# Economic Uncertainty and Structural Reforms: Evidence from Stock Market Volatility*

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**Abstract**

Does economic uncertainty promote the implementation of structural reforms? We answer this question using one of the most exhaustive cross-country panel datasets on reforms in six major areas and measuring economic uncertainty with stock market volatility. To identify causality, we exploit exogenous differential variation in countries' exposure to foreign volatility shocks due to pre-determined and time-invariant bilateral characteristics. Across all specifications, we find that stock market volatility has a positive and significant effect on the adoption of reforms. This result is robust to the inclusion of a large number of controls, such as political variables, economic variables, crisis indicators, and a host of country, reform and time fixed effects, as well as across various approaches for accommodating heterogeneous trends and contemporaneous shocks. Overall, this evidence suggests that times of market turmoil, which are characterized by a high degree of uncertainty, may facilitate the implementation of reforms that would otherwise not pass.

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## 1 Introduction

The Great Recession has been accompanied by an enormous increase in macroeconomic volatility, which has stimulated a new literature on how the resulting increase in uncertainty impacts economic activity and especially investment decisions (see, e.g., Bloom, 2009 and 2014). Despite the growing attention of both economists and policy makers to the topic, little effort has been devoted to studying the effect of economic uncertainty on public choices. Such an omission is unfortunate, because the financial crisis, besides marking the end of a period of market stability, has also exposed the urge for structural reforms. The literature has studied extensively the effect of crises as a possible stimulus or obstacle to reforms. However, economic uncertainty per se has received scant attention.

In this paper, we test the hypothesis that economic uncertainty, as captured by stock market volatility, has a casual effect on the adoption of structural reforms. In theory, the effect could be ambiguous. For instance, as in the case of private investment, uncertainty may make politicians more cautious. On the other hand, the literature has found cases in which it can promote reforms. For instance, uncertainty may divert attention and give an opportunity to implement policies that would otherwise not pass.[1] Identifying the effect of economic uncertainty is therefore an open empirical question. Using one of the most exhaustive cross-country panel datasets on reforms together with recent measures of macroeconomic uncertainty, as captured by realized stock-market volatility, this paper finds that economic uncertainty facilitates the implementation of structural reforms.

Following Giuliano, Mishra and Spilimbergo (2013), we define a reform as an increase in deregulation indices available in six sectors: domestic financial sector, capital account, product markets, agriculture, trade and current account transactions.[2] Our measure of economic uncertainty is taken from the rapidly-expanding literature (e.g., Bloom, 2014) that proxies for it with the volatility of stock market returns, built whenever possible from daily data. The idea behind this measure is that when the stock market is more volatile, macroeconomic performance is harder to predict.[3] The main advantage of this measure is that it is widely available and has been shown to be highly correlated with other proxies for macroeconomic uncertainty.[4] Henceforth, we refer to this measure as economic volatility or simply volatility. The resulting dataset spans 6 sectors of reform in 56 countries with yearly observations over the 1973-2006 period.

We start the analysis by showing that volatility is positively and significantly correlated with the adoption of reforms, and that this finding is robust to the inclusion of a large set of controls such as political variables, economic variables, crisis indicators, country-sector fixed effects, sector-time fixed effects and country-specific linear trends. These estimates inform us about conditional correlations,

[1] We discuss more in detail the predictions of existing models and the available evidence in Section 2.

[2] One advantage of focusing on structural reforms rather than fiscal reforms is that they are not affected by automatic stabilizers, which react directly to fluctuations in income.

[3] The theoretical underpinning behind this measure is that macroeconomic variables affect both expected cash flows accruing to stockholders and discount rates. Hence, when macroeconomic performance is harder to predict, the stock market becomes more volatile. Consistent with this view, stock market volatility is significantly related to the dispersion of economic forecasts (e.g., Arnold and Vrugt, 2008).

[4] We also compare the results obtained with alternative measures of economic uncertainty.

but do not have a causal interpretation. To identify the effect of economic volatility on reforms, we develop an Instrumental Variables (IV) strategy that builds on two premises. The first is the well-known result that stock markets are correlated across countries, so volatility shocks originating in one economy tend to spread to other countries. The second premise is that the interdependence between stock markets is stronger among countries that are more integrated with one another. Building on these insights, we construct an instrument for a country's volatility by interacting the volatility of foreign countries with a measure of bilateral integration entirely based on pre-determined geographical and historical characteristics. Identification is thus driven by the differential effect that foreign volatility shocks have on countries that differ in their exogenous exposure to these shocks. Since volatility abroad may be correlated with other characteristics that could directly influence reforms in neighboring countries, we also control for possible policy spillovers from foreign reforms, macroeconomic conditions and interest rates. The IV regressions confirm that an increase in stock market volatility promotes the adoption of structural reforms.

Next, we conduct an extensive sensitivity analysis and address potential remaining threats to identification. First, since foreign volatility is more likely to be exogenous for larger foreign countries, we show that our results continue to hold if we restrict the instrument to the largest economies or if we estimate the effect of volatility on small countries only. Second, we show that the results are largely insensitive to alternative definitions of reforms, such as focusing on large reforms or using changes in the liberalization indices over longer time windows, and to various ways of computing stock market volatility. Third, we find that the results do not crucially depend on any specific subset of countries or different reform areas. Fourth, we use various strategies to accommodate differential trends and contemporaneous shocks within country-sector pairs. Fifth, we implement a falsification test showing that current reforms are not explained by future realizations of volatility, suggesting that our results are driven by period-specific volatility shocks rather than by secular trends in reforms that antedate an increase in volatility. Finally, we study how a potential violation of the exclusion restriction would affect the statistical significance of our coefficient of interest. We find that even substantial relaxations of the exclusion restriction would leave inference informative about the effect of volatility on reforms.

While the main goal of this paper is to establish the causal effect of economic volatility on reforms, in the Appendix we investigate further aspects of the relationship in light of the existing theories. One prominent view is that reforms may be triggered by economic crises, which may in turn vary systematically with volatility. Although we always control for various measures of economic activity in our regressions, we show that our results hold if we add other proxies for economic growth, and are driven neither by the presence of IMF programs nor by countries with a frequent occurrence of crises. We also show how our results compare to recent evidence on the determinants of reforms, such as Abiad and Mody (2005), Mian, Sufi and Trebbi (2014), Ranciere and Tornell (2015) and Giuliano, Mishra and Spilimbergo (2013). Next, we show that volatility promotes liberalizations, but not their reversals, and that it has no effect on non-economic reforms. We also consider alternative

measures of economic uncertainty used in the literature. While not conclusive, we argue that these findings are consistent with the hypothesis that economic uncertainty may help diverting attention from the economic costs of liberalizations.

The results in this paper are important in at least two respects. First, they establish a new empirical fact that may contribute to better understanding the nature of the political resistance to reforms. Second, from a policy perspective, our results suggest that times of market turmoil, which are characterized by a high degree of uncertainty, may provide an opportunity to implement liberalizations that are needed but perceived as unpopular.

The remainder of the paper is organized as follows. Section 2 reviews the existing theoretical and empirical literature on reforms and uncertainty. Section 3 presents the data and shows some descriptive evidence. Section 4 discusses our empirical approach and identification strategy. Section 5 presents the main results. Section 6 contains an extensive sensitivity analysis to assess the robustness of our main evidence to the use of alternative instruments, estimation samples, variables definitions and specifications. Section 7 discusses possible remaining threats to identification. Section 8 concludes. In Appendix D.2, we provide additional results on the relationship between economic volatility and reforms.

## 2 Economic Uncertainty and Reforms: A Look at the Literature

The literature on the political economy of reforms is vast and summarizing it goes beyond the scope of this section.[5] Rather, we briefly discuss some of the main theoretical channels through which economic uncertainty may affect the incentives to implement reforms and then review the existing empirical evidence.

### 2.1 Theory

The term "reform" usually refers to a major change in policy, and common examples of structural reforms are liberalizations of markets for goods or services and changes in the regulatory environment. Even when considered welfare improving, reforms are often difficult to implement because of the unequal distribution of their costs and benefits. The costs may arise from relative price changes, implying adjustment costs, transitional unemployment and redistribution of income between different agents in the society. Frequently, the time profile is also troubling, with costs being paid up-front and benefits accruing with time (see Tommasi and Velasco, 1996, for a more extensive discussion). In the absence of efficient compensation and incentive schemes, the conflict of interest between winners and losers or between voters and policy makers can lead to institutional inertia. The literature has studied how economic conditions, especially crises, may affect the resistance to reform. Since economic crises and uncertainty are often correlated, it is imperative to distinguish between them. We therefore start by studying the predictions of some of the leading approaches

[5] See Tommasi and Velasco (1996) and Drazen (2000) for some surveys.

regarding crises, and then discuss how uncertainty may have an independent effect.

Negative economic shocks may trigger reforms through various channels. Directly, an economic crisis may signal the need for reforms (see, for instance, Drazen, 2000, and Ranciere and Tornell, 2015). Indirectly, it may increase the cost of waiting and hence help resolving any delay due to a war of attrition between political parties (see, for instance, Alesina and Drazen, 1991). On the other hand, an economic crisis may also reduce the likelihood of reforms. For instance, it may increase polarization, thereby weakening ruling coalitions, or it may trigger a backlash against liberal policies (see, for instance, Mian, Sufi and Trebbi, 2014, and Buera, Monge-Naranjo and Primiceri, 2011). It has also been argued that a sovereign crisis can distort the country's incentives, since the economic benefits of reforms may go largely to foreign creditors (see, for instance, Krugman, 1988, and Muller, Storesletten and Zilibotti, 2019).

Focusing instead on economic uncertainty, there are various reasons why it may block or delay the adoption of reforms. As in models of private investment, uncertainty may increase the option value of waiting, especially when considering decisions with long-term consequences. In the influential paper by Fernandez and Rodrik (1991), uncertainty regarding the distribution of gains and losses of a policy change may lead to a *status quo* bias. Economic uncertainty may amplify this bias if it makes it harder to predict who will benefit or lose from a reform.

On the other hand, uncertainty can also facilitate economic reforms. In Alesina and Cukierman (1990), uncertainty can act as a smoke screen that allows politicians more freedom over their actions. In this way, uncertainty might promote any policy change. Moreover, there are instances in which this mechanism alleviates agency problems.[6] In particular, Bonfiglioli and Gancia (2013) show that economic uncertainty can promote the adoption of policies with short-run costs and future benefits, which seems a plausible description of liberalizations. If the upfront costs are more visible than the future benefits, politicians are subject to a myopic bias against reforms. By making the re-election probability depend more on luck than on political actions, higher economic uncertainty lowers this bias.[7]

In sum, the theoretical literature suggests that the relationship between economic uncertainty and reforms may largely be an empirical question and that an important challenge is to control for the independent effect of economic crises.

### 2.2 Evidence

There is a large literature on the empirical determinants of reforms. Although many papers have studied how various economic conditions affect the likelihood of the adoption of reforms, the role

[6] For example, Dewatripont, Jewitt and Tirole (1999) and Holmstrom (1999) present examples in which the agent works harder in order to prove his worth if the principal receives a coarser signal on performance or has less precise information about the agent's type. More uncertainty can also lower the incentive for pandering (Maskin and Tirole, 2004) or conformism (Prat, 2005).

[7] Similarly to Rogoff (1990), the model requires that citizens cannot fully separate the cost of a reform from the effect of the competence of the politician undertaking it. We show these results in Appendix A, where we present a simplified version of the model.

of uncertainty has so far received little attention. After reviewing the experiences of developing countries with market-oriented policies, Tommasi and Velasco (1996) argue that there is a broad consensus in favor of the hypothesis that crises facilitate economic reforms. However, systematic evidence is still scarce. A recent paper by Ranciere and Tornell (2015) shows that trade liberalization, as measured by the Sachs and Warner (1995) index, tends to follow periods of severe crises. On the other hand, Mian, Sufi and Trebbi (2014) and Abiad and Mody (2005) show that banking crises hinder the adoption of financial reforms.[8]

Most of the existing evidence focuses on the adoption of stabilization plans aimed at reducing inflation, government deficit and the black market premium (see, among others, Alesina and Ardagna, 1998, Drazen and Easterly, 2001, and Hamann and Prati, 2002). This literature shows that stabilization plans are more likely to be put in place during periods when inflation, deficit and black market premium are particularly high. Moreover, Alesina, Ardagna and Trebbi (2006) provide evidence from a large panel of countries that fiscal reforms are more likely to occur during times of inflationary and budgetary crisis, when new governments take office and when governments are "strong."

Although crises and volatility are typically correlated, there is almost no evidence on the relationship between volatility and reforms. The only exception is Bonfiglioli and Gancia (2013), who find preliminary evidence that economic uncertainty, measured by the standard deviation of the output gap, is positively correlated with deficit stabilization in a panel of 20 OECD countries observed between 1975 and 2000. However, their analysis is limited to a restricted sample, one indicator of reform only, and provides no evidence on causality.

Other political variables that have been found to be associated with more reforms include the presence of left-wing governments (e.g., Alesina, Ardagna and Trebbi, 2006, and Bonfiglioli and Gancia, 2013) and democracy (e.g., Giavazzi and Tabellini, 2005, and Giuliano, Mishra and Spilimbergo, 2013). We contribute to this literature by identifying the effect of economic volatility using a relatively new and extensive dataset on structural reforms and controlling for the economic and political variables usually considered in previous work.

## 3 Data and Descriptive Evidence

In this section, we present the data, with special emphasis on the indicators of structural reforms and on our proxy for economic uncertainty, and show some preliminary descriptive evidence on the relationship between the two variables.

[8] Other papers (see Broz, Duru and Frieden, 2016, and Forbes and Klein, 2015) show that governments often react to balance-of-payment crises by imposing restrictions to capital flows and trade, and that this may depend on the visibility of the costs of such policies.

### 3.1 Measuring Structural Reforms and Economic Uncertainty

We base the empirical analysis on two recent datasets, which provide useful information for measuring structural reforms and economic uncertainty. For structural reforms, we rely on data that were collected and codified by the Research Department of the IMF, and consist of regulation indices for six sectors covering three areas of reform. In particular, these indices are available for the domestic financial sector and the external capital account (financial sectors reforms), for trade and the current account (foreign-oriented reforms), and for product markets and agriculture (product market reforms). These measures are available for 150 countries with annual observations between 1960 and 2006.

The indices of regulation, from which we derive our measures of reforms, are constructed as means or sums of a series of sub-indices, which are aimed at capturing the extent of regulation of a sector in different respects. We source our data from Prati, Onorato and Papageorgiou (2012) where, as in Giuliano, Mishra and Spilimbergo (2013), all indices take on values between 0 and 1, with 1 corresponding to the minimum degree of regulation. Since the values of these indices increase with the degree of deregulation, hereafter, we refer to them as liberalization indices. A structural reform in a sector is then measured as the annual change in its liberalization index. In Appendix B, we provide a description of the liberalization index for each sector, along with the other variables used in the analysis. Here, we report some of the aspects that are taken into account when compiling the indices, and refer to Ostry, Prati and Spilimbergo (2009) for more details.

The index for *domestic finance* takes into account restrictions imposed to banks in setting interest rates, amounts and conditions on credit and in opening branches; the presence of government ownership of banks; and the quality of bank supervision. It also assesses the policies put in place to develop stock, bond and security markets and to encourage access of foreign actors in these markets. The *capital account* index captures the degree of control and restrictions imposed to residents and non-residents when borrowing or lending across the border, and to firms doing Foreign Direct Investment in the country.

The index for *trade* is based on actual, or imputed, average tariff rates and captures the degree of restrictions applied to imports. It takes on value 0 if tariffs are above 60 per cent. The *current account* index measures restrictions imposed on the proceeds from international transactions (both imports and exports) in goods and services that may be visible and invisible (e.g., finance). It therefore captures additional regulations to trade.

The index for *product markets* focuses on the electricity and telecom sectors, and assesses to what extent these sectors are competitive and free of the direct control of the government. For instance, it contains sub-indices taking into account the extent of privatization, the regulatory power of the government and the degree of competition in the electricity wholesale market and in the local telecom services. Finally, the index for *agriculture* captures the degree of government regulation in the market for the main agricultural export commodities of the country (e.g., wheat, soybeans and cotton for the US or coffee and sugar for Brazil).

Liberalizations in all these sectors are widely considered to be beneficial by economists, since they are believed to improve efficiency and promote economic growth. Yet, these reforms often find harsh resistance. For instance, opponents of financial deregulation argue that it may induce excessive risk taking and may lead to a costly restructuring of the banking system. Among the downsides of trade liberalizations, reallocations and job losses are often mentioned. Privatizations are often blamed to have a regressive distributive impact and to lead to job losses and lower wages. In all cases, the potential costs are often more visible than the expected benefits for society at large, which often take the form of future economic growth.

Our measure of uncertainty, aimed at capturing ex-ante uncertainty about macroeconomic outcomes, is based on stock market volatility, which reflects the variability in investors' expectations over the sales of firms. This indicator is commonly used in the literature (see Bloom, 2014, for a survey) and it is often computed as implied volatility in option prices (VIX). Given the limited availability of VIX for many countries and over an extended time period, we follow Baker and Bloom (2013) and Bloom (2014) and use the volatility of daily stock market returns as our measure of uncertainty.[9] In particular, we use the data compiled by Baker and Bloom (2013), which cover a sample of 60 countries with daily observations on stock market indices from 1970 to 2013. The series we use is computed as the standard deviation of daily returns on the stock market index over non-overlapping quarters. For better cross-country comparability, stock market indices are taken from the same source, the Global Financial Database. In case daily data are not available (for seven countries in the early 1980s and 1990s), weekly or monthly observations are used instead. In the analysis, we take annual averages of quarterly volatility observations. More details on the construction of this variable is provided in Baker and Bloom (2013).

After merging the two datasets, we are left with a sample of 56 developed, emerging and developing countries (see Appendix Table B1) with annual observations between 1973 and 2006, and data on structural reforms in 6 sectors. This means that, after excluding missing data, our dataset contains an unbalanced panel of up to about 6700 observations.

### 3.2 Reforms and Economic Volatility: A Preliminary Glance at the Data

Table 1 reports summary statistics on the liberalization indices for the six sectors. The statistics are computed across all country-year pairs in our sample, using between 1043 and 1169 country-year observations depending on the index. The liberalization indices are equal to 0.79 on average for the trade and current account sectors; in the other sectors, the indices vary from 0.28 for product markets to 0.73 for capital account, agriculture and domestic finance being in between with an average index of 0.58 and 0.67, respectively. The liberalization indices also vary substantially across country-year pairs, with standard deviations ranging from 0.2 (trade) to 0.35 (agriculture). Finally,

[9] As shown by Arnold and Vrugt (2008), US stock market volatility is significantly related to the dispersion of economic forecasts from participants in the Survey of Professional Forecasters, which is a frequently used alternative indicator of fundamental macroeconomic uncertainty.

Table 1: Summary Statistics and Pairwise Correlations of Liberalization Indices

| | Summary Statistics | | | Pairwise Correlation with Liberalization Index for Sector: | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Obs. | Mean | Std. Dev. | Trade | Current Account | Product Market | Agriculture | Domestic Finance | Capital Account |
| Trade | 1111 | 0.79 | 0.20 | 1 | | | | | |
| Current Account | 1169 | 0.79 | 0.23 | 0.61 | 1 | | | | |
| Product Market | 1153 | 0.28 | 0.30 | 0.31 | 0.35 | 1 | | | |
| Agriculture | 1043 | 0.58 | 0.35 | 0.30 | 0.35 | 0.18 | 1 | | |
| Domestic Finance | 1092 | 0.67 | 0.25 | 0.62 | 0.67 | 0.57 | 0.32 | 1 | |
| Capital Account | 1168 | 0.73 | 0.25 | 0.64 | 0.84 | 0.39 | 0.37 | 0.68 | 1 |

*Notes*. The sample consists of 56 countries (listed in Table A1) over the 1973-2006 period. For the sectors *Trade* and *Domestic Finance*, the last sample year is 2005. The summary statistics are computed across all country-year pairs. All pairwise correlations are significant at the 1 per cent level.

the pairwise correlations between the six liberalization indices, shown in the right-hand panel of Table 1, are all positive and statistically significant at the 1 per cent level.

Table 2 reports statistics on the occurrence and size of reforms in our sample. For each sector, columns (1)-(6) report the number and fraction of all country-year pairs in the sample characterized by positive reforms (increases in the liberalization index), negative reforms (decreases in the liberalization index), and no reforms (no change in the index). The results suggest that reforms were generally rather infrequent events, the share of no reforms being larger than 70 per cent for five sectors out of six. The most frequent reforms occurred in the trade sector, with only 14 per cent of the country-year pairs showing no change in the liberalization index. Conversely, the least frequent reforms took place in the market for agricultural commodities, with 98 per cent of the country-year pairs characterized by no change in the liberalization index. Columns (1)-(6) also point out that, in any sector, the majority of reforms were positive, i.e., they led to de-regulations. Columns (7)-(9) report details on the average size of the reforms, both for all reform episodes and for positive and negative reforms separately. The average non-zero annual change in the liberalization indices ranges from 3 per cent for the trade sector to more than 40 per cent for agriculture. This pattern suggests that more frequent reforms tended to be smaller in size on average.

These observations are in line with the extensive descriptive evidence provided in Prati, Onorato and Papageorgiou (2012). To give an idea of the type of reforms captured by our indices, we mention here some of the major changes recorded in our dataset over the years. Trade reforms spiked in the early 1970s, when the six core EU countries implemented the first Generalized System of Preferences scheme, Greece opened to trade after the dictatorship, and Argentina and Ecuador also liberalized. Trade reforms appear also in the late 1970s, when the Tokyo round was implemented; in the late 1980s and early 1990s, as an effect of the Uruguay round; and in mid 1990s, when China significantly reduced tariffs. Product market liberalizations started only in 1992 and exhibited a spike in the late 1990s with the privatization of telecom and utility companies in most European countries. Finally,

Table 2: Summary Statistics on Reforms

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| | Positive Reforms | | Negative Reforms | | No Reforms | | Average Reform Size | | |
| | Number | Fraction | Number | Fraction | Number | Fraction | All | Positive | Negative |
| Trade | 595 | 0.54 | 357 | 0.32 | 156 | 0.14 | 0.01 | 0.03 | -0.03 |
| Current Account | 144 | 0.12 | 40 | 0.03 | 985 | 0.84 | 0.01 | 0.13 | -0.15 |
| Product Market | 180 | 0.16 | 0 | 0.00 | 973 | 0.84 | 0.02 | 0.16 | - |
| Agriculture | 17 | 0.02 | 3 | 0.00 | 1015 | 0.98 | 0.01 | 0.49 | -0.44 |
| Domestic Finance | 272 | 0.25 | 25 | 0.02 | 795 | 0.73 | 0.02 | 0.09 | -0.10 |
| Capital Account | 109 | 0.09 | 29 | 0.02 | 1030 | 0.88 | 0.01 | 0.17 | -0.19 |

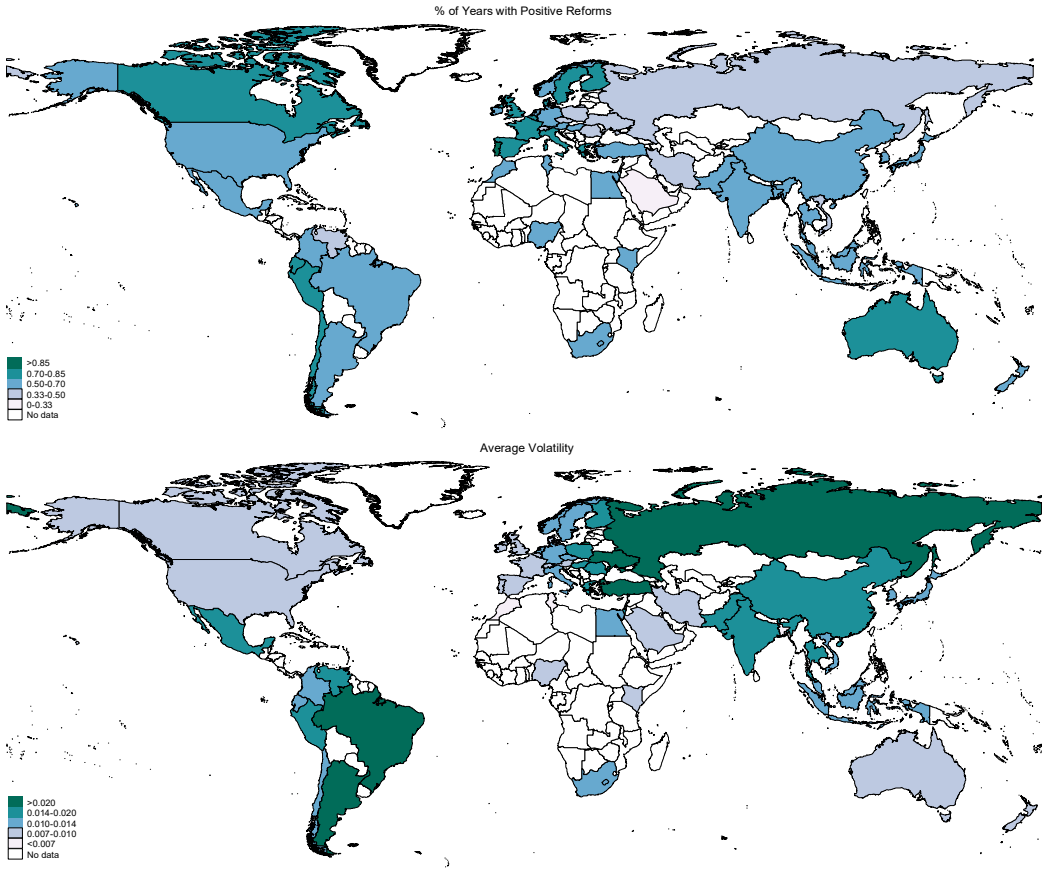
*Notes*. The columns labeled *Number* and *Fraction* report the number and percentage, respectively, of country-year pairs characterized by positive, negative or no changes in a given liberalization index. The columns labeled *Average Reform Size* report the arithmetic mean of the changes in a given liberalization index across: all country-year pairs (*All*); country-year pairs with positive changes in the index (*Positive*); and country-year pairs with negative changes in the index (*Negative*).

reforms to domestic finance were especially widespread in the 1980s, in 1990-91 (prominent examples were Colombia, the EU countries, India, Japan, Korea and Indonesia) and in 1996.

Figures 1 and 2 offer a preliminary look at the relationship between reforms and economic volatility. Figure 1 provides information on the pattern of geographical variation in reforms and volatility. For each country, the first map displays the fraction of years characterized by a positive change in an aggregate index of liberalization, computed as the arithmetic mean of the indices for the six sectors of reform. The second map displays instead the average value of stock market volatility in each country over the sample period. Overall, the figure highlights significant variation in both reforms and volatility across countries. It also reveals the existence of a positive correlation between the two variables, with both volatility and the frequency of de-regulations being higher in Latin America, Continental Europe, China and India, and lower in Africa and the Middle East.[10]

Figure 2 focuses on the evolution of reforms and volatility over time. In particular, the figure shows average volatility (diamond) and the average change in the aggregate liberalization index (hollow circles) across all countries in a given year. Volatility and reforms exhibit a marked comovement, with reforms increasing over the 1980s, a decade of rising volatility, and then decreasing over the 1990s and the early 2000s, a period characterized by a declining trend in volatility. Motivated by this evidence, in the next sections, we use regression analysis to systematically study the relationship between volatility and reforms, and to investigate the possible existence of a causal effect of the former on the latter.

[10] A similar picture (available upon request) emerges if one looks at the average size of reforms in each country rather than at the frequency of positive reforms.




*Notes.* The first map plots the fraction of years characterized by positive reforms in a country. Positive reforms are annual increases in an aggregate index of liberalization, computed as the simple average of the liberalization indices for six sectors of reform. The second map plots the average value of stock market volatility in a country over the sample period.

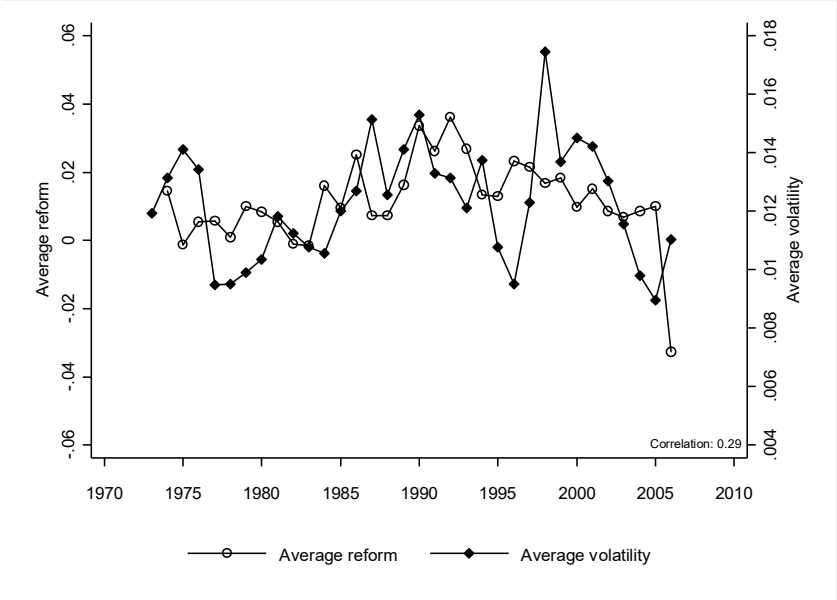
Figure 1: Volatility and Reforms across Countries

## 4 Empirical Approach and Identification Strategy

Our main approach to study how economic volatility affects the implementation of structural reforms consists of estimating specifications of the following form:

$$ref_{s,c,t} = \eta_{s,c} + \eta_{s,t} + \beta_1 lib_{s,c,t-1} + \beta_2 vol_{c,t-1} + \boldsymbol{\beta}_3 \mathbf{X}_{s,c,t-1} + \beta_{4,c} t + \epsilon_{s,c,t}, \tag{1}$$

where $ref_{s,c,t} \equiv lib_{s,c,t} - lib_{s,c,t-1}$ is the change in the liberalization index ($lib$) for sector $s$ and country $c$ over year $t$, and $vol_{c,t-1}$ is the stock market volatility of country $c$ at time $t-1$. We control for country-sector fixed effects, $\eta_{s,c}$, to absorb time-invariant determinants of reforms in each sector of a given country. Accordingly, we exploit time variation in reforms within country-sector pairs for identification. We also control for sector-year fixed effects, $\eta_{s,t}$, to account for aggregate trends and global shocks that could induce reforms within a given sector across all countries. Besides the two sets of fixed effects, we control for country-specific linear trends, $\beta_{4,c} t$, and a rich set of covariates, $\mathbf{X}_{s,c,t-1}$, to absorb other time-varying determinants of reforms potentially correlated with




*Notes.* *Volatility* is stock market volatility, computed as the arithmetic mean of all quarterly volatility observations for a country in a year. *Reform* is the annual change in an aggregate index of liberalization, computed as the simple average of the liberalization indices for six sectors of reform. The figure plots simple averages of *volatility* and *reform* in a given year across the countries listed in Table A1.

Figure 2: Volatility and Reforms over Time

volatility.[11] Finally, we control for the start-of-period level of the liberalization index, $lib_{s,c,t-1}$, to account for the fact that reforms may proceed at different pace across countries and sectors, depending on their initial level of regulation. For instance, it is often argued that the benefits of reforms are greater when starting from a higher level of regulation, which would imply $\beta_1 < 0$. Including $lib_{s,c,t-1}$ also helps comparability with the empirical literature on both structural and fiscal reforms, where this term is standard. We correct the standard errors for clustering at the country level (the level at which volatility is defined) to account for possible correlation in the residuals within a given country both across sectors and over time. In Appendix D.1, we show that alternative clustering schemes typically deliver lower standard errors, and thus imply less conservative inference about the coefficient $\beta_2$, than clustering by country.

As in Giuliano, Mishra and Spilimbergo (2013), eq. (1) pools observations across countries, sectors of reform and years, imposing the same coefficient on volatility and any other country-level variable across all sectors. This approach allows to fully exploit the information contained in the data and maximizes statistical power. However, restricting coefficients to be the same across sectors may hide heterogeneity in the response of different types of reform to volatility. In Section 6.3, we therefore complement the baseline analysis by estimating eq. (1) separately by reform area. These regressions allow to uncover potential heterogeneity in the effect of volatility but are estimated with

[11] See the next section for details on the control variables. Appendix B contains a detailed description of all the variables used in the analysis and their sources.

less statistical power, especially on areas in which reforms are infrequent and thus the liberalization indices seldom change on a yearly basis.

We start by estimating eq. (1) using OLS. The resulting estimates inform us about the conditional correlation between volatility and reforms but do not have a causal interpretation. Indeed, the political debate over the design and approval of a reform could influence volatility in the years prior to the adoption of the reform itself. The bias generated by this reverse causality could go either way. On the one hand, the discussion over a reform could raise economic and political uncertainty, implying an upward bias in the OLS estimate of $\beta_2$. On the other hand, the prospect of a reform in the near future may contribute to calming down the stock market before the reform is actually implemented, implying a downward bias in the coefficient $\beta_2$ estimated by OLS.

To identify the effect of volatility on reforms, we run Two-Stage Least Squares (2SLS) regressions. We need an instrument that is both a strong predictor of country $c$'s volatility and uncorrelated with country- and sector-specific unobservables influencing the adoption of reforms in country $c$. To construct the instrument, we build on well-known insights from the finance literature. First, stock market volatility is known to be correlated across countries, as volatility shocks originating in one economy propagate to other countries (see, for instance, King and Wadhwani, 1990, Forbes and Rigobon, 2002, Bonfiglioli and Favero, 2005, and Diebold and Yilmaz, 2015). As long as foreign volatility shocks influence the reform process in country $c$ only through its own volatility—the identifying assumption that we discuss in detail below—they can be used to construct a valid instrument for $vol_{c,t-1}$. Second, the interdependence among stock markets is stronger for countries that are more integrated with one another (see, e.g., Forbes and Rigobon, 2002). This suggests that the sensitivity of a country's volatility to another country's volatility is likely to be increasing in the level of interconnection between the two economies.

Building on these insights, we construct the instrument as follows:

$$vol\_shock_{c,t-1} = \sum_{j \in \Omega_{-c}} \frac{vol_{j,t-1}}{N_{-c}} \times \ln Int_{c,j}, \tag{2}$$

where $vol_{j,t-1}$ is the stock market volatility of country $j \neq c$ in year $t-1$, $\Omega_{-c}$ and $N_{-c}$ are the set and the number of countries excluding $c$, and $Int_{c,j}$ is a measure of economic integration between countries $c$ and $j$. This instrument is meant to isolate the differential variation that foreign volatility shocks induce on the volatility of each country $c$, depending on how interconnected it is with the origins of these shocks. The instrument accommodates both the absolute integration of each country and relative differences in integration across countries. To keep the composition of the instrument constant, we include in the set $\Omega_{-c}$ all countries with available data on stock market volatility for all years.[12]

[12] The countries included in the baseline instrument are Argentina, Australia, France, Germany, Japan, Korea, New Zealand, Singapore, Switzerland, Thailand, the UK and the US. By encompassing the largest economies in terms of stock market capitalization and spanning all continents (except for Africa), these countries are likely to be a relevant source of volatility spillovers. In Section 6.1, we perform an extensive sensitivity analysis on the composition of the

In the baseline version of the instrument, we proxy for economic integration between any pair of countries using the inverse of their geographical distance.[13] The literature on the gravity equation has long shown that distance is a strong inverse proxy for economic integration. More recently, a growing body of work has demonstrated that a gravity model explains international transactions in financial assets at least as well as trade in goods. For instance, the negative effect of distance has been established in Portes and Rey (2005), who argue that it proxies for information costs and the efficiency of transactions. Aviat and Coeurdacier (2007) confirm this result and show that the effect of distance works partly through its impact on trade in goods.[14] For our identification strategy, bilateral distance has the advantage of being reasonably exogenous. In particular, being time invariant, distance does not endogenously respond to changes in the volatility of any country. On the other hand, it matters for the transmission of volatility across countries. For instance, Appendix Figure D1 shows that the index of volatility connectedness constructed by Diebold and Yilmaz (2015), which provides an estimate of the volatility spillovers from abroad, falls markedly with the average distance from other countries. As an alternative, in Section 6.1, we use another instrument that employs a direct measure of economic integration based on the pre-determined component of bilateral trade due to geographical as well as historical characteristics. This second instrument is more restrictive but leaves our results unchanged.[15]

Another aspect of eq. (2) that is worth discussing is the treatment of countries of different size. On the one hand, countries with large economies and stock market capitalization—such as the US, the UK and Japan—may in principle have a disproportionate influence on the volatility of other economies. On the other hand, these countries in practice have experienced relatively low levels of volatility over the period of analysis, as shown in Figure 1. Given these two contrasting forces, we construct the baseline instrument in the simplest form, treating all countries equally. In Section 6.1, we propose alternative versions of the instrument that accommodate differences in economic size and stock market capitalization across countries. We find results to be largely insensitive to how these differences are treated.

We now turn to discussing identification. This requires the instrument $vol\_shock_{c,t-1}$ to be uncorrelated with the error term in eq. (1), conditional on the fixed effects and the covariates. In this respect, the sector-year fixed effects absorb all aggregate shocks and trends that could be correlated with global volatility and affect reforms uniformly across countries. The country-sector fixed effects soak up all time-invariant characteristics of a country-sector pair that could be correlated with the proxies for economic integration. Finally, the covariates and the country-specific

instrument, showing that our results do not depend on which countries are included in it.

[13] Information on bilateral distances (number of kilometers between countries' capital cities) is sourced from CEPII's GeoDist Database.

[14] Okawa and van Wincoop (2012) provide theoretical foundations for the growing literature estimating gravity equations in finance.

[15] An earlier version of this paper (Bonfiglioli and Gancia, 2015), following Baker and Bloom (2013), instrumented a country's volatility with natural disasters, political coups and revolutions occurred in the other countries. The results were in line with the present version.

linear trends mitigate the concern that the instrument could be correlated with the error term due to adjustments in country-specific conditions or differences in the deterministic evolution of reforms across countries.

Despite the large set of controls included in eq. (1), two threats remain to our identification strategy. First, foreign volatility could be correlated with other characteristics of foreign countries that influence country $c$'s reforms directly rather than through its own volatility. To account for this, the vector of covariates $\mathbf{X}_{s,c,t-1}$ includes four *spillover* variables controlling for key characteristics of foreign countries that could have a direct influence on country $c$'s reforms. These characteristics are: (i) reforms implemented in each sector, as country $c$ may choose to adopt some of the reforms enacted abroad, e.g., by imitation (see, for instance, Buera, Monge-Naranjo and Primiceri, 2011); (ii) real per-capita GDP and (iii) inflation, as foreign countries' business cycle could affect reforms in country $c$ by influencing its own economic conditions; and (iv) interest rates, as lower returns on financial assets abroad may induce capital inflows into country $c$, stimulating reforms therein (see, e.g., Abiad and Mody, 2005, and Bartolini and Drazen, 1997). We construct each spillover variable analogously to the instrument in eq. (2), replacing $vol_{j,t-1}$ with one of these four characteristics. A related concern is that reforms taking place in country $c$ could influence volatility in foreign countries, making the instrument endogenous to country $c$'s reforms. Conceivably, this is more likely to happen when $c$ is a large economy and $j$ is a small country. Accordingly, in Section 6.1 and 6.2, we show that our results continue to hold if we restrict the instrument to the largest economies and estimate eq. (1) on small countries only.

The second threat to identification is that, in specific countries and sectors, some underlying trend or contemporaneous shock might remain that influences the adoption of reforms and is correlated with the instrument. This concern is largely mitigated by the sector-year fixed effects and country-specific linear trends that are included in all specifications. However, these controls may still leave room to shocks hitting specific countries within sectors and to non-linear trends. In Section 7, we use various approaches to more flexibly account for underlying trends and contemporaneous shocks, and find that they are unlikely to drive our main results. We also implement a falsification test showing that current reforms are not explained by future realizations of volatility. This further suggests that our results are driven by period-specific volatility shocks rather than by secular trends in reforms that antedate an increase in volatility.

Overall, the controls included in the specification and the extensive sensitivity analysis indicate that our results are robust to the most plausible confounders. Yet, this may not dispel all possible concerns with a violation of the exclusion restriction. In Section 7, we therefore implement a complementary exercise, by studying how inference about the parameter of interest $\beta_2$ would change under various degrees of violation of the exclusion restriction, using an approach developed by Conley, Hansen and Rossi (2012). Given the strong predictive power of the instrument at the first stage, we find that inference would remain informative about the causal effect of volatility even in the presence of substantial violations of the exclusion restriction.

## 5 Main Results

The OLS estimates of $\beta_2$ are reported in Table 3. To have a sense of how the correlation between volatility and reforms is influenced by other covariates, we start with a benchmark specification that does not include any control variable, and then progressively add controls until we reach the most complete version of eq. (1). Due to the long list of control variables, the full set of coefficient estimates is reported in Appendix Table D1.

Column (1) refers to a simple regression of $ref_{s,c,t}$ on $vol_{c,t-1}$, including only country-sector and sector-year fixed effects. The positive and very precise estimate of $\beta_2$ implies that higher stock market volatility is associated with reforms leading to de-regulations. The point estimate remains stable when we add country-specific linear trends in column (2). In column (3), we further include the initial level of the liberalization index. As shown in Appendix Table D1, this variable enters with a negative and significant coefficient, suggesting that countries and sectors that start highly regulated tend to undergo stronger liberalizations. The negative autoregressive coefficient is consistent with previous findings in the empirical literature on reforms, and lends support to the view that de-regulations tend to be enacted when they are needed the most. Yet, the correlation between volatility and reforms is robust to controlling for the initial level of the liberalization index.

The remaining columns of Table 3 extend the specification by adding other time-varying controls. In column (4), we include several proxies for countries' economic and financial conditions. In particular, we add four dummies capturing the existence of a crisis in a country: a dummy equal to 1 in the presence of a recession, defined as a negative growth rate of real per-capita GDP; two dummies indicating the onset of a banking and a currency crisis, respectively, as coded by Laeven and Valencia (2012); and a dummy equal to 1 if the country declared default on its sovereign debt. We also include four equivalent dummies taking on value 1 if a crisis of a certain type occurred over the previous three years, in order to account for the long-term consequences of crises besides their short-run impact. Moreover, we control for inflation, as reforms may follow changes in macroeconomic conditions even in the absence of a crisis, and for average stock market returns, given that the first and second moments of stock market returns may be correlated even after accounting for macroeconomic conditions.[16] As shown in Appendix Table D1, while reforms are negatively correlated with inflation, they are not correlated with the first moment of stock market returns. The results also show that reforms are correlated with crises, with coefficient signs varying across types of crises and between the short and the long run. We go back to these results in Appendix D.2, where we compare our findings on volatility to those of other works studying alternative determinants of reforms. In any case, adding these controls does not overturn the positive correlation between reforms and volatility, which now is possibly even stronger.

[16] Like volatility, all controls enter the specification with a one-year lag. Hence, the dummies for the existence of crises over the previous three years are equal to 1 if a crisis of a certain type occured between $t-2$ and $t-4$. We have also experimented with dummies for crises over the previous five years, obtaining similar results (available upon request).

Table 3: Baseline Estimates, OLS

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| vol | 0.743*** | 0.657*** | 0.363*** | 0.747*** | 0.965*** | 0.947*** | 0.941*** |
| | [0.115] | [0.105] | [0.103] | [0.212] | [0.257] | [0.263] | [0.259] |
| Country-Sector FE | yes | yes | yes | yes | yes | yes | yes |
| Sector-Year FE | yes | yes | yes | yes | yes | yes | yes |
| Country-Specific Linear Trends | no | yes | yes | yes | yes | yes | yes |
| Initial Liberalization Index | no | no | yes | yes | yes | yes | yes |
| Economic and Financial Controls | no | no | no | yes | yes | yes | yes |
| Development Controls | no | no | no | no | yes | yes | yes |
| Political Controls | no | no | no | no | no | yes | yes |
| Controls for Spillovers | no | no | no | no | no | no | yes |
| Observations | 6725 | 6725 | 6725 | 6381 | 5833 | 5703 | 5703 |
| R-squared | 0.11 | 0.12 | 0.19 | 0.21 | 0.23 | 0.23 | 0.23 |

*Notes*. The regressions are estimated on pooled data across countries, sectors of reform and years. The dependent variable is the annual change in the liberalization index for a sector within country $c$. *Vol* is the one-year lag of stock market volatility in country $c$, computed as the arithmetic mean of all quarterly volatility observations for the country in a year. *Initial Liberalization Index* is the one-year lag of the liberalization index. *Economic and Financial Controls* are: average stock market returns; inflation; four dummies for the occurrence of a recession or a banking, currency and sovereign crisis in a given year; and four dummies for the occurrence of a recession or a banking, currency and sovereign crisis over the previous three years. *Development Controls* are: log real per-capita GDP; a dummy for countries that are OECD members; and a dummy for countries that are EU members two years later. *Political Controls* are: the polity2 index; a dummy for countries in which the party leading the government has a left-wing orientation with respect to economic policy; a dummy for countries with presidential political systems; and a dummy for years in which a legislative and/or an executive election takes place in country $c$. *Controls for Spillovers* are four variables defined as arithmetic averages of (i) the change in the liberalization index for a sector in country $j \neq c$, (ii) log real per-capita GDP in country $j \neq c$, (iii) inflation in country $j \neq c$, and (iv) the interest (lending) rate in country $j \neq c$ multiplied by the log inverse bilateral distance from country $c$. See Table A2 for the full list of estimated coefficients on the control variables. All regressors enter with a one-year lag, except for EU membership, which enters with a two-year lead. The standard errors, reported in square brackets, are corrected for clustering at the country level. *, ** and *** denote significance at 10, 5 and 1 per cent, respectively.

In column (5), we add a number of development indicators to account for the fact that countries at different stages of development may have different incentives to adopt reforms. In particular, we include the log of real per-capita GDP and a dummy equal to 1 if a country is an OECD member in a given year. To take into account that the prospective accession to the European Union (EU) may provide additional incentives to adopt reforms, we also include a dummy that takes on value 1 at time $t$ if a country is a member of the EU two years later (i.e., at time $t+2$).[17] These controls enter with small coefficients and have little bearing on the estimate of $\beta_2$.

In column (6), we add various political controls. Giuliano, Mishra and Spilimbergo (2013) find that democracy leads to reforms. Hence, we account for a country's degree of democracy using the polity2 index sourced from the Polity IV database. The ideology of the ruling party may also affect the adoption of reforms, as pointed out by Muller, Storesletten and Zilibotti (2016), among others. Therefore, we also control for a dummy taking on value 1 if the party leading the government has a left-wing orientation with respect to economic policy, as coded by the World Bank in the Database on Political Institutions (DPI). Presidential systems are argued to be better suited to overcome the resistance of small interest groups and hence to adopt more reforms (see, for instance, Persson

[17] We find that the effect of joining the EU is strongest two years before accession. However, the results are not very sensitive to changing this time window.

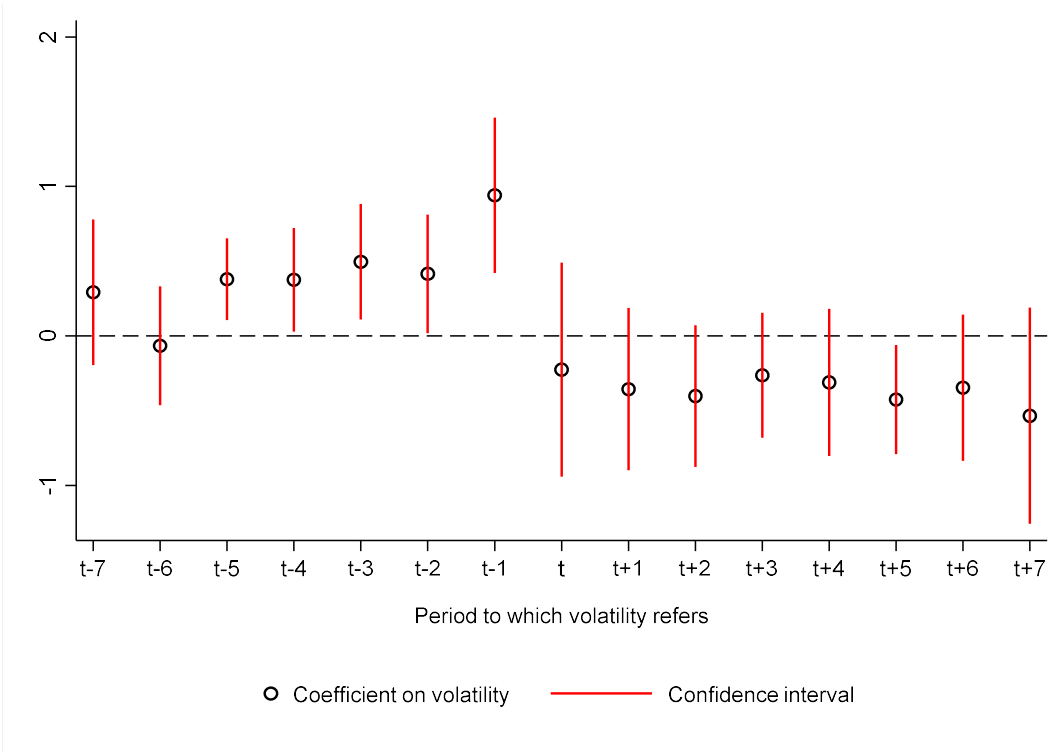
and Tabellini, 2002). Thus, we also include a dummy equal to 1 if the political system is coded as presidential according to the DPI. Finally, we control for a dummy equal to 1 in years in which a legislative and/or an executive election takes place, as recorded by the DPI. None of these controls enters with a statistically significant coefficient, and the correlation between structural reforms and volatility is accordingly unchanged.

Finally, in column (7), we include the four spillover variables described in the previous section. These variables control for the possibility that country $c$'s reforms are correlated with foreign countries' reforms or macroeconomic variables, such as per-capita GDP, inflation and interest rates.[18] Appendix Table D1 shows a positive and statistically significant coefficient on the proxy for reform spillovers, consistent with recent studies finding that imitation and catching up across countries play an important role in the implementation of structural reforms (see, e.g., Abiad and Mody, 2005, Buera, Monge-Naranjo and Primiceri, 2011, Giuliano, Mishra and Spilimbergo, 2013). The other spillover variables enter instead with small and imprecisely estimated coefficients. The estimate of $\beta_2$ is slightly reduced when controlling for the spillover variables, but remains positive, very precisely estimated and in the same ballpark as the previous estimates.

Column (7) is our preferred specification and we henceforth refer to it as the baseline. The $R^2$ associated with this specification is 0.23, implying that a substantial fraction of the variation in structural reforms remains unexplained despite the large set of fixed effects and covariates. This result partly reflects the nature of structural reforms: in some sectors, these reforms do not occur very frequently, so the liberalization indices for these sectors exhibit sporadic changes on a yearly basis. As shown in the next section, when this feature of the data is taken into account by measuring reforms over a longer time window, the explanatory power of the regressors substantially increases. More generally, the moderate $R^2$ reported in column (7) confirms the view that the implementation of structural reforms is a complex phenomenon, which depends on many factors and is thus hard to explain. In this respect, the existing theories reviewed in Section 2.1 suggest that volatility can facilitate reforms although it does not necessarily act as a direct determinant.

To dig deeper into the timing of the relationship between volatility and reforms, Figure 3 reports point estimates and 95 per cent confidence intervals of $\beta_2$ obtained by separately estimating the baseline specification using different lags and leads of volatility. The results show that reforms are positively and significantly correlated with past volatility over a period of five years, which corresponds to the typical lifetime of a legislature. Within this time frame, the correlation is stronger for shorter lags of volatility and reaches its maximum at the first lag. Consistent with this evidence, we use the first lag of volatility in our main regressions. At the same time, the significant coefficients on longer lags of volatility are consistent with the fact that some reforms take more than one year to be completed. In Section 6.3, we provide more evidence on this point using alternative specifications,

[18] To construct the proxy for interest rate spillovers, we use data on lending rates, which are available for the largest number of countries and years from the World Bank Development Indicators. We have also experimented with deposit rates and government bold yields (sourced from FRED, St. Louis FED) obtaining similar results (available upon request).




*Notes.* Each coefficient is obtained by separately estimating the specification in column (7) of Table 3 using a different lag or lead of volatility, as indicated on the horizontal axis. All regressions are estimated using OLS. The confidence intervals are based on standard errors corrected for clustering at the country level and refer to the 95 per cent significance level.

Figure 3: Timing of the Relationship between Volatility and Reforms

which relate changes in the liberalization indexes to volatility over longer time horizons. Conversely, Figure 3 shows that reforms are uncorrelated with future volatility, suggesting that governments are largely insensitive to expectations about uncertainty when deciding upon a reform. While this finding also suggests that reforms do not seem to be a driver of volatility, we now turn to 2SLS regressions to identify a causal effect of volatility on reforms.

The main results for the baseline 2SLS specification are reported in Table 4, which shows the coefficients on $vol\_shock_{c,t-1}$ from both the first-stage regression (column 1) and the reduced-form regression (column 2), as well as the coefficient on $vol_{c,t-1}$ from the second-stage regression (column 3). Appendix Table D2 contains the complete list of coefficient estimates on all the right-hand-side variables from these three regressions.

The first-stage coefficient on $vol\_shock_{c,t-1}$ is positive, as expected, and also large and highly statistically significant, with a point estimate of 0.632 and a standard error of 0.069. This underscores the strong predictive power of the instrument at explaining differences in stock market volatility across countries.[19] The reduced-form coefficient on $vol\_shock_{c,t-1}$ is also positive and very precisely estimated, with a point estimate of 1.298 and a standard error of 0.371. These numbers imply that

[19] The Kleibergen-Paap $F$-statistic is equal to 82.9 and thus exceeds the value of 10 normally considered as a rule-of-thumb threshold for instrument relevance.

Table 4: Baseline Estimates, 2SLS

| | (1) | (2) | (3) |
|---|---|---|---|
| | First Stage | Reduced Form | Second Stage |
| vol | | | 2.055*** |
| | | | [0.545] |
| vol_shock | 0.632*** | 1.298*** | |
| | [0.069] | [0.371] | |
| Country-Sector FE | yes | yes | yes |
| Sector-Year FE | yes | yes | yes |
| Country-Specific Linear Trends | yes | yes | yes |
| Control Variables | yes | yes | yes |
| Kleibergen-Paap $F$-stat. | 82.9 | | |
| Observations | 5703 | 5703 | 5703 |
| R-squared | 0.86 | 0.23 | 0.23 |

*Notes*. The regressions are estimated on pooled data across countries, sectors of reform and years. *Vol* is the one-year lag of stock market volatility in country *c*, computed as the arithmetic mean of all quarterly volatility observations for the country in a year. *Vol_shock* is the arithmetic average of the one-year lag of stock market volatility in all countries *j≠c* with non-missing observations, multiplied by the log inverse bilateral distance from country *c*. The dependent variable is *vol* in column (1) and the annual change in the liberalization index for a sector within country *c* in columns (2) and (3). *Control variables* are those included in column (7) of Table 3. See Table A3 for the full list of estimated coefficients on the control variables. The standard errors, reported in square brackets, are corrected for clustering at the country level. *, ** and *** denote significance at 10, 5 and 1 per cent, respectively.

countries that are more exposed to foreign volatility shocks tend to implement larger reforms.

Under the exclusion restriction that $vol\_shock_{c,t-1}$ affects $ref_{s,c,t}$ only through $vol_{c,t-1}$, the second-stage coefficient yields the causal effect of volatility on reforms. This coefficient corresponds to the ratio between the reduced-form and the first-stage coefficients, and is thus equal to 2.055 (s.e. 0.545). The larger size of $\beta_2$ when using 2SLS suggests a downward bias in the effect of volatility on reforms detected by OLS. In terms of magnitude, the point estimate of $\beta_2$ reported in Table 4 implies that an increase in $vol_{c,t-1}$ by one interquartile range (0.007), which is also roughly equal to one standard deviation in our sample, would lead to an increase in the dependent variable $ref_{s,c,t}$ by 1.4 per cent, which is a reform of approximately the average size in our data. Overall, these numbers imply that the effect of volatility on reforms is not only statistically significant but also quantitatively sizable.

## 6 Robustness Checks

In this section, we perform an extensive sensitivity analysis to assess the robustness of the main results to the use of alternative instruments, estimation samples, variables definitions and specifications.

Table 5: Alternative Formulations of the Baseline Instrument

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Stock Market Volatility Interacted with: | | | |
| | Inverse Distance and SMC | Inverse Distance and GDP | Inverse Distance | Inverse Distance |
| 2nd Stage Regression | | | | |
| vol | 2.045*** | 2.055*** | 1.644* | 1.973*** |
| | [0.546] | [0.539] | [0.849] | [0.587] |
| Observations | 5703 | 5703 | 5703 | 5703 |
| R-squared | 0.23 | 0.23 | 0.23 | 0.23 |
| 1st Stage Regression | | | | |
| vol_shock | 0.025*** | 0.025*** | 0.374*** | 0.235*** |
| | [0.003] | [0.003] | [0.057] | [0.032] |
| Kleibergen-Paap $F$-stat. | 89.6 | 84.4 | 42.3 | 54.9 |
| Country-Sector FE | yes | yes | yes | yes |
| Sector-Year FE | yes | yes | yes | yes |
| Country-Specific Linear Trends | yes | yes | yes | yes |
| Control Variables | yes | yes | yes | yes |
| Countries Included in Instrument | All | All | Top-5 SMC | Top-1 SMC in each Continent |

*Notes*. The regressions are estimated on pooled data across countries, sectors of reform and years. The dependent variable is the annual change in the liberalization index for a sector within country $c$. *Vol* is the one-year lag of stock market volatility in country $c$, computed as the arithmetic mean of all quarterly volatility observations for the country in a year. To construct the instrument, *vol_shock*, the one-year lag of stock market volatility in country $j \neq c$ is multiplied by: the log inverse distance of country $j$ from country $c$ and the log stock market capitalization of country $j$ in 2006 (column 1); the log inverse distance of country $j$ from country $c$ and the log GDP of country $j$ in 1973 (column 2); the log inverse distance of country $j$ from country $c$ (columns 3-4). The resulting products are averaged across all countries $j \neq c$ belonging to the set indicated in the last row of the table. Control variables are those included in column (7) of Table 3. The standard errors, reported in square brackets, are corrected for clustering at the country level. *, ** and *** denote significance at 10, 5 and 1 per cent, respectively.

### 6.1 Alternative Instruments

As mentioned in Section 4, our baseline instrument treats countries equally, independent of the size of their economies and the capitalization of their stock markets. To study the implications of this formulation for our results, in columns (1) and (2) of Table 5, we re-construct the instrument to allow for differences in stock market capitalization and GDP across countries, by multiplying the log inverse distance of country $c$ from country $j$ in eq. (2) by the log of either variable in country $j$.[20] In both cases, the coefficient $\beta_2$ is very close to the baseline estimate.

Foreign countries' volatility could respond to reforms undertaken in country $c$, especially when foreign countries are relatively small. One way to address this concern is to exclude small countries from the construction of the instrument. Accordingly, in columns (3) and (4), we re-construct the instrument by restricting the set of foreign countries $j$ in eq. (2) to, respectively, the top five economies

[20] We source data on stock market capitalization and GDP from the World Development Indicators. We use data on GDP for 1973, the first sample year, and on stock market capitalization for 2006, the first year with complete data coverage for all countries in our sample.

by stock market capitalization and the economy with the highest stock market capitalization in each continent.[21] This approach also represents a complementary way of allowing for differences in size, as only the top countries are included in the instrument. In all cases, the coefficient $\beta_2$ is positive, precisely estimated and in the same ballpark as the baseline estimate.

We now turn to the role of geographical distance as a proxy for economic integration. As discussed in Section 4, while distance is arguably exogenous, it is not the only measure of exposure to foreign volatility. As an alternative, one could construct the instrument using direct measures of market integration, such as international trade. These measures, however, would be endogenous, as they respond to structural reforms undertaken, e.g., in the trade or current account sectors. Building on Frankel and Romer (1999), we therefore proxy for market integration between any two countries using the component of their bilateral trade that is explained by pre-determined (geographical and historical) bilateral characteristics, while netting out origin- and destination-specific factors that could have a direct impact on reforms (see Appendix C for details). With this predicted trade variable, $\hat{T}_{c,j}$, in hand, we then construct an alternative instrument by setting $\ln Int_{c,j} = \ln \hat{T}_{c,j}$ in eq. (2). While exploiting a direct measure of market integration, this instrument requires stronger functional-form assumptions and is subject to stricter identification conditions than the baseline instrument, as none of the bilateral characteristics must be correlated with unobservable determinants of reforms. This notwistanding, the estimates of $\beta_2$ reported in Table 6 are close in size to their counterparts obtained with the baseline instrument and shown in Tables 4 and 5.

### 6.2 Alternative Samples

As previously mentioned, our identifying assumption would be endangered if the political debate over reforms taking place in country $c$ influenced volatility in foreign countries. In this respect, we have shown that our main results continue to hold when restricting the construction of the instrument to the largest countries, whose volatility is less likely to be influenced by events occuring abroad. Here, we perform a complementary exercise and re-estimate the baseline specification in eq. (1) after excluding large countries from the estimation sample. Focusing on small countries makes it less likely that their domestic reforms could have an influence on foreign countries' volatility. In column (1) of Table 7, we start by dropping the US, the largest country in our sample by both GDP and stock market capitalization. In columns (2) and (3), we instead exclude all countries whose stock market capitalization and GDP, respectively, are at least as high as the sample median. The coefficient $\beta_2$ is positive and very precisely estimated regardless of how we restrict the sample to exclude large countries.

A related concern is that, due to structural change, some countries could have actively engaged in reforms and experienced sustained volatility over the sample period. One example are Central

[21] The top five countries in terms of stock market capitalization in 2006 are the US, Japan, the UK, France and Germany. By continent, the top economies by stock market capitalization are the US (North America), Argentina (South America), Japan (Asia), the UK (Europe) and Australia (Oceania).

Table 6: Alternative Instruments

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Stock Market Volatility Interacted with: | | | | |
| | Predicted Trade | Predicted Trade and SMC | Predicted Trade and GDP | Predicted Trade | Predicted Trade |
| 2nd Stage Regression | | | | | |
| vol | 1.984*** | 1.988*** | 1.986*** | 1.751** | 1.768** |
| | [0.575] | [0.570] | [0.569] | [0.857] | [0.776] |
| Observations | 5703 | 5703 | 5703 | 5703 | 5703 |
| R-squared | 0.23 | 0.23 | 0.23 | 0.23 | 0.23 |
| 1st Stage Regression | | | | | |
| vol_shock | 0.567*** | 0.022*** | 0.023*** | 0.346*** | 0.201*** |
| | [0.066] | [0.003] | [0.003] | [0.057] | [0.035] |
| Kleibergen-Paap $F$-stat. | 73.2 | 77.1 | 75.0 | 36.5 | 32.7 |
| Country-Sector FE | yes | yes | yes | yes | yes |
| Sector-Year FE | yes | yes | yes | yes | yes |
| Country-Specific Linear Trends | yes | yes | yes | yes | yes |
| Control Variables | yes | yes | yes | yes | yes |
| Countries Included in Instrument | All | All | All | Top-5 SMC | Top-1 SMC in each Continent |

*Notes*. The regressions are estimated on pooled data across countries, sectors of reform and years. The dependent variable is the annual change in the liberalization index for a sector within country $c$. *Vol* is the one-year lag of stock market volatility in country $c$, computed as the arithmetic mean of all quarterly volatility observations for the country in a year. To construct the instrument, *vol_shock*, the one-year lag of stock market volatility in country $j \neq c$ is multiplied by: the log predicted bilateral trade between country $j$ and country $c$ (columns 1 and 4-5); the log predicted bilateral trade between country $j$ and country $c$ and the log stock market capitalization of country $j$ in 2006 (column 2); the log predicted bilateral trade between country $j$ and country $c$ and the log GDP of country $j$ in 1973 (column 3). The resulting products are averaged across all countries $j \neq c$ belonging to the set indicated in the last row of the table. The log predicted bilateral trade is constructed using the estimated coefficients from a gravity-type regression of log bilateral trade in 1972 on origin country fixed effects, destination country fixed effects, log distance, a dummy for the existence of a common border, a dummy equal to 1 if both countries are landlocked, and four dummies for common religion, common legal origin, common language and a colonial relationship. Control variables are those included in column (7) of Table 3. The standard errors, reported in square brackets, are corrected for clustering at the country level. *, ** and *** denote significance at 10, 5 and 1 per cent, respectively.

and Eastern European (CEE) countries during the transition from a communist to a market-oriented economy. More generally, reforms could have especially taken place in developing countries, which also tended to be relatively more volatile. To account for these facts, in columns (4) and (5), we re-estimate the baseline specification after excluding CEE countries and less-developed countries (LDC), respectively, from the estimation sample. The effect of volatility on reforms continue to hold equally strong in both sub-samples. Finally, to allay the concern that the effect of volatility could be confounded by common trends inducing some groups of countries to adopt reforms in different waves, we estimate our baseline specification on the split samples of advanced vs. non-advanced economies, as classified by the IMF, and of OECD members vs. non-member states. The similar coefficients reported in columns (6)-(7) and (8)-(9), respectively, suggest that even if more advanced countries may have concentrated their reforming efforts in different time periods than the less advanced ones, this has no bearing on the effects of volatility on reforms.

Table 7: Alternative Samples

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| | No US | No Large Countries (SMC) | No Large Countries (GDP) | No CEE | No LDC | Advanced Countries | Other Countries | OECD Members | Non-OECD Members |
| 2nd Stage Regression | | | | | | | | | |
| vol | 2.087*** | 1.539*** | 2.124*** | 2.203*** | 2.373*** | 2.366 | 2.097** | 3.115* | 2.792*** |
| | [0.545] | [0.507] | [0.379] | [0.554] | [0.559] | [1.457] | [0.864] | [1.677] | [0.582] |
| Observations | 5535 | 2036 | 2205 | 5493 | 4959 | 3270 | 2433 | 3224 | 2479 |
| R-squared | 0.23 | 0.33 | 0.32 | 0.22 | 0.23 | 0.27 | 0.28 | 0.27 | 0.27 |
| 1st Stage Regression | | | | | | | | | |
| vol_shock | 0.628*** | 0.974*** | 0.972*** | 0.607*** | 0.595*** | 0.705*** | 0.497*** | 0.616*** | 0.503*** |
| | [0.071] | [0.108] | [0.118] | [0.068] | [0.074] | [0.066] | [0.087] | [0.064] | [0.103] |
| Kleibergen-Paap $F$-stat. | 79.2 | 81.8 | 67.5 | 80.4 | 64.0 | 113.6 | 32.4 | 92.6 | 23.7 |
| Country-Sector FE | yes | yes | yes | yes | yes | yes | yes | yes | yes |
| Sector-Year FE | yes | yes | yes | yes | yes | yes | yes | yes | yes |
| Country-Specific Linear Trends | yes | yes | yes | yes | yes | yes | yes | yes | yes |
| Control Variables | yes | yes | yes | yes | yes | yes | yes | yes | yes |

*Notes*. The regressions are estimated on pooled data across countries, sectors of reform and years. The dependent variable is the annual change in the liberalization index for a sector within country $c$. *Vol* is the one-year lag of stock market volatility in country $c$, computed as the arithmetic mean of all quarterly volatility observations for the country in a year. *Vol_shock* is the arithmetic average of the one-year lag of stock market volatility in all countries $j \neq c$ with non-missing observations, multiplied by the log inverse bilateral distance from country $c$. Control variables are those included in column (7) of Table 3. CEE are Central and Eastern European countries; LDC are low-income and lower-middle income countries according to the World Bank classification; advanced and other countries are defined according to the International Monetary Fund classification. In column (2), large countries are defined as those whose stock market capitalization in 2006 is above the sample median; in column (3), large countries in terms of GDP in 2006 are defined accordingly. The standard errors, reported in square brackets, are corrected for clustering at the country level. *, ** and *** denote significance at 10, 5 and 1 per cent, respectively.

### 6.3 Alternative Variables Definitions and Specifications

We now consider alternative variables definitions and specifications. In Table 8, we use alternative ways of constructing the main variables. In column (1), we re-compute the key explanatory variable after excluding the quarter of maximum volatility for country $c$ in a given year.[22] In column (2), we define instead the main regressor as the volatility observed in the most volatile quarter of a year for country $c$. In both cases, we continue to find a positive and statistically significant estimate of $\beta_2$, suggesting that our main evidence is not driven by how we measure stock market volatility.

Next, we use alternative definitions of the dependent variable. A possible concern is that a small change in a liberalization index over a year may reflect a mechanical adjustment in the index rather than a true reform. One could also worry that the positive effect of volatility on reforms documented so far may be driven by small reform episodes, while large and important reforms may not be influenced by volatility. To address these concerns, we replace the dependent variable $ref_{s,c,t}$ with a dichotomous variable, which takes on value 1 when the annual change in a liberalization index is above a certain threshold, and is equal to 0 otherwise. To define the threshold, we use the sample median in column (3) and the 75th percentile of the distribution in column (4). The coefficient on volatility is positive and precisely estimated in both cases, suggesting that volatility raises the likelihood of major reforms.

We now assess the sensitivity of the baseline results to the use of alternative specifications. As shown in Appendix Table B1, our data set is unbalanced, because new countries enter the sample as data on their stock market volatility become available. To study the implications of changes in sample composition for the estimate of $\beta_2$, we estimate eq. (1) on balanced samples of countries.

[22] Specifically, we compute $vol_{c,t-1}$ as the annual average of quarterly volatility observations for country $c$ in year $t-1$, after excluding the quarter of maximum volatility for the country in that year.

Table 8: Alternative Variables Definitions

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | No Max Vol. | Only Max Vol. | Reforms Above Median | Reforms Above 75th pct |
| 2nd Stage Regression | | | | |
| vol | 2.897*** | 1.343*** | 10.552*** | 8.109** |
| | [0.894] | [0.353] | [3.169] | [3.131] |
| Observations | 5703 | 5703 | 5703 | 5703 |
| R-squared | 0.22 | 0.22 | 0.34 | 0.26 |
| 1st Stage Regression | | | | |
| vol_shock | 0.448*** | 0.966*** | 0.632*** | 0.632*** |
| | [0.089] | [0.110] | [0.069] | [0.069] |
| Kleibergen-Paap $F$-stat. | 25.2 | 76.9 | 82.9 | 82.9 |
| Country-Sector FE | yes | yes | yes | yes |
| Sector-Year FE | yes | yes | yes | yes |
| Country-Specific Linear Trends | yes | yes | yes | yes |
| Control Variables | yes | yes | yes | yes |

*Notes*. The regressions are estimated on pooled data across countries, sectors of reform and years. The dependent variable is the annual change in the liberalization index for a sector within country $c$ in columns (1) and (2), and a dummy equal to 1 if the change in the index is above the indicated threshold in columns (3) and (4). $Vol$ is the one-year lag of stock market volatility in country $c$ computed after excluding the quarter of maximum volatility in column (1); as the volatility observed in the most volatile quarter in column (2); and as the arithmetic mean of all quarterly volatility observations in columns (3) and (4). $Vol\_shock$ is the arithmetic average of the one-year lag of stock market volatility in all countries $j \neq c$ with non-missing observations, multiplied by the log inverse bilateral distance from country $c$. Control variables are those included in column (7) of Table 3. The standard errors, reported in square brackets, are corrected for clustering at the country level. *, ** and *** denote significance at 10, 5 and 1 per cent, respectively.

Our data allow us to construct three balanced samples while limiting the loss of observations. In particular, in column (1) of Table 9, we use the sample of countries with available data on stock market volatility for the whole sample period, 1973-2006. In column (2), we consider instead countries with available data on stock market volatility for the 1983-2006 period (i.e., the countries considered in column 1 plus all countries for which stock market volatility data became available between 1974 and 1983) and estimate eq. (1) using observations for these countries over 1983-2006. In column (3), we proceed analogously, considering countries with stock market volatility data available for the 1989-2006 period and using observations for these countries over 1989-2006. The coefficient on volatility remains positive and precisely estimated across the board; if anything, broadening the sample to include countries with more recent volatility data tends to lower the estimate of $\beta_2$. In column (4), we perform a complementary exercise by excluding country-year pairs with missing data on reforms for some sectors. Using a balanced panel of reforms across countries, sectors and years also has no bearing on the coefficient $\beta_2$.

By exploiting yearly changes in the liberalization indices, the baseline specification in eq. (1) fully uses the information contained in the data and maximizes statistical power. One may be

concerned, however, that the annual changes in the liberalization indices could be noisy, as some sectors are not frequently subject to reform. Moreover, as long as reforms take more than one year to be completed, the annual change in an index may not capture the full extent of the reform, and the coefficient on the first lag of volatility may not capture the full effect of this variable on the reform. Hence, we estimate an alternative version of eq. (1), in which the dependent variable is the change in a liberalization index over a window of three or five years and all explanatory variables are lagged accordingly. The specification reads as follows:

$$ref^{\tau}_{s,c,t} = \eta_{s,c} + \eta_{s,t} + \beta_1 lib_{s,c,t-\tau} + \beta_2 vol_{c,t-\tau} + \boldsymbol{\beta}_3 \mathbf{X}_{s,c,t-\tau} + \beta_{4,c} t + \epsilon_{s,c,t}, \tag{3}$$

where $ref^{\tau}_{s,c,t} \equiv lib_{s,c,t} - lib_{s,c,t-\tau}$ and $\tau \in \{3, 5\}$.[23] We instrument $vol_{c,t-\tau}$ using the $\tau$th lag of the instrument in eq. (2), namely,

$$vol\_shock_{c,t-\tau} = \sum_{j \in \Omega_{-c}} \frac{vol_{j,t-\tau}}{N_{-c}} \times \ln Int_{c,j},$$

where $Int_{c,j}$ is the inverse bilateral distance between countries $c$ and $j$. The estimates of eq. (3) are reported in columns (5) and (6) of Table 9. As anticipated, the $R^2$ substantially increases compared to when using annual changes. Aside from the improved fit of the model, the two specifications confirm our main evidence regarding the effect of volatility on reforms. In particular, the coefficient $\beta_2$ estimated from eq. (3) is always positive and highly statistically significant, suggesting that our main evidence holds when defining reforms over longer time horizons. Quantitatively, the point estimate of $\beta_2$ tends to increase with the length of the window, in line with the fact that some reforms take more than one year to be completed.

Finally, we revert to the baseline specification and study how the effect of volatility varies across the reform areas discussed in Section 3.1. To this purpose, in column (7), we estimate eq. (1) on the sub-sample of foreign-oriented reforms occurring in the trade and current account sectors; in column (8), we focus on product market reforms (agriculture and telecom/electricity); and in column (9), we consider financial sectors reforms (domestic finance and capital account). The coefficient $\beta_2$ is positive in all cases. The point estimates imply that the effect of volatility is somewhat stronger and more precisely estimated for reforms in foreign and financial sectors than in product markets, but the confidence intervals around the three coefficients largely overlap with each other. The lower precision for product market reforms is not too unsurprising, given that reforms in agriculture are very rare and product market liberalizations started towards the end of the sample.[24] Overall, given the lack of significant heterogeneity across reform types and the infrequent nature of these events, we opt for a baseline specification that pools observations across sectors so as to maximize statistical

[23] The dummy for future membership of the EU is still equal to 1 at time $t$ if country $c$ is a member of the EU at time $t + 2$.

[24] We have further extended the analysis by estimating eq. (1) separately on each of the six sectors of reform. Despite the limited number of observations, these regressions delivered a positive, albeit less precise, estimate of $\beta_2$ for each of the six indexes. These results, omitted to save space, are available upon request.

Table 9: Alternative Specifications

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| | Balanced Panel: 1973-2006 | Balanced Panel: 1983-2006 | Balanced Panel: 1989-2006 | Balanced Panel: All Sectors | 3-Year Windows | 5-Year Windows | Foreign Sectors | Product Markets | Financial Sectors |
| 2nd Stage Regression | | | | | | | | | |
| vol | 3.118*** | 1.857*** | 0.919** | 2.106*** | 4.877*** | 5.375*** | 2.387** | 1.554 | 1.991** |
| | [0.592] | [0.555] | [0.443] | [0.531] | [1.156] | [1.131] | [1.136] | [1.228] | [0.934] |
| Observations | 2415 | 3116 | 3347 | 5100 | 5081 | 4471 | 1952 | 1805 | 1946 |
| R-squared | 0.24 | 0.25 | 0.28 | 0.24 | 0.49 | 0.67 | 0.28 | 0.28 | 0.26 |
| 1st Stage Regression | | | | | | | | | |
| vol_shock | 0.616*** | 0.655*** | 0.678*** | 0.646*** | 0.639*** | 0.633*** | 0.626*** | 0.638*** | 0.624*** |
| | [0.108] | [0.088] | [0.057] | [0.063] | [0.076] | [0.086] | [0.073] | [0.067] | [0.073] |
| Kleibergen-Paap $F$-stat. | 32.8 | 55.3 | 140.8 | 103.8 | 70.3 | 54.4 | 73.1 | 90.7 | 74.1 |
| Country-Sector FE | yes | yes | yes | yes | yes | yes | yes | yes | yes |
| Sector-Year FE | yes | yes | yes | yes | yes | yes | yes | yes | yes |
| Country-Specific Linear Trends | yes | yes | yes | yes | yes | yes | yes | yes | yes |
| Control Variables | yes | yes | yes | yes | yes | yes | yes | yes | yes |

*Notes*. The regressions are estimated on pooled data across countries, sectors of reform and years. The dependent variable is the change in the liberalization index for a sector within country $c$; the change is computed over one year in columns (1)-(4) and (7)-(9), over three years in column (5) and over five years in column (6). *Vol* is lagged stock market volatility in country $c$, computed as the arithmetic mean of all quarterly volatility observations for the country in a year; columns (1)-(4) and (7)-(9) use a one-year lag, column (5) a three-year lag and column (6) a five-year lag. *Vol_shock* is the arithmetic average of the one-year, three-year or five-year lag of stock market volatility in all countries $j \neq c$ with non-missing observations, multiplied by the log inverse bilateral distance from country $c$. Control variables are those listed in column (7) of Table 3. All time-varying controls are lagged one year in columns (1)-(4) and (7)-(9), three years in column (5) and five years in column (6); EU membership always enters with a two-year lead. In column (1), the sample spans the 1973-2006 period and consists of countries with available information on stock market volatility between 1973 or earlier years and 2006; the samples in columns (2) and (3) are defined accordingly. The sample in column (4) consists of country-year pairs for which the change in the liberalization index is non-missing for all six sectors of reform. The regressions in columns (7), (8) and (9) are estimated on observations for, respectively, the trade and current account sectors, the agriculture and product market sectors, and the domestic finance and capital account sectors. The standard errors, reported in square brackets, are corrected for clustering at the country level. *, ** and *** denote significance at 10, 5 and 1 per cent, respectively.

power.

## 7 Threats to Identification

In this section, we deal with the main remaining threats to identification. The identifying assumption behind our empirical strategy is that, after controlling for sector-year fixed effects, country-sector fixed effects, country-specific linear trends and a wealth of covariates, no unobservable remains that correlates with the instrument and influences the adoption of reforms across countries and sectors. As mentioned in Section 4, two types of confounding factors might endanger the exclusion restriction: (i) other characteristics of foreign countries that correlate with their volatility and have a direct influence on country $c$'s reforms; and (ii) trends or shocks that could determine a differential pace of reforms across countries and sectors independent of volatility.

To deal with the first confounder, our baseline specification includes numerous controls for cross-country spillovers. The influence of the second confounder is partly mitigated by the sector-year fixed effects and the country-specific linear trends included throughout the analysis. Yet, these controls do not rule out the effect of shocks to specific countries within sectors and of non-linear trends. We now discuss the role of these remaining potential confounders for our results. We use alternative strategies for accommodating the impact of these confounding factors and study their potential implications for our parameter of interest $\beta_2$.

Table 10: Threats to Identification: Contemporaneous Shocks

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| 2nd Stage Regression | | | | | |
| vol | 1.905*** | 2.525*** | 2.780*** | 2.005*** | 3.184*** |
| | [0.478] | [0.655] | [0.623] | [0.593] | [0.877] |
| Observations | 5703 | 5703 | 5703 | 5703 | 5703 |
| R-squared | 0.26 | 0.25 | 0.24 | 0.249 | 0.25 |
| 1st Stage Regression | | | | | |
| vol_shock | 0.673*** | 0.550*** | 0.528*** | 0.637*** | 0.622*** |
| | [0.075] | [0.076] | [0.121] | [0.051] | [0.186] |
| Kleibergen-Paap $F$-stat. | 80.6 | 51.7 | 19.1 | 154.4 | 11.2 |
| Country-Sector FE | yes | yes | yes | yes | yes |
| Sector-Year FE | yes | yes | yes | yes | yes |
| Country-Specific Linear Trends | yes | yes | yes | yes | yes |
| Control Variables | yes | yes | yes | yes | yes |
| Controls for Shocks to: | aggregate reform | GDP growth | inflation | democr. | geographical area |

*Notes*. The regressions are estimated on pooled data across countries, sectors of reform and years. The dependent variable is the annual change in the liberalization index for a sector within country $c$. *Vol* is the one-year lag of stock market volatility in country $c$, computed as the arithmetic mean of all quarterly volatility observations for the country in a year. *Vol_shock* is the arithmetic average of the one-year lag of stock market volatility in all countries $j \neq c$ with non-missing observations, multiplied by the log inverse bilateral distance from country $c$. Control variables are those listed in column (7) of Table 3. The regressions also control for full sets of interactions between the year dummies and dummies for: quintiles of the change in the aggregate liberalization index of country $c$ over the sample period (column 1); quintiles of the change in real per-capita GDP of country $c$ over the sample period (column 2); quintiles of the change in inflation in country $c$ over the sample period (column 3); quintiles of the change in the polity2 index of country $c$ over the sample period (column 4); and seven geographical areas (column 5). The standard errors, reported in square brackets, are corrected for clustering at the country level. *, ** and *** denote significance at 10, 5 and 1 per cent, respectively.

We start with unobserved shocks contemporaneous to reforms. To begin with, we consider the possibility that countries experiencing similar changes in certain observable characteristics may be hit by similar unobservable shocks that influence their reform process. To accommodate this type of shock, we divide countries into five bins corresponding to the quintiles of the overall change in a certain characteristic over the sample period. We then interact a dummy for each bin with a full set of year dummies. Adding these interactions to the baseline specification soaks up shocks hitting in a similar manner countries that experienced a similar change in a given characteristic over the sample period. Consequently, identification only exploits the remaining variation in volatility occurring within a given year across countries falling in the same bin.

We first define the bins based on the change in the aggregate liberalization index over the entire sample period. This exercise accounts for the fact that some groups of countries may have been more or less successful than others at reforming their economies due to some common shock experienced over the time span of our analysis. As shown in column (1) of Table 10, however, the coefficient of interest barely changes when adding these interactions. Next, we construct the bins based on the

overall change in real per-capita GDP (column 2) and inflation (column 3) over the sample period. This exercise accounts for the fact that countries experiencing similar changes in macroeconomic conditions may be subject to similar shocks that also influence reforms. Also in this case, however, we do not find noteworthy changes in the coefficient $\beta_2$. In column (4), we construct instead the bins using the change in the polity2 index over the entire sample period. This exercise absorbs reform shocks hitting countries that experienced similar changes in their political regime over the time span of our analysis. The coefficient $\beta_2$ is unchanged.

Countries belonging to the same geographical area may be hit by common reform shocks independent of the evolution of other observable characteristics. For instance, civic movements may coordinate their actions across borders, eventually leading to waves of simultaneous reforms in a region. To account for this type of shock, in column (5), we extend the baseline specification by adding a full set of interactions between the year dummies and indicators for seven geographical areas identified by the World Bank.[25] These interactions absorb shocks hitting all countries in the same area, so identification now only exploits the remaining variation in volatility within a given region. The coefficient $\beta_2$ remains positive and very precisely estimated also in this very demanding specification, suggesting our evidence not to be driven by shocks to specific geographical areas.

Next, we study the role of underlying trends. In columns (1)-(7) of Table 11, we allow for the possibility that reforms followed heterogeneous trends across countries characterized by different initial conditions. To this purpose, following Goldberg et al. (2010), we extend the baseline specification by adding a full set of interactions between the year dummies and the first-year value of several country-level variables: aggregate reform index (column 1), real per-capita GDP (column 2), inflation (column 3), polity2 index (column 4), a dummy for left-wing orientation of the government (column 5), a dummy for membership of the OECD (column 6) and a dummy for advanced economies (column 7). The coefficient $\beta_2$ is remarkably stable across all these specifications, suggesting that our main evidence is not driven by reforms following heterogeneous trends across countries characterized by different initial conditions.

Overall, the previous evidence suggests that our results are unlikely to reflect the confounding effect of shocks and trends influencing the pace of reforms independent of volatility. We now perform two additional exercises to further raise confidence in our 2SLS estimates. In column (8) of Table 11, we implement a falsification test by regressing current reforms, $ref_{s,c,t}$, on future volatility, $vol_{c,t+1}$. We include the same controls as in the baseline specification, and instrument future volatility using the $t+1$ level of the instrument defined in eq. (2). It would be problematic for our identification strategy if future volatility predicted current reforms: this would raise the concern that our evidence could be driven by trends in reforms that anticipate an increase in volatility, or by long-standing factors jointly driving volatility and reforms over time. Consistent with the correlations shown in Figure 3, however, we find that the coefficient on $vol_{c,t+1}$ is very small and imprecisely estimated.

[25] The seven areas are: (1) East Asia and Pacific; (2) Europe and Central Asia; (3) Latin America and the Caribbean; (4) Middle East and North Africa; (5) North America; (6) South Asia; and (7) Sub-Saharan Africa.

Table 11: Threats to Identification: Underlying Trends

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| 2nd Stage Regression | | | | | | | | |
| vol | 1.800*** | 2.212*** | 2.389*** | 2.045*** | 1.788*** | 2.289*** | 2.263*** | |
| | [0.431] | [0.537] | [0.515] | [0.491] | [0.474] | [0.562] | [0.705] | |
| future_vol | | | | | | | | -0.421 |
| | | | | | | | | [0.441] |
| Observations | 5703 | 5703 | 5703 | 5703 | 5703 | 5703 | 5703 | 5700 |
| R-squared | 0.24 | 0.23 | 0.23 | 0.24 | 0.24 | 0.23 | 0.23 | 0.23 |
| 1st Stage Regression | | | | | | | | |
| vol_shock | 0.683*** | 0.631*** | 0.657*** | 0.645*** | 0.640*** | 0.584*** | 0.548*** | |
| | [0.065] | [0.072] | [0.074] | [0.063] | [0.067] | [0.077] | [0.086] | |
| future_vol_shock | | | | | | | | 0.918*** |
| | | | | | | | | [0.110] |
| Kleibergen-Paap $F$-stat. | 109.7 | 77.6 | 77.9 | 104.0 | 92.3 | 57.0 | 40.2 | 70.0 |
| Country-Sector FE | yes | yes | yes | yes | yes | yes | yes | yes |
| Sector-Year FE | yes | yes | yes | yes | yes | yes | yes | yes |
| Country-Specific Linear Trends | yes | yes | yes | yes | yes | yes | yes | yes |
| Control Variables | yes | yes | yes | yes | yes | yes | yes | yes |
| Controls for Trends: | by initial aggr. ref. | by initial GDP | by initial inflation | by initial degree of democr. | by initial political orient. | by initial OECD member. | by develop. group | - |

*Notes*. The regressions are estimated on pooled data across countries, sectors of reform and years. The dependent variable is the annual change in the liberalization index for a sector within country $c$. *Vol* and *future_vol* are, respectively, the one-year lag and the one-year lead of stock market volatility in country $c$, computed as the arithmetic mean of all quarterly volatility observations for the country in a year. *Vol_shock* and *future_vol_shock* are the arithmetic averages of the one-year lag and one-year lead, respectively, of stock market volatility in all countries $j \neq c$ with non-missing observations, multiplied by the log inverse bilateral distance from country $c$. Control variables are those included in column (7) of Table 3. The regressions in columns (1)-(7) also include a full set of interactions between the year dummies and: the first-year value of the change in the aggregate liberalization index of country $c$ (column 1); the first-year value of real per-capita GDP of country $c$ (column 2); the first-year value of inflation in country $c$ (column 3); the first-year value of the polity2 index of country $c$ (column 4); the first-year value of a dummy for left-wing orientation of the government in country $c$ (column 5); the first-year value of a dummy for membership of the OECD by country $c$ (column 6); and a dummy for whether country $c$ is an advanced economy (column 7). The standard errors, reported in square brackets, are corrected for clustering at the country level. *, ** and *** denote significance at 10, 5 and 1 per cent, respectively.

That future volatility does not predict current reforms helps strengthening the view that our evidence captures the effects of period-specific shocks to volatility rather than other time-varying confounds.

In a second exercise, we change perspective and start from the premise that some confounding factor may exist, so that our instrument may be correlated with the error term. Then, following an approach developed by Conley, Hansen and Rossi (2012), we study how strong a violation of the exclusion restriction would have to be for inference about $\beta_2$ to become uninformative about the causal effect of volatility on reforms. If we found that inference remains informative even for sizable violations of the exclusion restriction, this would further raise confidence in our baseline results, suggesting that they are not crucially driven by confounding factors.

To briefly illustrate the idea behind the approach of Conley, Hansen and Rossi (2012) using our set-up, consider the following version of eq. (1):

$$ref_{s,c,t} = \eta_{s,c} + \eta_{s,t} + \beta_1 lib_{s,c,t-1} + \beta_2 vol_{c,t-1} + \boldsymbol{\beta}_3 \mathbf{X}_{s,c,t-1} + \beta_{4,c} t + \lambda vol\_shock_{c,t-1} + \epsilon_{s,c,t},$$

where $\lambda$ is a parameter measuring the size of the violation of the exclusion restriction. The results presented so far are based on the standard IV assumption that $\lambda = 0$. However, if the exclusion

restriction is violated, so that $\lambda \neq 0$, inference about $\beta_2$ can still be performed, provided that alternative priors can be formed about $\lambda$ and conditional on the assumed values of this parameter. This can be done by estimating the following specification

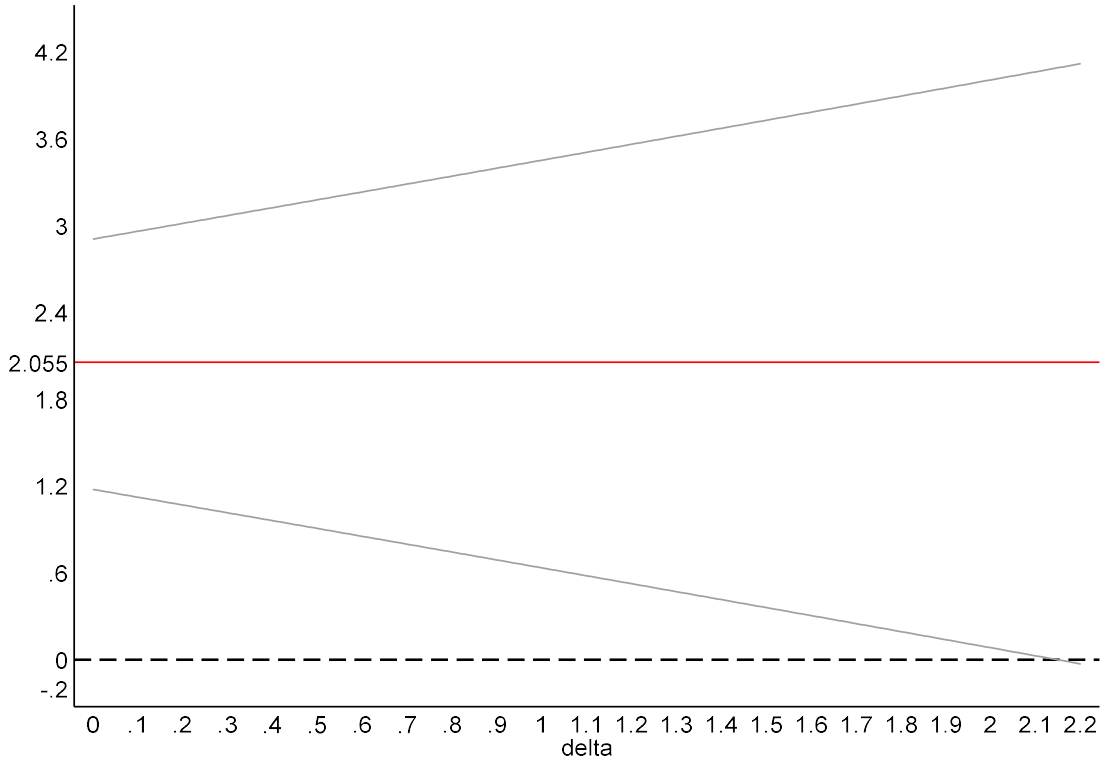
$$(ref_{s,c,t} - \lambda vol\_shock_{c,t-1}) = \eta_{s,c} + \eta_{s,t} + \beta_1 lib_{s,c,t-1} + \beta_2 vol_{c,t-1} + \boldsymbol{\beta}_3 \mathbf{X}_{s,c,t-1} + \beta_{4,c} t + \epsilon_{s,c,t}$$

with 2SLS, using $vol\_shock_{c,t-1}$ as an instrument for $vol_{c,t-1}$. By varying the prior about $\lambda$, we can assess how inference about $\beta_2$ would be influenced by different degrees of violation of the exclusion restriction. We can also study how strong a violation would have to be for inference to become completely uninformative about the causal effect of uncertainty on reforms. Conley, Hansen and Rossi (2012) emphasize that, because the sensitivity of the 2SLS estimator to violations of the exclusion restriction is inversely related to the strength of the instrument, the same value of $\lambda$ implies a smaller decrease in the precision of the estimate of $\beta_2$ (compared to the case in which $\lambda = 0$) the stronger is the first-stage relationship.

We implement the above approach for several values of $\lambda$ and compare the resulting inference about $\beta_2$. To this purporse, we set $\lambda$ to be a function of a parameter $\delta$ that we progressively raise so as to generate increasingly stronger violations of the exclusion restriction. In particular, $\delta = 0$ will correspond to the benchmark case in which the exclusion restriction is satisfied; $\delta = x > 0$ will correspond instead to a violation of the exclusion restriction such that a change in $vol\_shock_{c,t-1}$ by one interquartile range has a direct effect on $ref_{s,c,t}$ equal to the effect of a change in $vol_{c,t-1}$ by $x$ interquartile ranges. We increase $\delta$ by intervals of 0.01 starting from 0. For each resulting value of $\lambda$, we estimate the confidence interval of $\beta_2$ for both the lower and the upper end of the support $[-\lambda, \lambda]$ and compute the final confidence interval of $\beta_2$ as the union of the two confidence intervals.[26]

The results are shown in Figure 4, which plots the 90 per cent confidence interval of $\beta_2$ corresponding to different values of $\delta$. When $\delta = 0$, the confidence interval is $[1.176, 2.907]$. As $\delta$ departs from this benchmark, the confidence interval progressively widens. However, thanks also to the strong predictive power of the instrument at the first stage, the decrease in precision proceeds very slowly and the confidence interval of $\beta_2$ starts including zero only when $\delta > 2.14$. Hence, for our parameter of interest to become statistically not significant, and thus uninformative about the causal impact of volatility on reforms, the direct effect of $vol\_shock_{c,t-1}$ on $ref_{s,c,t}$ would have to be more than twice as large as the effect of a commensurate exogenous change in $vol_{c,t-1}$. We conclude

[26] In particular, $\lambda \equiv 2.055 \times \delta/6$, where 2.055 is the baseline 2SLS estimate of $\beta_2$ (see column 3 of Table 4) and the interquartile range of $vol_{c,t-1}$ relative to $vol\_shock_{c,t-1}$ is approximately one sixth. Besides this "union of confidence intervals" approach, Conley, Hansen and Rossi (2012) discuss other strategies that use more prior information about $\lambda$, e.g., using priors also on the distribution of $\lambda$ within the support or applying Bayesian techniques that use priors over all model parameters and assumptions about the error distribution. Compared to just specifying the support of $\lambda$, these alternative approaches impose additional parametric restrictions, and thus yield narrower confidence intervals around the treatment parameter. Accordingly, these approaches are advisable when researchers have additional information allowing them to confidently give a higher or lower likelyood to specific types of violation of the exclusion restriction in a certain application. Lacking this additional prior information, we remain agnostic about the distribution of $\lambda$ within a given support, so as to obtain the most conservative inference about $\beta_2$.




*Notes.* The figure plots 90 per cent confidence intervals around the baseline coefficient on *vol* (obtained using the specification in column 3 of Table 4 and indicated with a red line in the graph) for different priors about a potential violation of the exclusion restriction. Priors are described by the parameter *delta* reported on the horizontal axis: *delta* equal to zero implies that the exclusion restriction is satisfied; *delta* equal to *x>0* corresponds to a violation of the exclusion restriction such that a change in the instrument *vol_shock* by 1 interquartile range has a direct effect on the dependent variable *ref* equal to the effect of a change in *vol* by *x* interquartile ranges. The confidence intervals are based on standard errors corrected for clustering at the country level.

Figure 4: Sensitivity of Inference about the Effect of Volatility to Violations of the Exclusion Restriction

that even substantial, and likely implausible, relaxations of the exclusion restriction would leave inference informative about the effect of volatility on reforms.

## 8 Conclusions

How does economic uncertainty affect the adoption of structural reforms? This paper is the first to answer this question empirically. Using an exhaustive panel dataset on structural reforms and widely-used data on stock market volatility, we have shown that economic volatility is positively correlated with liberalizations in six sectors of the economy. This positive correlation is robust to the inclusion of a large set of fixed effects and a wide host of controls for political institutions as well as economic and financial crises. To identify causality, we have used an instrument that exploits exogenous differential variation in countries' exposure to foreign volatility shocks stemming from pre-determined and time-invariant bilateral characteristics.

Our results have important implications. First, they suggest that times of market turmoil, which are characterized by a high degree of uncertainty, may facilitate the implementation of reforms that

would otherwise not pass. Second, one hypothesis consistent with our findings is that economic volatility may alleviate electoral concerns when implementing unpopular reforms, an interpretation that can be rationalized by agency models with asymmetric information.[27] If confirmed, this would suggest that promoting transparency, guaranteeing media independence and educating voters could help making reforms more politically viable. We believe that providing more evidence in this direction would be a desirable avenue for future research.

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[27] See Bonfiglioli and Gancia (2013). Additional evidence in support of agency models is provided by Shi and Svensson (2006), who show that political budget cycles take place mainly in countries where voters cannot effectively monitor fiscal policies, and by Brender and Drazen (2008), who show that high growth increases the re-election probability especially in less developed countries. Media scrutiny has been found to improve the incentives of politicians (e.g., Snyder and Strömberg, 2010), but its effect on reforms has not been studied extensively. Ponzetto (2011) shows that more information promotes trade liberalization.

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