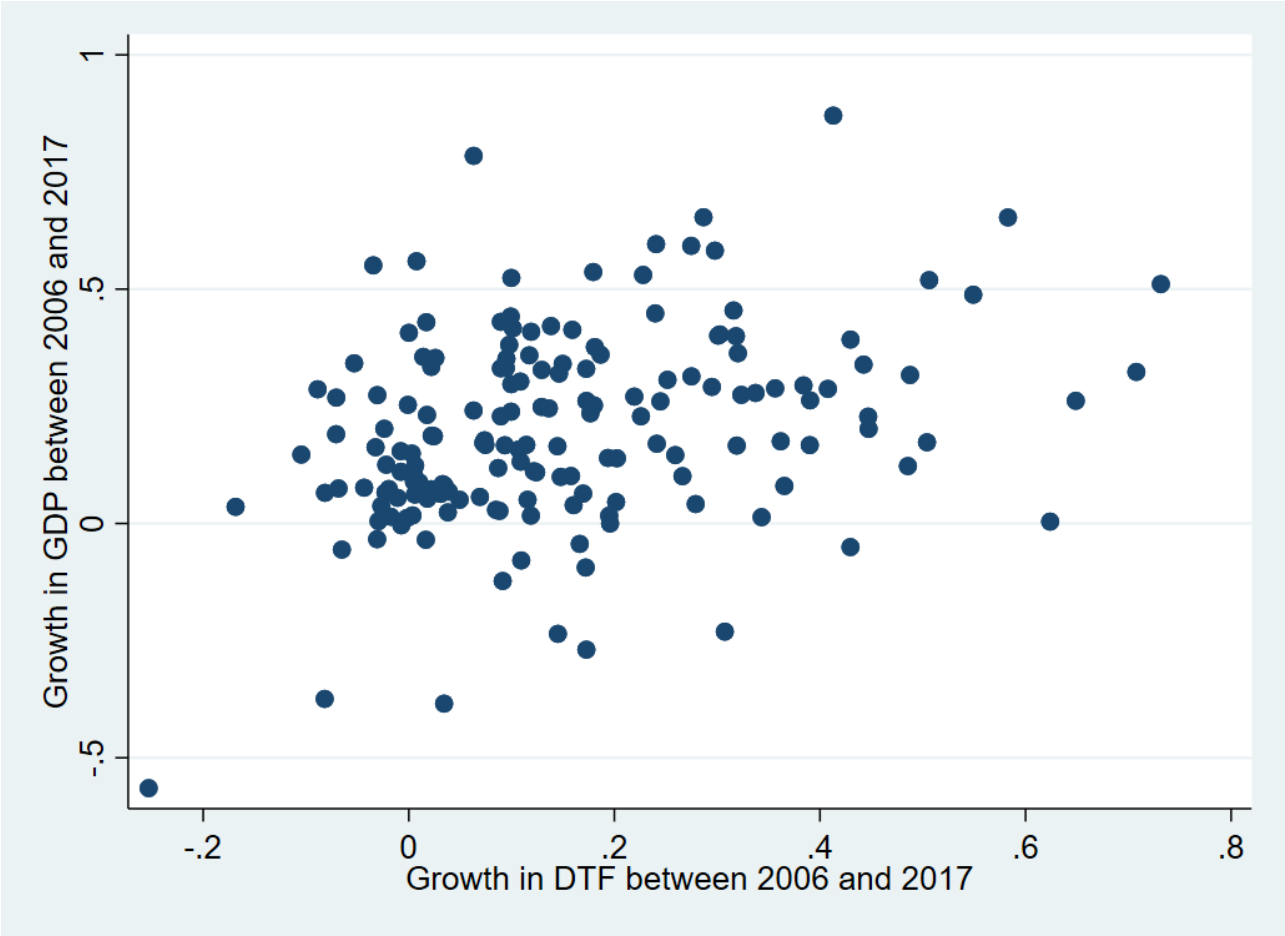


Figure 5: GDP Growth vs Ease of Business Growth



3. Results

The scatter plots just shown fit with intuition for why we might expect to find a relationship between real GDP per capita and the ease of doing business as measured by the DTF score. But a simple cross-sectional scatter plot is not a substitute for a detailed econometric analysis. The cross-sectional relationship may be purely driven by endogeneity, with the causal direction going from GDP to the DTF measure. Alternatively, a relationship that looks strong in a cross-sectional analysis may break down when we look at the time series variation within countries. In this section, we report our initial econometric analysis into this relationship. We use a balanced panel with 165 countries and with 12 years of available data (2006 to 2017) for each country.

3.1. Dynamic Panel Specification

We start by reporting results for the standard dynamic panel specification that has been used extensively in empirical growth analysis since Islam (1995). The specification allows for differences in the long-run levels of GDP for each county by including country-level fixed effects and controls for global economic developments by including time effects. We report a number of specifications including various lags of the log of real GDP and the log of the DTF variable. This gives us a specification of the form:

$$\begin{aligned} \log GDP_{i,t} &= \gamma_i + \mu_t + \beta_1 \log GDP_{i,t-1} + ... + \beta_n \log GDP_{i,t-n} \\ &\quad + \delta_0 \log DTF_{i,t} + ... + \delta_n \log DTF_{i,t-n} + \epsilon_{i,t} \end{aligned} \tag{1}$$

Because both variables have been logged, the coefficients can be interpreted as elasticities.

The results from estimation of this specification via OLS are shown in Table 3. Column 1 shows there is still a positive estimated relationship between DTF and real GDP per capita even after controlling for time and country-level fixed effects. This suggests that the relationship between the two variables is not simply a cross-sectional relationship and that the within-country variation in DTF, which is what is being used here to identify this coefficient, has the expected positive relationship with GDP. However, once we include some dynamics the picture is less clear. Including one lagged value of DTF of GDP per capita, we estimate a large negative coefficient on the first lag of DTF which is larger than the contemporaneous positive coefficient. When second lagged values are added, we find a positive coefficient on the second lag of DTF. Third lags are not found to be statistically significant.

Table 4 summarizes the predictions for the long-run effect of DTF on GDP by reporting the same specifications re-written to show the sum of the coefficients on the DTF terms. Depending on the dynamic specification chosen, DTF has either a significant positive effect (no lags), a significant negative effect (one lag) or an insignificant negative effect (two lags).

Table 3: Estimating the Effect of DTF on Real GDP Per Capita

	(1)	(2)	(3)
	Per Capita GDP	Per Capita GDP	Per Capita GDP
GDP_{t-1}		0.894***	1.077***
		(0.011)	(0.249)
GDP_{t-2}			-0.222***
			(0.025)
DTF_t	0.272***	0.109***	0.115***
	(0.027)	(0.035)	(0.027)
DTF_{t-1}		-0.149***	-0.209***
		(0.036)	(0.039)
DTF_{t-2}			0.084*
			(0.028)
N	1980	1815	1650
F	3754	1275.7	6095.21

standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Estimating the Effect of DTF on Real GDP Per Capita

	(1)	(2)	(3)
	$\log \text{GDP}_t$	$\log \text{GDP}_t$	$\log \text{GDP}_t$
$\log GDP_{t-1}$		0.894***	1.077***
		(0.0111)	(0.0249)
$\log GDP_{t-2}$			-0.222***
			(0.025)
$\log DTF_t$	0.272***	-0.039***	-0.009
	(0.027)	(0.014)	(0.016)
$\Delta \log DTF_t$		0.149***	0.125***
		(0.027)	(0.028)
$\Delta \log DTF_{t-1}$			-0.084
			(0.029)
N	1980	1815	1650
F	3754	18784	17996

Notes: Model estimated via OLS with time and country dummies.

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

3.2. VAR Specification

As with the the positive cross-sectional relationship between real GDP per capita and the DTF score, one reason to potentially distrust the positive contemporaneous coefficients estimated in Table 3 is that there is likely to be an endogeneity issue. The higher the average living standard in a country is, the more likely it is to have a more powerful set of political and social forces favouring business-friendly policies and so any estimated positive relationship may simply reflect causality running from real GDP to the policies summarised by the DTF variable.

This endogeneity has been recognised widely in the literature that attempts to link economic policies and institutions with levels of GDP or long-run growth rates. However, this literature has focused largely on cross-sectional analysis, so the proposed solution to the endogeneity problem has been to construct exogenous instruments to assess the effects of the explanatory policy variable. In the case of the ease of doing business rank, for example, Djankov, McLiesh and Ramalho (2006) use instruments such as dummy variables for the origin of the country's legal institutions and principal religion as well dummy variables such as distance from the equator in their analysis of the impact of the doing business rank on long-run growth. Gillanders and Whelan (2014) use the following variables as instruments for the ease of doing business rank in their analysis of the relationship between the real and the level of real GDP per capita: Distance from the equator, a landlocked dummy, a measure of the suitability of the soil for rain fed crops and the proportion of land that is suitable for irrigated rice crops.

Within the confines of a purely cross-sectional analysis, it is clear that some kind of instrumental variable approach has to be adopted but the existing literature in this area has some important weaknesses. In many cases, the variables chosen as instruments have only very weak correlations with the policy variables being instrumented and these low-first stage fits are well-known to lead to unreliable inference.[5] The cross-sectional analysis is also, by definition, unable to answer important questions about the impacts over time of policy changes: If, for example, a country introduces a set of reforms that improve its ease of doing business scores, over what time period can it expect these changes to impact its GDP? The cross-section estimates are best viewed as giving the long-run effects with no information of the short- or medium-term dynamics.

To address these weakness, we employ a panel Vector Autoregression approach and report the impulse responses of real GDP per capita to shocks to the DTF score. The basic VAR specification consists of estimating two dynamic equations, one for the log of real GDP per capita and one for the

[5] See, for instance, Staiger and Stock (1997).

log of the DTF variable.

$$\begin{aligned}\log GDP_{i,t} &= \gamma_i^{GDP} + \mu_t^{GDP} + \beta_{11}\log GDP_{i,t-1} + ... + \beta_{1n}\log GDP_{i,t-n} \\ &\quad +\delta_{11}\log DTF_{i,t} + ... + \delta_{1n}\log DTF_{i,t-n} + \epsilon_{i,t}^{GDP} \qquad (2)\\ \log DTF_{i,t} &= \gamma_i^{DTF} + \mu_t^{DTF} + \beta_{21}\log GDP_{i,t-1} + ... + \beta_{22n}\log GDP_{i,t-n} \\ &\quad +\delta_{21}\log DTF_{i,t} + ... + \delta_{2n}\log DTF_{i,t-n} + \epsilon_{i,t}^{DTF} \qquad (3)\end{aligned}$$

Testing for lag lengths, we select the best-fitting specification as the one with 2 lags, meaning our VAR has an effective T of 10 but the key results reported here are also obtained with different lag lengths. Table 5 reports the coefficients from the two regressions that make up our estimated VAR.

The approach to identification of shocks in a VAR analysis relies on the assumptions made about the relationships between the reduced-form residuals $\epsilon_{i,t}^{GDP}$ and $\epsilon_{i,t}^{DTF}$ and an assumed underlying set of structural shocks. In our analysis, we follow the Cholesky decomposition approach and place the log of real GDP per capita first in the ordering of shocks. This means we are assuming the contemporaneous correlation between $\epsilon_{i,t}^{GDP}$ and $\epsilon_{i,t}^{DTF}$ is fully driven by a causal link going from GDP to DTF. With this assumption, we can then analyse the impact on GDP of that component of $\epsilon_{i,t}^{DTF}$ which is uncorrelated with $\epsilon_{i,t}^{GDP}$.

The assumption that contemporaneous correlation between $\epsilon_{i,t}^{GDP}$ and $\epsilon_{i,t}^{DTF}$ is fully driven by the causal link from GDP to DTF may seem strong but there is merit in focusing on the dynamic effects of those shocks that are uncorrelated with contemporaneous changes in GDP. The effects of changes in business regulations are likely to emerge over time rather than being quickly identifiable short run impacts. Moreover, while a regression of $\epsilon_{i,t}^{DTF}$ on $\epsilon_{i,t}^{GDP}$ reveals a strong statistical relationship (a t-statistic of 4.38) the structural DTF shocks we are examining still account for the vast majority of the random variation in the DTF series.

The estimated impulse-response functions for one standard deviation shocks of both types are shown as the black lines in Figure 6. The lines above and below are 10% and 90% confidence intervals generated using 1000 bootstrap replications of the estimated VAR model. We use bootstrapped standard errors because the time element of our sample is small and error terms in this kind of cross-sectional dataset tend to be heteroskedastistic and non-normal, thus making alternatives such as asymptotic standard errors or Monte Carlo error bands less accurate.[6] Given the limited time series dimension of our dataset, we limit ourselves to showing estimated responses up to seven years after the initial shock as it would be unwise to make judgments on very long-run dynamics based on such a short dataset.

The left-hand graphs in Figure 6 show the response of GDP and DTF to one-standard deviation

[6]Each draw of the bootstrap residuals draws from the same historical time period so the bootstrapped residuals contain the same correlations across countries and for the two variables within countries as in the historical data.

shocks to GDP while the right-hand graphs show the responses to one-standard deviation shocks to DTF. As expected, a positive shock to GDP provides a boost to DTF over the time horizon we are examining. More interesting is the upper-right graph, which shows the dynamic impact on GDP of a shock to DTF. Since this is our main point of interest, we have shown this graph on its own as Figure 7. The results are fairly striking. By definition, the DTF shock has no impact on GDP in period zero (when the shock occurs) but it then has a negative impact that is estimated to only gradually fade away over time. In terms of magnitudes, we have multiplied the series by 100, so we can see from the bottom right chart in Figure 6 that the one-standard deviation shock to DTF is about 3 percentage points in size. The point estimate of the maximum reduction in GDP is estimated at 0.37 percentage points four periods after the shock, albeit the bootstrapped confidence intervals suggest a wide amount of uncertainty around this figure.

By definition, all shocks in this model are temporary, so the improvement in DTF modelled here fades away over time. However, one can use the estimated VAR coefficients to calculate the theoretical impact of a permanent shock to DTF of the same size as the initial shock modelled here. Figure 8 shows that a permanent improvement of 3 percentage points in DTF is estimated to be associated with a reduction in GDP of about 0.55 percentage points.

Table 5: VAR Regressions

	(1)	(2)
	$\log$ GDP$_t$	$\log$ DTF$_t$
GDP_{t-1}	1.085 ***	0.066***
	(0.025)	(0.023)
GDP_{t-2}	-0.226***	-0.029
	(0.025)	(0.023)
$\log DTF_{t-1}$	-0.095***	0.983 ***
	(0.028)	(0.026)
$\log DTF_{t-2}$	0.069**	-0.134***
	(0.028)	(0.027)
N	1980	1980

Notes: Model estimated with time and country dummies.

Notes: Standard errors in parenthesis.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 6: Impulse Responses to One Standard Deviation Shocks (Includes Bootstrapped 10% and 90% Confidence Intervals)

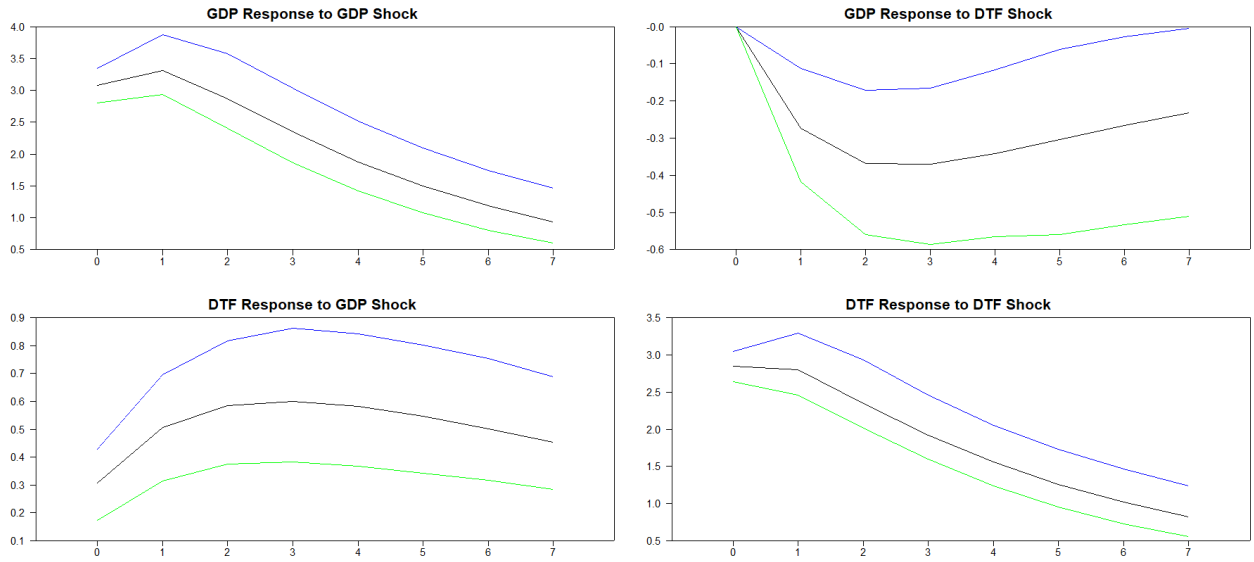


Figure 7: Response of the Level of GDP to a One Standard Deviation DTF Shock (Baseline Specification, Includes Bootstrapped 10% and 90% Confidence Intervals)

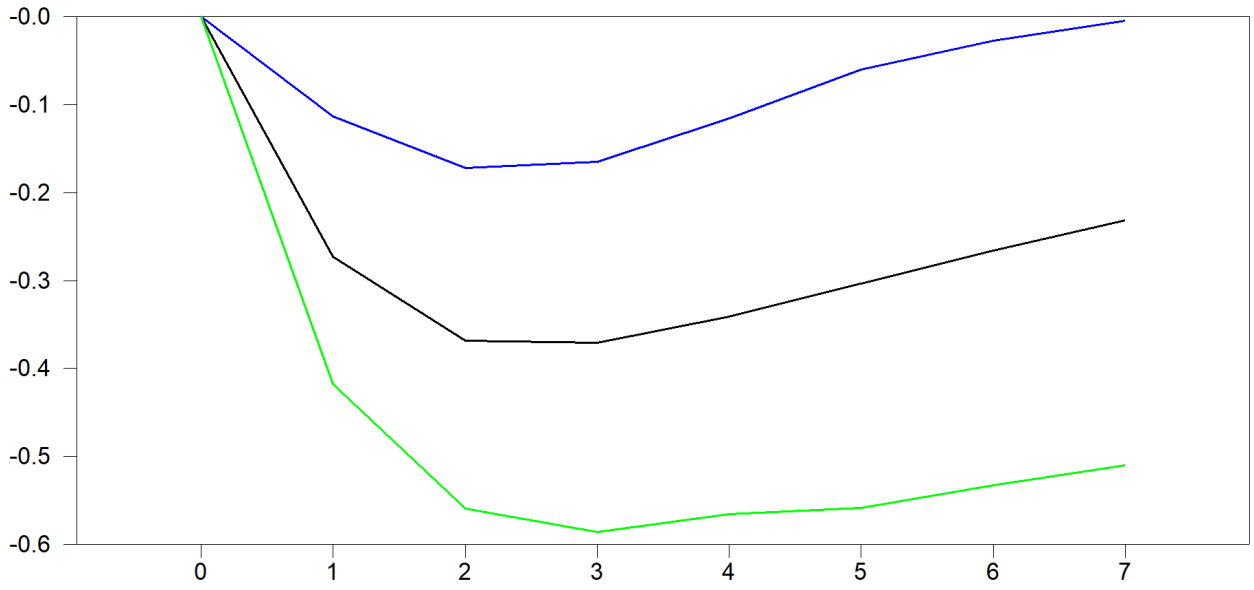
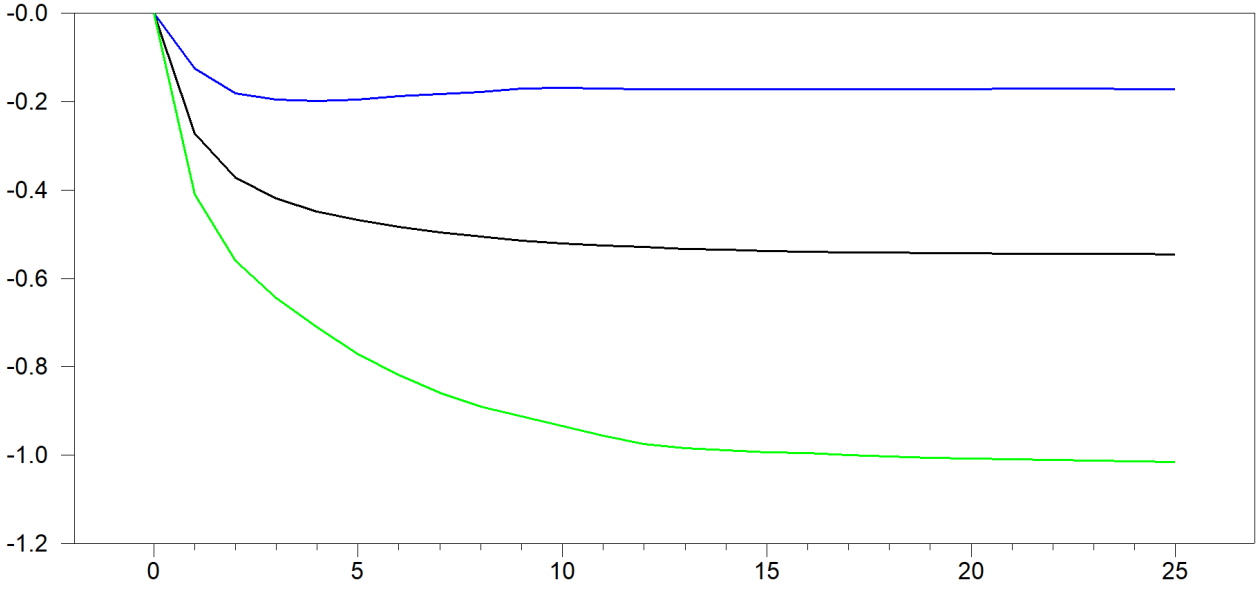


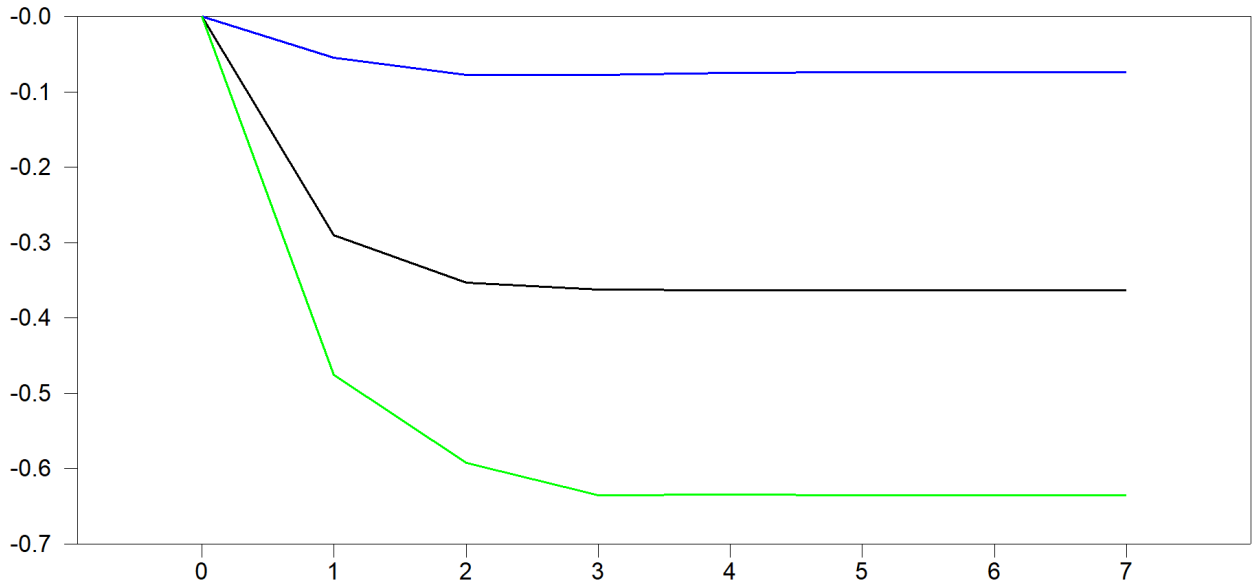
Figure 8: Response of the Level of GDP to a Permanent One Standard Deviation DTF Shock (Baseline Specification, Includes Bootstrapped 10% and 90% Confidence Intervals)



3.3. Growth Rate Specification

Our estimated model assumes both variables are stationary around a trend determined by time effects. It is possible that the correct underlying model for the log of real GDP has a unit root and that perhaps the specification should best be estimated in first-difference form. We performed the panel unit root tests of Levin, Lin and Chu (2002) on both variables and these suggest strong rejections of the null that either series have unit roots. But given the relatively low power of these kinds of tests with small T samples, we decided to estimate the model in first-difference form, thus featuring the growth rates of real GDP per capital and DTF. For comparability, Figure 9 reports the estimated responses to the levels of log GDP per capita to a shock to DTF, obtained by cumulating the impulse responses for the first-difference specification. The magnitude of the estimated decline in GDP is similar to that estimated in the baseline VAR though because of the difference in specifications, it is estimated to be a permanent effect.

Figure 9: Response of the Level of GDP to a One Standard Deviation DTF Shock (Growth Specification, Includes Bootstrapped 10% and 90% Confidence Intervals)



3.4. Arellano-Bond Estimation

Beyond the possibility of unit roots, it is well known that OLS estimates of dynamic panel regressions suffer from biases when T is small. The presence of both country-specific fixed effects and lagged endogenous variables means there are a series of correlations between the error terms and explanatory variables which violate the traditional assumptions required for fixed effect OLS to be an unbiased estimator. As discussed in detail by Bond (2002), there are no easy fixes for these problems, particu-

larly in cases where you have highly persistent dependent variables such as the per capita GDP series used in our paper. One popular method, however, is the Arellano-Bond estimator. This estimator is based on first-differencing the basic specification and then constructing a series of instruments based on the moment conditions which state that that error terms should not be correlated with lagged levels of the explanatory variable.

Table 6 reports the estimation of our VAR specification using the Arellano-Bond method. These estimates were then used to repeat our Cholesky decomposition method and estimate the impulse reponse of GDP per capital to DTF. To maintain comparability, we estimated the impact of a unit shock to GDP and then scaled the responses using the same standard deviation of DTF shocks reported in Figure 6. As shown in Figure 10, these estimates are relatively similar to our baseline estimates in also showing a short-run negative response, though this effect fades away somewhat quicker.

Table 6: Arellano-Bond Estimation of the VAR Model

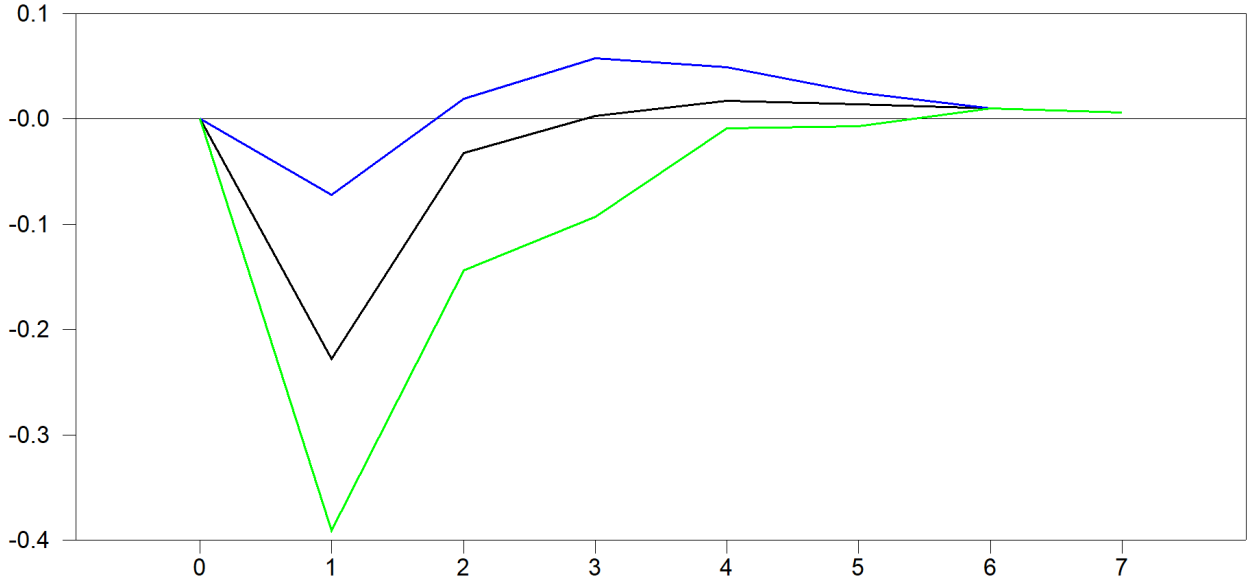
	(1)	(2)
	$\log \text{GDP}_t$	$\log \text{DTF}_t$
GDP_{t-1}	0.706***	0.071***
	(0.033)	(0.014)
GDP_{t-2}	-0.087***	0.037 **
	(0.013)	(0.012)
$\log DTF_{t-1}$	-0.080***	0.196 ***
	(0.023)	(0.012)
$\log DTF_{t-2}$	0.061**	0.091***
	(0.019)	(0.014)
N	1485	1485

Notes: Model estimated with time and country dummies.

Standard errors in parentheses

$^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

Figure 10: Response of the Level of GDP to a One Standard Deviation DTF Shock (Arellano-Bond Estimation, Includes Bootstrapped 10% and 90% Confidence Intervals)



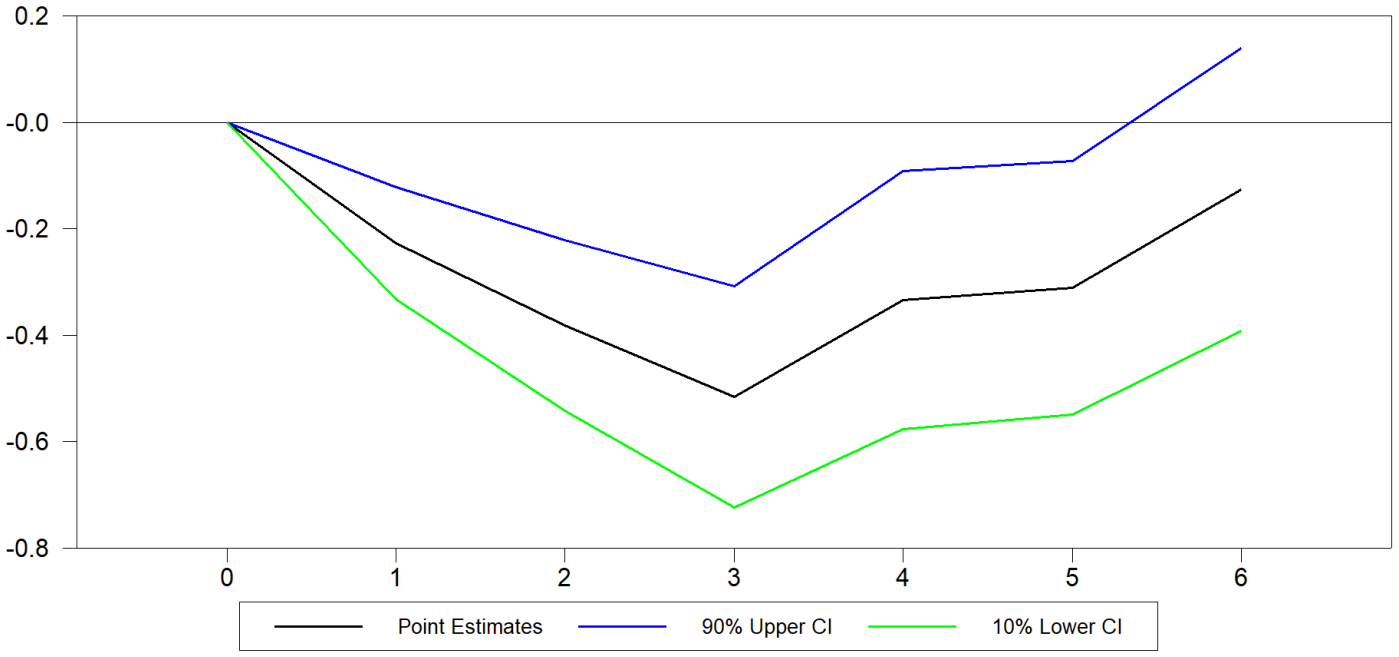
3.5. Local Projection Estimates

The local projection methodology introduced by Jorda (2005) has been commonly used in recent years to estimate impulse responses without having to take a stance on all the parameters governing the dynamics of the variables at hand. We employed this method to estimate impulse responses to a DTF shock as follows. For $j = 1, ..6$, we estimated

$$\log GDP_{i,t+j} = \gamma_i + \mu_t + \beta^j \log DTF_t + \sum_{k=1}^{2} \theta \log DTF_{t-1} + \sum_{k=0}^{2} \theta \log GDP_{t-1} \tag{4}$$

and then use β^j to measure the impulse response to a DTF shock after j periods. The inclusion of contemporanous GDP in the regression specification means, as in our baseline VAR, we are not attributing any contemporaneous effect on GDP of changes in DTF. The estimated standard errors on β^j are used to construct confidence intervals of the same size as the bootstrapped intervals shown earlier. The results are shown in Figure 11. This is not our preferred method for estimating these impulse responses. With a T of 12, we can only use a small fraction of the time dimension of the data set to estimate the longer-dated impulse responses. It is also likely that the standard errors from these regressions are inaccurate due to small sample bias problems. However, the size and pattern of the estimates are similar to those obtained from our baseline VAR.

Figure 11: Response of the Level of GDP to a One Standard Deviation DTF Shock (Local Projection Estimation, Model Includes Two Lags of GDP and DTF, Includes 10% and 90% confidence intervals based on estimated Newey-West standard errors)



3.6. Alternative Measures of Real GDP

One possible explanation is that we are using an inadequate measure of real GDP. It is well known, for example, that movements in the real GDP measures in the WDI do not always correlate well with the movements in the real GDP measures produced by the Penn World Tables.[7] Our measure of GDP does not attempt to make any PPP-related adjustments. Table 7, however, shows that the coefficients from the regression for real GDP in VAR are similar when using two alternative measures: real GDP from the Penn World Tables and the WDI's PPP-adjusted measure of real GDP. The key driver of the negative short-run effect is of a shock to DTF is the negative coefficient on the first lag of DTF and this finding is robust across all three measures. Figures 12 and 13 repeat the VAR impulse response analysis for the impact of DTF shocks on real GDP using the two different measures of real GDP. Both charts back up the conclusions from the previous analysis that shocks to DTF are followed by a negative impact on GDP.

3.7. Using Rank Instead of DTF

We described a number of reasons above why we believe the DTF measure we are using is more appropriate for a panel data analysis the impact of changes in the ease of doing business. The DTF scores are the true indicators underlying the rankings and, as such, one could consider the rankings to be a transformation of the original data that obscure information rather than assist.

Still, one could be suspicious that our specially-constructed DTF time series is somehow responsible for the results that we have found. Figure 14 shows, however, that if we repeated our analysis using the ease of doing business rank that has been used in other studies, the same results are effectively obtained. A one-standard deviation positive shock to a country's *Doing Business* rank (meaning the country's DTF score is worsening) produces an estimated temporary improvement in real GDP, with the estimated magnitudes being similar to those estimated with the DTF analysis.

[7] See Ram and Ural (2014)

Table 7: VAR Regression for $\log GDP$ with Alternative Measures of GDP

	(1)	(2)	(3)
	$\log$ GDP$_t$	$\log$ GDP$_t$	$\log$ GDP$_t$
	WDI	PPP	PWT
$\log GDP_{t-1}$	1.085***	1.100***	0.979***
	(0.025)	(0.025)	(0.033)
$\log GDP_{t-2}$	-0.226***	-0.241***	-0.177***
	(0.024)	(0.024)	(0.045)
$\log DTF_{t-1}$	-0.095***	-0.096***	-0.220***
	(0.028)	(0.028)	(0.047)
$\log DTF_{t-2}$	0.069**	0.068**	0.183***
	(0.028)	(0.029)	(0.044)
N	1980	1980	1188

Notes: Model estimated with time and country dummies.

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 12: Impulse Responses of GDP to a One Standard Deviation Shock to DTF Using PPP-Adjusted GDP (Includes Bootstrapped 10% and 90% Confidence Intervals)

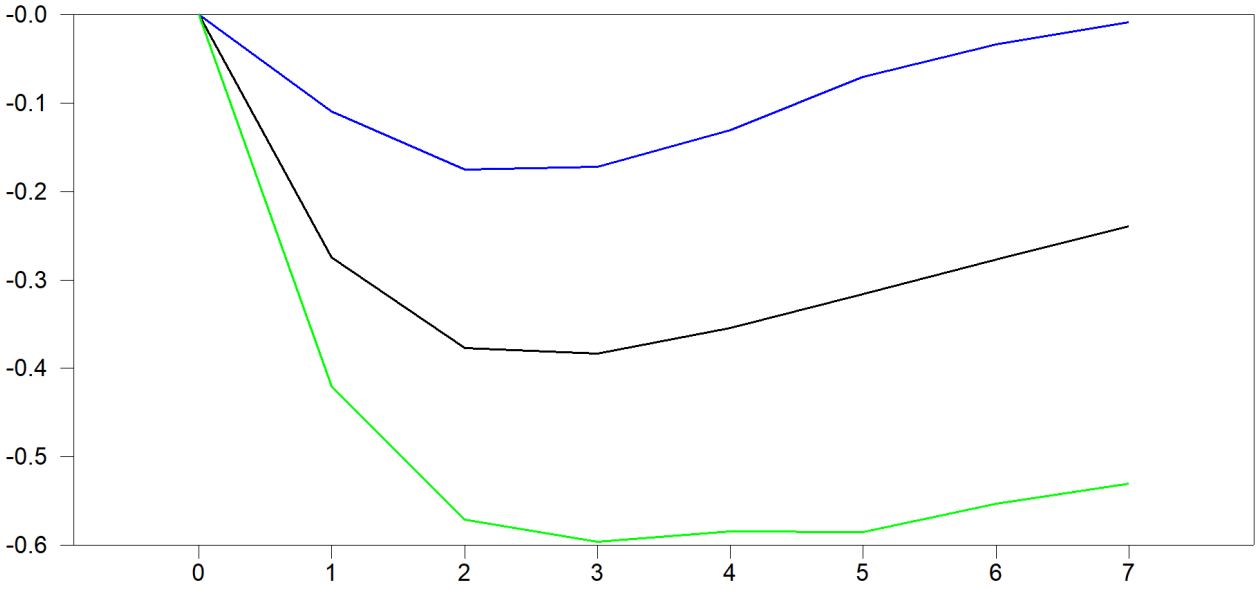


Figure 13: Impulse Responses of GDP to a One Standard Deviation Shock to DTF Using Penn World Tables GDP (Includes Bootstrapped 10% and 90% Confidence Intervals)

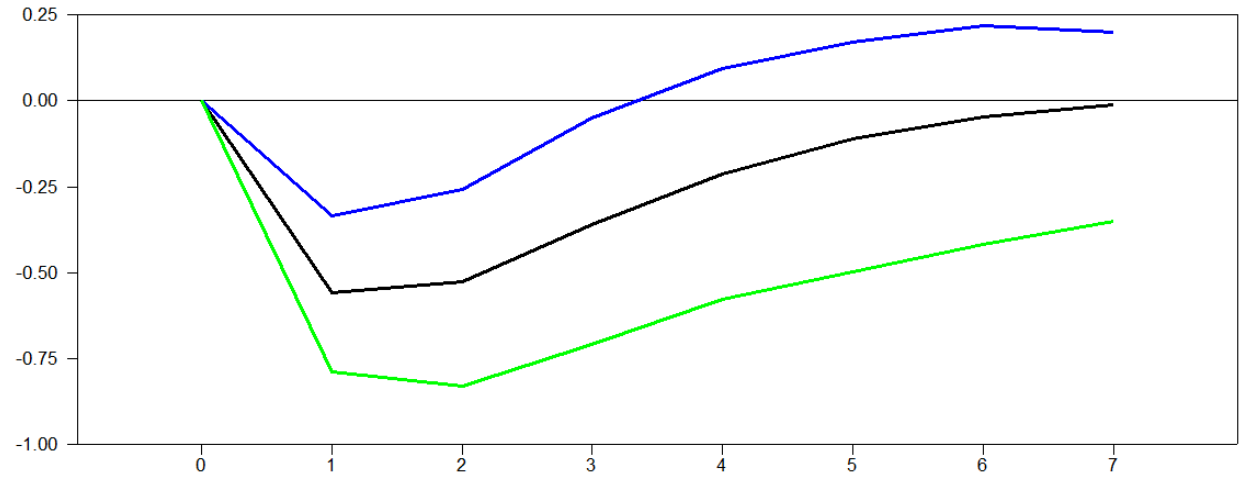
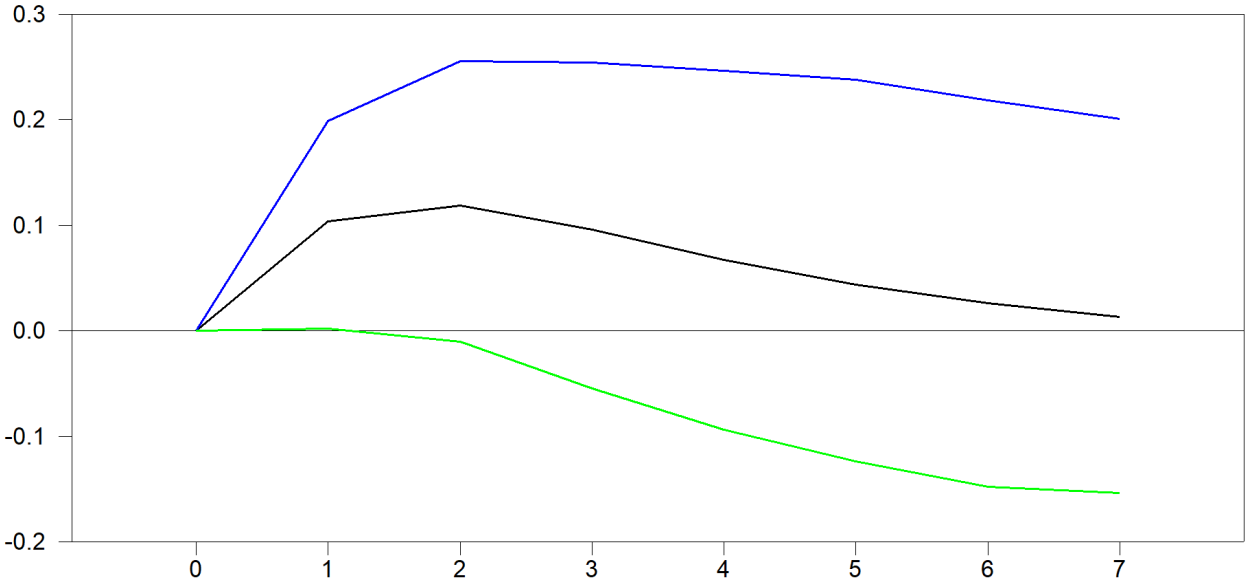


Figure 14: Impulse Responses of GDP to a One Standard Deviation Shock to Doing Business Rank (Includes Bootstrapped 10% and 90% Confidence Intervals)



3.8. Effect on Components of GDP

One question raised by our results is how are they reflected in the reponse of the various sub-components of GDP. One possible explanation for the negative effects of improvements in DTF could be that business-friendly reforms tend to be introduced by conservative governments that are simultaneously cutting public expenditure, so there is a short-run negative impact on GDP due to Keynesian aggregate demand effects. To assess whether this is what is driving the results, Figure 15 shows the results from running our VAR model replacing GDP with expenditure sub-components for consumption, investment and government spending on goods and services. The results do show a point estimate of an immediate reduction in government spending but this is not statistically significant and this is followed by a significant increase in government spending a few years later. The response of investment is estimated to be positive but statistically insignificant. Thus, our results are driven by a negative impulse response of consumption to improvements in DTF. This itself will of course be driven by the overall reduction in GDP, so no clear causal story emerges from looking at these sub-components.

3.9. Alternative Cholesky Decomposition

Recognizing the likely seriousness of the endogeneity problem whereby higher levels of GDP per capita likely improve *Doing Business* scores, we have focused on shocks to DTF that are uncorrelated contemporaneously with shocks to GDP. It is possible that this methodology understates the impact of improvements in the ease of doing business because it discounts potential positive contemporaneous impacts. For completeness, we thus report the results for the alternative identification—with DTF ordered first and the contemporaneous correlation being attributed to DTF immediately impacting GDP—in Figure 16.

As expected, given the contemporaneous correlation of the residuals, the estimated impact of the effect of an improvement in DTF is positive and statistically significant. However, the point estimates for the second and subsequent periods are negative, though not statistically significant. This suggests that even if we can interpret the contemporaneous correlation between GDP and changes in the *Doing Business* as a causal effect running from the DTF score to GDP, the short-run positive impact seems to disappear quickly.

3.10. Using Sub-Components of the DTF Score

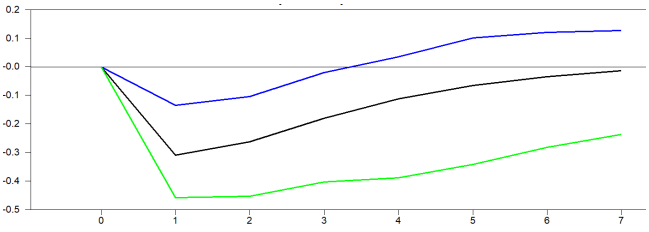
We have also repeated our analysis by replacing the overall DTF score with its various sub-components to assess whether our puzzling findings are driven by particular components of the overall measure. Figure 17 reports that point estimates for impulse responses to improvements in the sub-component indicators are negative over our horizon of seven years for almost all of the indicators. For four of these sub-components, the confidence intervals suggest significant negative effects at various horizons, while the other negative point estimates are not reported as statistically significant.

3.11. Sub-Samples of Different Countries

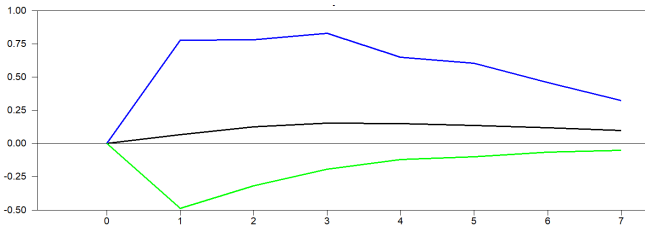
An obvious question is whether our results are driven by a particular sub-set of the countries our sample. One quick way to assess this possibility is to split the sample according to GDP per capita. We have split the sample into quartiles based on the level of initial GDP per capita in the sample. Figure 18 reports the results for the bottom, second, third and top quartiles. Only for the top quartile of income levels do we see evidence of a clear positive impact of improvements in DTF. For the other samples, the point estimates are mainly negative, though there are fewer statistically significant effects relative to the batch of full-sample estimates reported above. This is to be expected because the bootstrap methodology for constructing error bands replicates the underlying sampling distribution and with smaller samples there will be more sampling-related error.

We also considered other country-related sample splits, such as dividing countries according to their scores on the World Bank's Governance Matters indicators or Transparency International's corruption perceptions index but these splits did not show any systematic patterns.

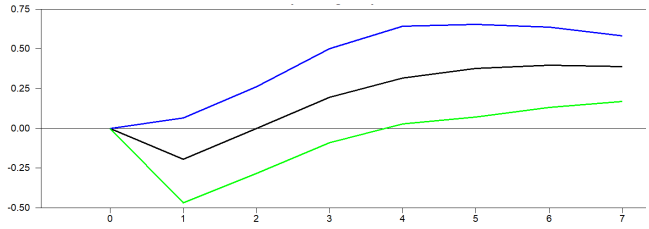
Figure 15: **Impulse Responses of components of GDP to a One Standard Deviation Shock to DTF (Includes Bootstrapped 10% and 90% Confidence Intervals)**



(a) Effect on Consumption



(b) Effect on Investment



(c) Effect on Government Spending

Figure 16: Impulse Responses of GDP to a One Standard Deviation Shock to DTF Using Alternative Causal Ordering with DTF Ordered First (Includes Bootstrapped 10% and 90% Confidence Intervals)

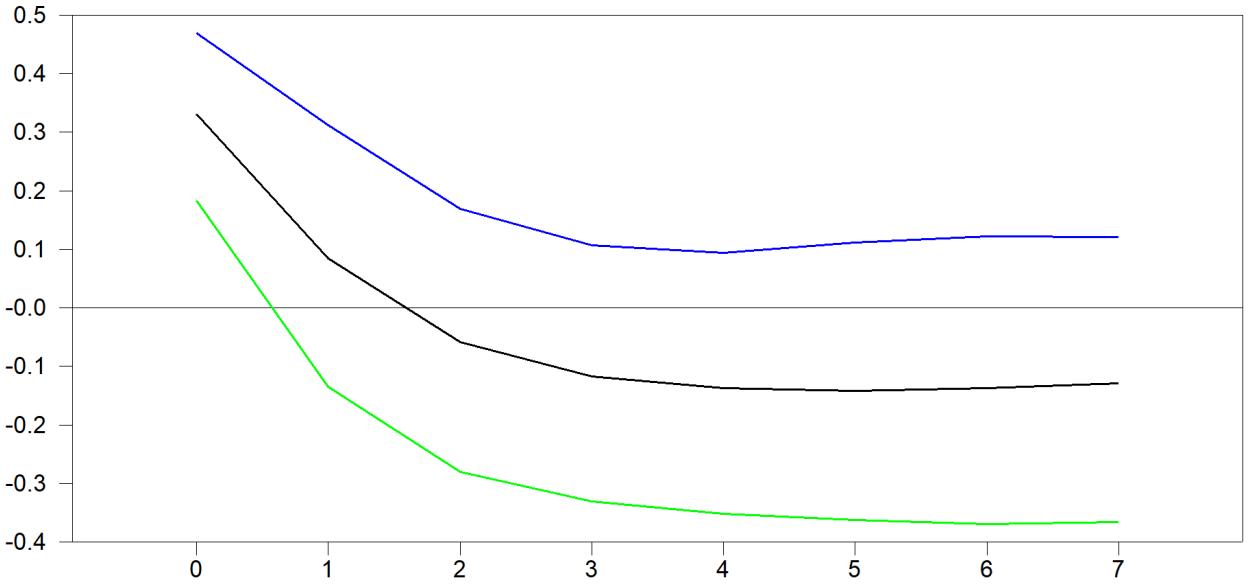
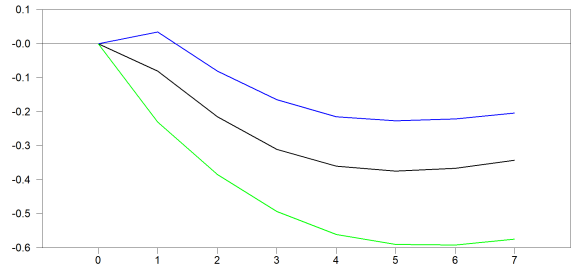
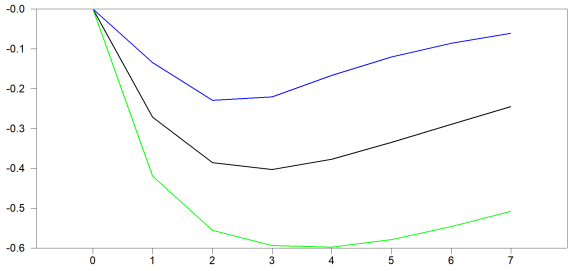


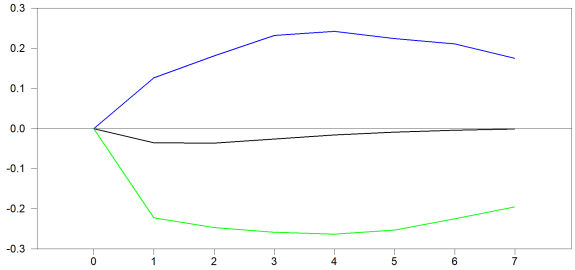
Figure 17: **Impulse Responses of GDP to a One Standard Deviation Shock to Sub-components of Doing Business (Includes Bootstrapped 10% and 90% Confidence Intervals)**



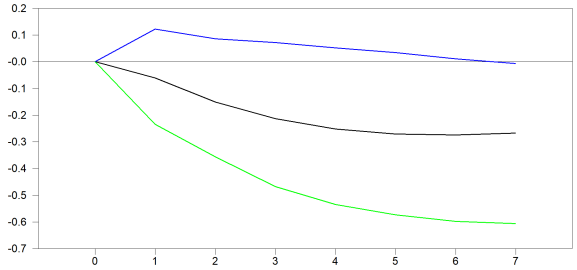
(a) Starting a Business



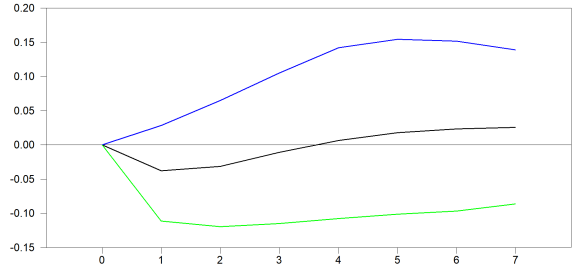
(b) Paying Taxes



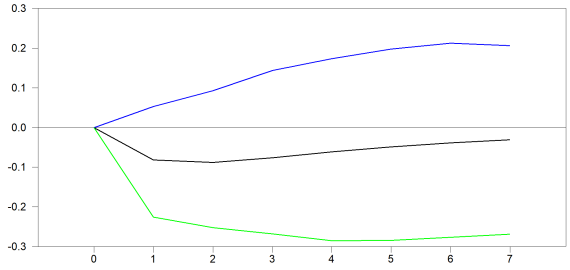
(c) Ease of Trade



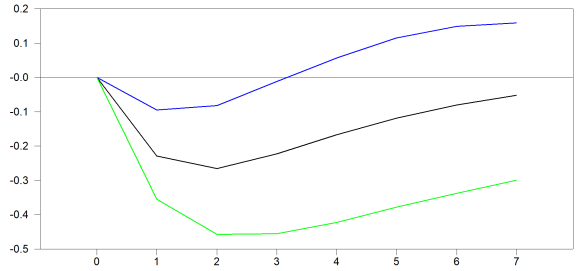
(d) Resolving Insolvency



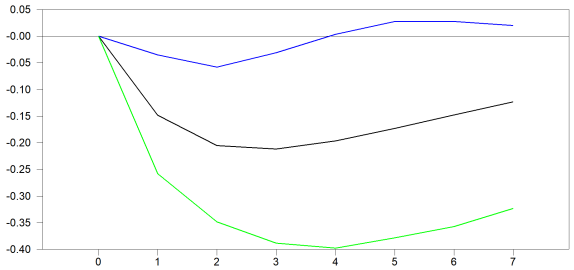
(e) Protecting Minority Investors



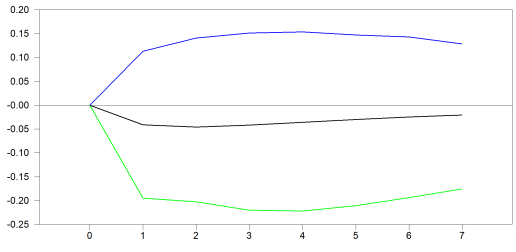
(f) Ease of getting construction permits



(g) Enforcing Contracts

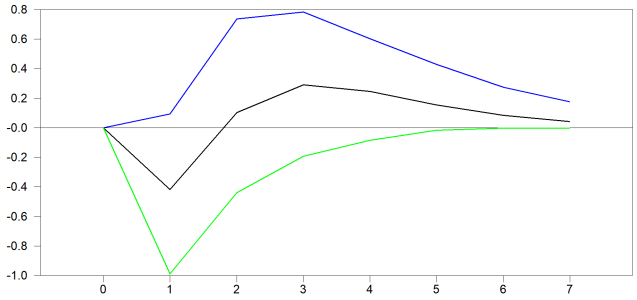


(h) Ease of getting credit

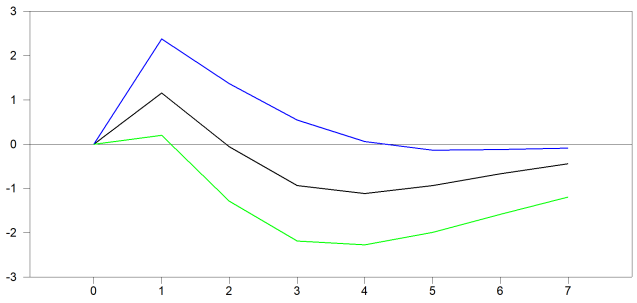


(i) Registering Property

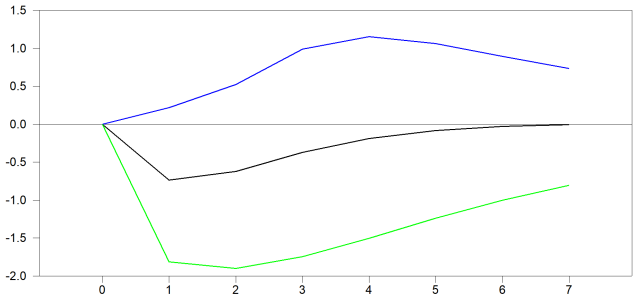
Figure 18: **Impulse Responses of GDP to a One Standard Deviation Shock to DTF for four Quartiles of GDP Per Capita (Includes Bootstrapped 10% and 90% Confidence Intervals)**



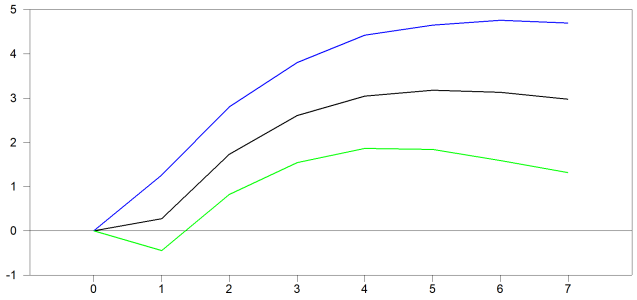
(a) Bottom Quartile



(b) Second Quartile



(c) Third Quartile



(d) Top Quartile

4. Conclusions

In light of previous findings, we find the results reported here to be somewhat surprising. However, we believe the methodology of using within-country time series variation to assess the effect of changes in the ease of doing business is a valid one and the results turn out to be robust across a wide range of different specifications, data types and estimators.

This raises an obvious question: Why do we find that improvement in the ease of doing business appear to have had a negative effect on GDP? One possibility is that the widespread focus in the developing world on the *Doing Business* indicators has perhaps had a negative effect, with those countries that have had the best improvements in their DTF scores being countries that have focused on box-ticking exercises to improve their ranking rather than substantive reforms. Another possibility is that implementing improvements in business environment takes take time to have a positive impact—more time than the short time element in our analysis can pick up—and our findings are picking up some shorter-run disruptions that stem from reforms that ultimately have a positive effect. A final possibility is that the *Doing Business* indicators do not do a very good job at measuring the ease of doing business. As noted above, there is a relatively low cross-sectional correlation between the *Doing Business* indicators and comparable estimates from occasional surveys such as the World Bank's *Enterprise Surveys*.

A final important qualifier to our results is that they clearly can only be considered representative of the sample that we have examined here. It may simply be that the period under investigation in this paper is unrepresentative of the general relationship between the business environment. Certainly, we believe that further investigation of the findings reported here is warranted.

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