

Structural Reforms and Labor Market Outcomes

International Panel Data Evidence

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Abstract

This paper explores the impact of structural reforms on a comprehensive set of macro-level labor-market outcomes, including the unemployment rate, the average wage index, and overall and female employment levels and labor force participation rates. Together these outcome variables capture the overall health of the labor market and the aggregate welfare of workers. Yet, there seems to be no other comprehensive empirical investigation in the existing literature of the impact of structural reforms at the cross-country macro level on labor-market outcomes other than the unemployment rate. Data were collected from a variety of sources, including the World Bank World Development Indicators, the International Monetary Fund International Financial Statistics, and the International Labor Organization Key

Indicators of the Labor Market. The resulting dataset covers up to 88 countries, the majority being developing, for 10 years on either side of structural reforms that took place between 1960 and 2001. After documenting the average trends across countries in the labor-market outcomes up to 10 years on either side of each country's structural reform year, the authors run fixed-effects ordinary least squares as well as instrumental variables regressions to account for the likely endogeneity of structural reforms to labor-market outcomes. Overall the results suggest that structural reforms lead to positive outcomes for labor. Unlike related literature, the paper does not find conclusive evidence on unemployment. Redistributive effects in favor of workers, along the lines of the Stolper-Samuelson effect, may be at work.

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Structural Reforms and Labor Market Outcomes: International Panel Data Evidence¹

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1. Introduction

The debate over the effects of structural reforms has been revived in recent years, in part as a result of the ongoing debt crises in European economies. As countries engage in fiscal stabilization through austerity policies, pressure has been mounting for debt-ridden economies to undertake structural reforms in conjunction with macroeconomic stabilization. The main argument in favor of structural reforms is that they tend to improve productivity and thus may be one of the few potential sources of growth in the context of fiscal austerity. From the viewpoint of developing countries, we have seen this move before, when numerous economies undertook both fiscal stabilization and structural reforms throughout the 1980s and 1990s (see Figure 1).

This paper contributes to the literature by focusing on the impact of structural reforms on labor-market outcomes at the country level. Research on the impact of structural reforms on a comprehensive set of labor-market outcomes is needed to inform current debates about the potential impacts of comprehensive structural reforms on workers. The outcomes that we study are the unemployment rate, employment level, average wage index, labor force participation rates (overall and female) and female employment level. The data on labor-market outcomes as well as on the dates of countries' reforms to assess how labor-market trends were influenced by the advent of structural reforms within countries are publicly available. We adopt a structural reform index from the literature that proxies for the date when countries reached a threshold of broad reforms including macroeconomic stabilization, privatization, trade opening, as well as the end of interventionist states such as communism. In principle, the liberalization date is the date after which all of the Sachs-Warner reform criteria are continuously met.

These labor-market outcomes jointly capture the aggregate welfare of workers and the overall health of the labor market. In a labor market with full employment, wages and employment are the only two outcome variables. For workers who are employed, their wage will determine their wellbeing, and for a given wage, greater employment leads to greater welfare of workers as a whole. When frictions in job search and matching are introduced, the set of all outcome variables describing the labor market also includes the unemployment rate. When the possibility of opting out of the labor force (exercising the option of not looking for a job) is further introduced, the labor force participation rate becomes a fourth outcome variable. Changes in employment can now take place through changes in the unemployment rate for a given labor force participation rate or through changes in the labor force participation rate for a given unemployment rate (or a combination of changes in unemployment and labor force participation). An increase in the labor force participation rate also indicates a smaller proportion of discouraged workers and thus an improvement in their overall wellbeing. Yet in the case of the decision regarding whether or not to participate in the labor market (whether to look for a job or not), the factors affecting men and women can be different (maternity, raising children, discrimination, etc.). Therefore, we separately look at the female employment and labor force participation rates.

To our knowledge, there seems to be no other comprehensive empirical investigation in the existing literature of the impact of structural reforms at the cross-country macro level on labor-market outcomes other than the unemployment rate. On the theoretical side, however, there is a long list of papers that analyze the impact of trade policy on labor-market outcomes. The models of trade and search unemployment are all extensions of the basic search unemployment models of Mortensen and Pissarides

(Pissarides 2000). Most take the labor force participation rate as 100 percent and focus on the unemployment rate and the wage, which decrease and increase in labor-market tightness, respectively. However, the lack of comparable wage data across countries thus far has prevented researchers from also empirically looking at the impact of trade on the overall wage at the cross-country macro level. Pissarides (2000) also extends the basic model to include an endogenous labor force participation rate, and the possible impact of structural reforms on participation rates is also discussed in this paper. In this context, another literature shows the discrimination-reducing impact of product market competition (Becker 1957). Trade and other structural reforms, therefore, would encourage women to look for work and should have a positive impact on female labor force participation. This effect is also empirically tested herein using female employment and labor force participation rates. This paper thus fills important gaps in the literature.

The analysis first documents average trends (across countries) in our labor-market outcomes up to 10 years on either side of each country's reform year. This is done by controlling for country fixed effects and another specification also controls for real GDP, the labor force participation rate and the working age population. On average we find that unemployment rates are higher after reforms than before reforms. In addition, we find that employment is higher on average and its trend is more positive after as compared to before reforms. In the case of wages and labor force participation rates (overall and female), it is the trend that takes an upward turn after reforms.

In turn, we estimate fixed-effects ordinary least squares as well as instrumental variables regressions of our labor-market outcomes on a reform dummy variable, a time trend, square of the time trend and the same set of controls mentioned above. Focusing on wages, the Stolper-Samuelson theorem implies that in developing countries, which generally are abundant in unskilled labor, liberalization will increase the wage rate of unskilled workers. However, the assumptions under which this result holds, including free mobility of labor across sectors, can be questioned. When labor does not move across sectors (is sector-specific) and earns a higher wage in the protected manufacturing sectors, the fear is that removing protection will instead lower these workers' wages. Also, the presence of adjustment costs could lead to higher unemployment after reforms at least in the short run, if not in the long run. While trade reforms are a very important component of structural reforms, other aspects of structural reforms include macroeconomic stabilization, privatization, deregulation, etc. Such reforms provide a better macroeconomic environment along with better economic incentives for everyone, leading to higher productivity and therefore higher wages and employment. However, moving to a better environment also involves short-run adjustments with temporary adverse consequences for labor-market outcomes.

The effects shown by our regressions for the employment level and the wage index are quite strong and conclusive: even in the presence of a strongly significant and positive time trend that we control for, reforms have a positive and significant impact on employment and wages. The interpretation of our instrumental variables estimates of the effect of reforms on unemployment rates instead range from inconclusive to some weak support for the presence an unemployment-reducing effect of reforms. Because the labor force participation rate of the formal sector is included in the regressions and due to the weaker results on unemployment after reforms, the results suggest that some of the increase in employment may be coming from informal workers joining the labor force.

The evidence on the effect of structural reforms on labor force participation rates (overall and female) is somewhat inconclusive even though we find that there is a positive time trend and positive effect of real GDP. Finally, there is some indication that structural reforms may have increased female employment. Because we controlled for the time trend and real GDP, our results show the impact of structural reforms on labor-market outcomes beyond what happens through the impact on growth. Redistributive effects in favor of workers, along the lines of the Stolper-Samuelson effect, may be at work.

While the literature that finds evidence of a positive effect of structural reforms and greater openness on growth is well established, to our knowledge, there are only two major cross-country empirical studies that look at the impact of trade policy on unemployment rates.² One is the paper by Dutt, Mitra and Ranjan (2009) and the other is by Felbermayr, Prat and Schmerer (2011a).³ Both papers show that countries that have less protectionist (more open) trade policies have lower unemployment rates. This is true both without any controls and after controlling for other policies and institutions that have a more direct impact on labor markets. Dutt, Mitra and Ranjan (2009) also find that the short-run impact of trade reforms is an increase in the unemployment rate followed by a reduction in the long run to a lower steady-state unemployment rate. In addition, there are papers studying the role of other kinds of policies and institutions, namely labor-market policies and institutions, in the determination of unemployment rates. Scarpetta (1996) uses a panel of Organisation for Economic Co-operation and Development (OECD) countries to study the role of labor-market policies and institutions on unemployment rates and the speed of adjustment of the labor market. Another paper looking at the role of labor-market institutions on unemployment rates in the OECD is Nickell, Nunziata and Ochel (2005). Blanchard and Wolfers (2000) study the interaction between macroeconomic shocks (such as shocks to total factor productivity and the interest rate) and labor-market institutions (such as the degree of employment protection) in the determination of unemployment rates in Europe.

Our paper differs from Dutt, Mitra and Ranjan (2009) and Felbermayr, Prat and Schmerer (2011a) along several dimensions. The first major and probably the main difference is that while these papers focus on the unemployment rate, our paper looks at a multitude of labor-market outcomes. Secondly, unlike the previous studies, we include sample periods in our panel, such that for each country we cover a maximum of 10 years before reform and a maximum of 10 years after reforms. While this approach can limit the number of observations, it gives us greater confidence in our results for the countries we are able to study. Thirdly, unlike the two papers mentioned above, our focus is not limited to trade policies. We look at the impact of structural reforms in general (of which trade reform is just one component) on labor-market outcomes. Finally, we are able to use credible instrumental variables for our structural reform variable. It is well known that structural reforms are endogenous to macroeconomic policies and conditions and therefore to unemployment rates and other labor-market outcomes. We draw on the political-economy literature to come up with a fairly extensive set of

² Included in the literature that finds evidence of structural reforms leading to growth is the seminal work of Sachs and Warner (1995). While the paper's measurement and estimation framework came under criticism, Wacziarg and Welch (2008) show that liberalization has, on average, robust positive effects on growth. See Baldwin (2003) for a survey of the literature.

³ See also Hasan et al. (2012) for an intra-country study on cross-state and cross-industry variations in unemployment in the context of trade reforms.

instruments. The overidentification of the estimated equations allows us to econometrically check the quality of these instruments.

Although similar in spirit, there are reasons to expect our results of the impact of structural reforms on unemployment to differ from these papers' results of the impact of trade on unemployment. Dutt, Mitra and Ranjan (2009) and Felbermayr, Prat and Schmerer (2011a) empirically search for a causal relationship between openness and unemployment and both papers find that more open economies tend to have lower unemployment levels. In contrast, we find weak evidence for a causal impact of structural reforms on unemployment, at least within 10 years of reform. Our broader focus on structural reforms rather than just trade policy along with the coverage of a longer time period (which spans symmetrically from 10 years before to 10 years after each reform) leads us to different results. The weakness, however, is that we do not go beyond 10 years after reforms. Structural reforms involve short-run adjustments during which we might see some adverse consequences on labor-market outcomes. In fact, Dutt, Mitra and Ranjan (2009) do find the short-run impact of trade reforms to be an increase in the unemployment rate followed by a decrease in the long run to a lower steady-state unemployment rate.

The remainder of the paper proceeds as follows. Section 2 discusses related empirical literature and Section 3 discusses theoretical predictions of the impacts of structural reforms on labor-market outcomes and why structural reforms are likely endogenous to these outcomes. Section 4 details the econometric methodology that accounts for this endogeneity. Section 5 presents the data and Section 6 the results. Section 7 concludes.

2. Existing Related Empirical Evidence

Krugman (1993) argued that "...the level of employment is a macroeconomic issue, depending in the short run on aggregate demand and depending in the long run on the natural rate of unemployment, with microeconomic policies like tariffs having little net effect." Subsequent empirical work as well as theoretical developments at the macro level has shown this not to be the case. For example, Trefler (2004) clearly provides evidence of a reduction in employment (and an increase in the unemployment rate) in Canada arising from the short-run adjustment costs upon the signing and implementation of the NAFTA. Recent cross-country studies (Dutt, Mitra and Ranjan 2009 and Felbermayr, Pratt and Schmerer 2011a) show how unemployment rates and trade protection are positively related.

The micro literature also suggests that trade and trade liberalization impact labor-market outcomes, but with heterogeneous results across countries, sectors, and firm and worker characteristics. Autor, Dorn and Hanson (2013) show that in the United States, rising imports from low-income countries including China explains one-quarter of the aggregate decline in manufacturing employment, leading to higher unemployment, lower labor force participation, and lower wages in local labor markets with import-competing manufacturing industries. Looking specifically at trade reforms, Currie and Harrison (1997) show that changes in Morocco's trade policies affected manufacturing employment for export-oriented and publically-owned firms, yet Harrison and Hanson (1999) find a small impact of trade reform on employment in Mexico and Morocco. In Colombia, trade reform also had little impact on employment structure across industries (Goldberg and Pavcnik 2005a). When labor faces costly

mobility across sectors, and industry affiliation is an important determinant of workers' earnings, then the impact of trade reform might be more pronounced on wages than employment.

The micro literature, however, also finds differing results of the effect of trade liberalization on industry wages. Slaughter (2001) shows that a decline in transport costs in the United States had no impact on regional wages or inter-regional wage convergence. Goldberg and Pavcnik (2005) find that reductions in output tariffs reduce industry wage premiums in Colombia, while in Brazil Pavcnik et al. (2004) find insignificant effects of a decline in output tariffs on industry wage premiums, even though industry affiliation is an important component of worker earnings. Amiti and Davis (2011) reconcile these differences by allowing the effect of tariff cuts on firm wages to depend on the firm's export and import orientation, raising workers' wages within firms that export and use imported inputs. The authors, however, assume homogenous labor and perfect labor markets.

There is evidence from the micro literature that the impact of trade on workers can depend on worker characteristics, such as skill level and formality status. Large rice price increases after trade reforms in Vietnam are associated with shifts in labor from informal households to wage labor markets (Edmonds and Pavcnik 2006). Hanson and Harrison (1999) find that in Mexico, the trade reforms of 1985 disproportionately affected low-skilled industries, potentially explaining the increase in wage inequality; because different industries employ different proportions of skilled and unskilled workers, increasing the relative price of skilled-intensive goods may translate into changes in the relative incomes of skilled relative to unskilled workers. Geishecker and Görg (2005) show that low-skilled workers in low-skill-intensive industries (not high-skill-intensive industries) experience reductions in real wages resulting from international fragmentation within their industry, while the opposite is true for high-skill workers in high-skill-intensive industries. The paper's results are in line with the studies by Munch and Skaksen (2009) and Geishecker and Görg (2008) that show foreign outsourcing harms low-skilled workers but improves the wages of high-skilled workers. In the United States, the connection between globalization, technology and wages has also become more important in explaining increased labor inequality in recent years (Haskel et al. 2012).

An important issue in the context of this paper, however, is that of micro- versus macro-level studies. While micro-level studies are very popular within and outside the academic economics profession, macro-level studies are not as well respected these days. However, we argue that broad questions of the sort we are dealing with in this paper can best be answered through cross-country, macro-level panel regressions. Micro-level studies, which find that wages and/or employment go up in some sectors and fall in others and within a sector they go up in some firms and fall in others in response to structural reforms, can provide insights into the conditions under which reforms impact heterogeneous groups differently. But these studies by design are silent with respect to aggregate or economy-wide effects driven by general equilibrium dynamics, and thus they are of lesser value to policy makers who have to decide on broad policy reforms such as trade reforms, industrial deregulation, tax reforms, etc. In fact, it is plausible that the estimated micro effects give us a partial and incomplete picture when the within-firm or within-industry effects are estimated without taking into account resulting labor flows between industries or firms within an industry or into or out of unemployment, informality, or even the labor force. More specifically, ignoring the destinations (or sources) of labor flows out of (or into)

affected firms or industries, as is generally true in micro-level studies, can produce biased estimates of the impact of reforms on workers.

Although little empirical work exists on the aggregate labor-market effects of structural reforms, our paper is related to other important research that estimates cross-country unemployment regressions. The literature is mainly concerned with the impact of macroeconomic shocks on labor-market institutions, including, for example, Blanchard and Wolfers (2000) and Scarpetta (1996). (See Bassanini and Duval (2006, 2009) for a survey of this literature.) Other more recent examples include Nickell, Nunziata and Ochel (2005) and Boulhol (2008). Boulhol (2008) focuses on trade openness interacted with labor-market institutions but does not address the endogeneity of this relationship. However, this literature has focused primarily on developed OECD countries. In addition, many of these studies are more about long-run relationships than about the short-run effects. On the theoretical side, as discussed in the proceeding section, there is a long list of papers showing the impact of trade policy on the structural unemployment rate.

3. Theory

3.1 The impact of structural reforms on labor-market outcomes

In this section, we discuss the theoretical predictions of the impact of structural reforms and liberalization on the unemployment rate, the level of employment, the wage rate and the labor force participation rate. Rather than presenting just one model, this section describes the predictions of different models in the literature, including Felbermayr, Prat and Schmerer (2011), Helpman and Isthokhi (2010), Davidson, Martin and Matusz (1999), Moore and Ranjan (2005) and Pissarides (2000). Here we want to reiterate that our focus is not just limited to trade reforms; we look at the impact of structural reforms in general. However, it is important to note that trade reforms are a very important part of structural reforms and so a large part of the discussion is about the impact of trade reforms on labor-market outcomes.

Consider a two-sector Ricardian model with labor as the only factor of production. But, unlike a standard Ricardian model, suppose there are search frictions in the labor market. If we assume that the nature and extent of search frictions are the same in both sectors, trade liberalization leads to an increase in the value of the marginal product of labor (along with specialization in one of the two goods). Each worker will get a higher wage. For a given labor force participation rate, more vacancies are created relative to potential workers searching for jobs. This will make the labor market tighter and lower the unemployment rate in the long run, after short-run adjustments have taken place. How long it takes for these short-run adjustments to be completed is an empirical issue. Note that such a change will also lead to an increase in the labor force participation rate as the higher wage creates an incentive to look for a job. Thus, overall employment will increase.

From here, let us move to a two-factor, two-sector Heckscher-Ohlin framework. Let us call the two factors labor and capital and assume that capital services are sold in a perfectly competitive market while there are search frictions in the labor market. A labor-abundant country has a comparative advantage in the labor-intensive good. Therefore, upon opening to trade, the structure of such an economy gets more specialized towards the labor-intensive good. This increases labor demand and the value of the marginal product of labor goes up, increasing the incentive for posting more vacancies.

This, in turn, results in an increase in labor-market tightness and therefore leads to an increase in the wage, an increase in the labor force participation rate and a reduction in the unemployment rate. It is easy to see that the results in the case of a capital-abundant economy will be just the opposite.

Another possible productivity-driven, unemployment-reducing effect of trade is fully worked out in Felbermayr, Prat and Schmerer (2011). The effect of trade in that paper works through an interfirm labor reallocation effect in a one-sector model of search frictions with monopolistic competition, increasing returns to scale and heterogeneous firm productivity. After trade liberalization, the least productive firms are not able to survive the greater competition from more productive domestic and foreign firms. Also, the more productive firms grow at the expense of the other firms. Average firm productivity rises and, as a result, so does employment and wages. Unemployment falls upon trade liberalization. Helpman and Isthokhi (2010) construct a similar model but they have a second sector, which is one with homogeneous productivity, constant returns to scale and perfect competition. The impact of trade reforms on unemployment is ambiguous in that model. Examples of other models in which trade reforms have ambiguous effects on unemployment are Davidson, Martin and Matusz (1999) and Moore and Ranjan (2005). In Davidson, Martin and Matusz (1999), the results depend on international and intersectoral differences in search frictions. On the other hand, in Moore and Ranjan (2005) the results are driven by the assumption that there are two types of labor (skilled and unskilled).

As mentioned earlier, other components of structural reforms that include macroeconomic stabilization, privatization, deregulation, etc. create a better economic environment and better economic incentives for businesses and workers. These improvements raise productivity and lead to better labor-market outcomes.

We next discuss the possible short-run effects of structural reforms on unemployment. In the short-run, labor would hardly be able to move from one sector to another, and so we assume no intersectoral mobility of labor in the short run. If there is another factor such as capital, then even that factor would not be able to move so quickly. In addition, we could think of job destruction as endogenous, along the lines of chapter 2 in Pissarides (2000). A firm-job pair starts at full productivity at the point of creation, but this is followed by a series of productivity shocks over time that are received by each firm at a Poisson arrival rate. The threshold productivity level for firm survival is the one at which the firm just breaks even, below which the firm-worker pair is destroyed. Since revenues of a firm-wage pair are increasing in output price and productivity, an increase in price reduces the threshold productivity for survival. With structural reforms (such as trade reforms) or even their reversal, the domestic relative price of one sector goes down and that of the other goes up. In the sector where the relative price goes up, the threshold productivity level falls and, as a result, the job creation rate rises and the job destruction rate falls. In the other sector, where the relative price goes down, the threshold productivity rises and, as a result, the job destruction rate rises and the job creation rate falls. Thus starting from a steady state of zero net job creation or destruction in each sector, we get net job creation in the sector with the price rise and net job destruction in the other sector. Since job creation takes time and job destruction can be instantaneous, the impact effect of a structural reform can be an increase in economy-wide unemployment.

3.2 Endogenous structural reforms and their instrumental variables

This paper examines the impact of structural reforms on labor-market outcomes. However, whether structural reforms take place or not and their timing depend on economic and political factors and interactions between such factors. Macroeconomic conditions themselves might determine structural reforms. For example, poor macroeconomic performance and conditions (that would include high unemployment rates or alternatively high inflation rates) might lead governments to seek technical help from multilateral institutions, including the International Monetary Fund (IMF). Such technical assistance often comes with the conditionality of deep economic reforms, especially structural reforms.

A set of instruments to tackle this endogeneity is identified using economic intuition based on existing models. A valid instrument is one that is correlated with the endogenous explanatory variable but orthogonal to the error term. While no instrument can be perfect, and finding valid instruments is arguably made more difficult by the country-level analysis, we make use of the best available, namely the external debt-to-GDP ratio, lagged reforms, terms of trade, and democratization. For example, micro-level studies that regress individual wages on industry-level variables to measure the impact of outsourcing argue that the empirical specification is less prone to endogeneity bias, since the industry's outsourcing intensity may be largely considered exogenous to the individual worker (Geishecker and Görg 2005, Munch and Skaksen 2009). We now present the economic arguments behind the use of these instrumental variables, and why we expect that they satisfy each of their two requirements. Econometric tests of the validity of these instruments are discussed below in the results section. While there is the possibility that the instruments are still invalid even if we fail to reject the null hypothesis of these tests, these tests inform that any endogeneity problem is not severe enough to distort our estimates.

External debt-to-GDP ratio: The external debt-to-GDP ratio can be a good instrument for reforms in our regressions because, while it triggers reforms, it does not, by itself, affect unemployment. It is government spending (and the budget deficit) that is related to unemployment and other labor-market outcomes. In certain cases, high levels of government spending lead to high aggregate economic activity and economic growth and a low unemployment rate. However, this high level of spending is possible through big budget deficits. Over time, national debt keeps building and at a certain point in time the size of the debt can become risky, making an economy susceptible to a debt crisis when capital markets stop providing roll-over financing. Such a debt crisis can lead the government to seek financial assistance from the IMF, which comes attached with the strong conditionality of reforms. Reforms that were not politically viable earlier become politically viable in times of crises. Below we present different models showing that the country's debt can be an important variable determining the likelihood and timing of reforms. Note that while the budget deficit and government spending are flow variables, a country's overall and external debt are stock variables. In other words, the deficit can be high but the debt need not be high unless the high deficit is run for many years. The current deficit is a small part of the debt and due to the volatile nature of the deficit its correlation with the debt is likely to be small. Measuring this relationship using a sample of over 90 countries over all available years suggests that the correlation between a country's external debt-to-GDP ratio and deficit-to-GDP ratio is low, measuring only 0.35. If we recognize that it is the flow variable (deficit) that determines unemployment and not the stock variable (debt), then we can understand why a variable like the debt-to-GDP ratio can be a good instrument for reforms as it would be orthogonal to the error in our labor-market regression models.

Two models of Alesina and Drazen (1991) and Drazen and Grilli (1993) show that the cost of not undertaking structural reforms, especially stabilization, rises with public debt. This debt builds up over time until it reaches a threshold level. In such cases, the government may need to seek financial assistance from an international institution, which will only come with a strong conditionality of reform. In these models, reforms are like a public good. However, there are also costs that need to be incurred while implementing reforms. Just like any costly public good that generates benefits for everyone, reforms face a free-rider problem. Everyone wants everybody else but themselves to incur the costs. In Alesina and Drazen (1991) and Drazen and Grilli (1993), there are two groups, both of which would benefit from a fiscal reform. In the absence of a fiscal reform, there are distortionary taxes that are costly and cannot raise enough revenues to close the fiscal deficit. A fiscal reform will raise direct taxes that are less distortionary and will improve and stabilize the macroeconomic climate. There is a war of attrition between the two groups in that each wants to out-wait the other one in the hopes that they will give in and incur most of the costs (will agree to bear a bigger share of the tax burden). The model is based on incomplete information available to a group about the foregone benefits of the other group. Thus, there is a delay until one group gives in. The costs rise over time due to a buildup of the fiscal debt. It is important to note here that any financial assistance from an international institution should be able to speed up stabilization in that it could increase the current benefit from reform for both groups.

Related to the issue of the IMF's role in reforms is the issue of "status-quo bias" that prevents reforms, which is elegantly demonstrated by Fernandez and Rodrik (1991). Fernandez and Rodrik (1991) show that reforms are unlikely to happen by themselves and that a dictator or an international institution is needed to force these reforms. This can happen even when the movers (all those who move from the import-competing sector to the export sector after reforms) and those who were in the export sector prior to reforms (and continue there after reforms) benefit and form the majority of voters in the presence of uncertainty about who ends up moving. In the words of Fernandez and Rodrik (1991), there is "individual-specific uncertainty" regarding who ends up moving and who ends up staying in the import-competing sector after the reforms.

Let us say all those who are in the export sector prior to reforms gain from reforms. Let us assume 40 percent of the population is in that sector to begin with. After reforms, this sector will utilize 70 percent of the population. Each mover will gain x and each person stuck in the import-competing sector will lose y . Let us assume $y > x$, in which case prior to reform, any producer in the import-competing sector initially views her expected change in welfare as $0.5(x - y) < 0$. Thus, we get a vote against reforms ex ante even if ex post a majority of the people benefit. As mentioned above, a government goes to an international institution for assistance only in times of crisis. When there is a debt crisis, which can be mitigated through financial assistance from the IMF, the government might care more about obtaining this assistance than about popular support. Alternatively, the popular support could be affected by the possibility of this assistance in a crisis situation. Suppose this assistance conditional on structural reforms adds an amount Δ to the above expected change in welfare for every individual when a crisis is looming and can be prevented by IMF assistance. If Δ is large enough (which could be the case if the possible crisis is severe and the potential IMF assistance is substantial), then we can have $0.5(x - y) < 0 < 0.5(x - y) + \Delta$. Under these conditions, reforms can take place. A subsequent vote on whether reforms should continue or be reversed, even in the absence of further IMF pressure or

financial assistance, will support the continuation of reforms since reforms have now already revealed the identities of the beneficiaries of these reform, i.e., the “individual-specific uncertainty” has been resolved. **Lagged value of the reform indicator:** The aforementioned models also imply that if an economy is reformed, the likelihood of reversal is low. Therefore, whether the economy was in a state of reform or not in the previous period will determine the current state. The lagged value of the reform variable, which indicates whether an economy is reformed or not, is a possible instrument as these will clearly be correlated. It could be a questionable one however if the effect of reforms on labor-market outcomes takes hold with a lag. In that case, this variable would directly impact the labor market in addition to impacting the current status of reforms, making it correlated with the error. On the other hand, if labor-market outcomes change in anticipation of reforms, then this could be a good instrument as then the lagged reform indicator will affect the labor market only through its effect on the current reform indicator (which, in turn, affects future reform status). We therefore rely on econometric tests to tell us whether this is a reasonable instrument.

Terms of trade: Another factor that could affect the likelihood of reforms is a country’s terms of trade. We summarize here two political-economy models that show how changes in the terms of trade can affect the likelihood of reforms. Krishna and Mitra (2008) uses a median-voter model to study this relationship. Consider an economy with two sectors, an import-competing sector and an exportable sector. Every individual in this economy is assumed to have his or her personal comparative advantage (relative productivity) in the production of these goods. In this model when the relative world price of the importable goes down (the relative world price of the exportable improves), amounting to a terms-of-trade improvement, the proportion of voters supporting reforms goes up. With a higher world relative price of the exportable, the relative payoff to working in the export sector goes up for everyone.

In another paper, Krishna and Mitra (2005) extend a Grossman-Helpman “Protection for Sale” lobbying model to include endogenous lobby formation by specific factor owners in the export sector in the presence of a pre-existing import-competing lobby. The authors of the paper show that an increase in the world price of the exportable increases the incentive for lobby formation by the producers of the export good, which neutralizes the import-competing lobby to bring about reform.⁴

The question that might exist about such an instrument is that terms of trade by themselves might affect the domestic labor-market outcomes directly and independently of reforms taking place, not just through their effect on the possibility and the timing of reforms, making it correlated with the error term. What might break this direct relationship between unemployment and the terms of trade is the fact that, prior to reforms, most developing countries were so closed that the external terms of trade that they faced would not have affected their labor-market outcomes in their state of virtual autarky. In addition, their trade policies (and other structural policies) were aimed at completely insulating these countries from fluctuations in their external terms of trade. Given that in our empirical analysis we will have a substantial number of observations pre- and post-reforms, this direct correlation between unemployment and terms of trade after controlling for reforms will break down.

⁴ Another paper that arrives at the same result is an older paper by Coates and Ludema (2001). While they take a more black-box approach to lobbying than Grossman and Helpman (1994), they introduce some other “real-world complications” that are quite relevant in this context. In particular, they introduce negotiations between trading partners and ratification by each country’s legislature.

Democratization: We next move to the role of democratization in explaining liberalization. Milner and Kubota (2005) show that democratization makes it harder for protectionist governments to maintain political support in developing (labor-abundant) countries. Suppose individuals possess capital and labor in a two-sector, two-factor economy. In a labor-abundant country freer trade through structural reform increases the reward to labor and reduces the return on capital. In a labor-abundant country where only the elite (the relative capital-rich individuals) can vote, we will see protectionist policies in place since those policies will benefit the scarce factor, namely capital, at the expense of the abundant factor, namely labor. Democratization gives the relatively capital-poor (people with fewer assets) the right to vote, which moves the country to more liberalized policies as it benefits the abundant factor, labor. Milner and Kubota (2005) find strong support for their hypothesis using data from developing countries for the period 1970-99. Also, Mitra, Thomakos and Ulubasoglu (2002) find that the weight the government puts on aggregate welfare relative to political contributions in a Grossman-Helpman “Protection for Sale” model has been higher with democratic governments than with dictatorships in Turkey. In the presence of lobbies that predominantly represent import-competing sectors, this result supports the positive relationship between democratization and liberalization: democratization leads to reforms. Since the only way democratization can affect the labor market would be through changes in economic policies, virtually all changes in such policies would be part of structural reforms, making democratization uncorrelated with the error term. Thus a change in a country’s democracy score over the last several years could be a suitable instrument for structural reforms.

4. Econometric Methodology

We first run the following regression for each of our labor-market outcomes:

$$Y_{it} = \beta_0 + \beta_1 X_{it} + \sum_{j=10}^1 \gamma_j PreDummy(j)_{it} + \sum_{j=1}^{10} \gamma_j PostDummy(j)_{it} + v_i + \varepsilon_{it} \quad (1)$$

where i denotes the country, t denotes the year, Y denotes the labor-market outcome variable under consideration (unemployment rate, employment, female employment, wage index, labor force participation rate or female labor force participation rate), X includes control variables if specified (these regressions are run with and without controls), $PreDummy(j)$ is a dummy variable equal to 1 j years prior to reform (0 otherwise), $PostDummy(j)$ is a dummy variable equal to 1 j years after reform (0 otherwise), v is the country fixed effect and ε is the idiosyncratic error. The coefficient of each $PreDummy(j)$ and $PostDummy(j)$ dummy variable represents the average level of the outcome variable in its corresponding time period relative to the average level the year of reform. The controls include real GDP, the labor force participation rate and the working age population for all labor-market outcome variables except when the overall and female labor force participation rates are themselves the dependent variables. When the dependent variable is the overall or female labor force participation rate, the controls used are real GDP and the working age population. After running the above regressions, we present graphs that plot the estimated dummy variable regression coefficients against time where time is equal to -1 the year prior to reform, 0 the year of reform, 1 the year after reform, etc.

We next run the following regression for each labor-market outcome variable:

$$Y_{it} = \alpha_0 + \alpha_1 POST_{it} + \alpha_2 time_{it} + \alpha_3 time_{it}^2 + \alpha_4 X_{it} + u_i + \omega_{it} \quad (2)$$

where, just as in the case of (1), i denotes the country, t denotes the year, Y denotes the labor-market outcome variable under consideration (unemployment rate, employment, female employment, wage index, labor force participation rate or female labor force participation rate), X includes control variables if specified (these regressions are run with and without controls), u is the country fixed effect and ω is the idiosyncratic error. POST is a dummy variable that takes the value 1 every year after reform and 0 otherwise. Once again, the controls include real GDP, the labor force participation rate and the working age population for all labor-market outcome variables except the overall and female labor force participation rates. When the dependent variable is the overall or female labor force participation rate, the controls used are real GDP and the working age population. Specification (2) above is run as a fixed-effects ordinary least squares regression with and without controls.

As argued in our theory section above, the implementation and timing of reforms can be endogenous to economic variables, especially to macroeconomic policies and conditions. Thus, our variable POST can be endogenous to the unemployment rate and other labor-market outcome variables of interest. To tackle this endogeneity problem, we run the same fixed-effects specification for each labor-market outcome variable, both with and without controls, as an instrumental variables (IV) regression using alternative sets of instruments. The variable that is instrumented is POST. POST is instrumented with the external debt-to-GDP ratio, terms of trade and five-year change in a country's democracy score. Alternatively, POST is instrumented with the change in the world interest rate (proxied by the change in the U.S. Treasury Bill rate, which obviously is truly exogenous to our variables of interest) interacted with the external debt-to-GDP ratio, terms of trade and five-year change in a country's democracy score. Additionally we also include one-year lagged POST in our set of instruments. We have explained in the theory section, using economic intuition based on existing models, why these are potentially good instruments for POST.

We also conduct econometric tests for the validity of these instruments, which inform that even if there is an endogeneity problem it is not severe enough to distort our estimates. The first requirement is that these instruments should be fairly correlated with the endogenous variable we are trying to instrument. After partialling out the effects of the exogenous variables that are included in the second stage, a substantial proportion of the variation in the instrumented endogenous variable should be explained by its instruments. To ascertain the relevance of the IVs, we calculate Shea's first-stage partial R^2 and the Kleibergen-Paap F statistic. The second requirement is that each of the instruments is uncorrelated with the error term. To be able to verify that, we need our main equation of interest to be overidentified, that is, the number of instruments to exceed the number of right-hand side variables instrumented. If the equation is overidentified, then the Hansen J test statistic for overidentifying restrictions can be calculated (when allowing for robust and clustered standard errors). The null hypothesis of this test is that all the extra instruments are jointly exogenous (each is uncorrelated with the error term). If the p-value corresponding to the value of this test statistic exceeds five percent, the null hypothesis of joint exogeneity of these instruments cannot be rejected at the five percent significance level (and so on).

5. Data and Summary Statistics

The country sample and liberalization dates are adopted from Wacziarg and Welch (2008), which

proxies for the date when countries reached a threshold of broad reforms including macroeconomic stabilization, privatization, trade opening, as well as the end of interventionist states such as communism. Wacziarg and Welch (2008) revise and update Sachs and Warner's (1995) dates of liberalization through 2001. In principle, the liberalization date is the date after which all of the Sachs-Warner openness criteria are continuously met (however data limitations often imposed reliance on country case studies of trade policy). A country is classified as open if it displays none of the following characteristics: (1) average tariff rates of 40 percent or more; (2) nontariff barriers covering 40 percent or more of trade; (3) a black market exchange rate at least 20 percent lower than the official exchange rate; (4) a state monopoly on major exports; and (5) a socialist economic system. Countries that according to Wacziarg and Welch (2008) had liberalized their policies prior to 1960 or that had not liberalized by 2001 are excluded from our analysis.

Our structural reform index, POST, is a dummy variable that takes the value 1 every year after the date of liberalization and 0 otherwise. We call it a structural reform index because it proxies for the date when countries reached a threshold of broad reforms including macroeconomic stabilization, privatization, trade opening, as well as the end of interventionist states such as communism. For countries with multiple attempts at liberalization, the year of the final liberalization episode was used. Figure 1 presents a histogram of the number of countries liberalizing in each year. See Appendix Table 1 for a complete list of the countries included in the dataset and the year of liberalization from Wacziarg and Welch (2008).

Six labor-market outcomes are considered separately as dependent variables of interest. The **unemployment rate (UnempRate)** is defined as the percentage of the labor force that is without work but available for and seeking employment. The series was constructed using data from the IMF International Financial Statistics (IFS) (International Monetary Fund 2012), the World Bank World Development Indicators (WDI) (World Bank 2012) as well as the ILO Key Indicators of the Labor Market (KILM) (International Labor Organization 2012), the OECD Labor Force Statistics (LFS) (Organization for Economic Co-operation and Development 2012), and other regional agencies and country-specific sources. **Employment (Emp)** measured in millions, accessed from The Conference Board Total Economy Database (TED) (Conference Board 2012), includes employees, the self-employed, unpaid family members that are economically engaged, apprentices, and the military. Employment series for countries not available from the TED were accessed from the World Bank WDI as total employment aged 15 and older. **Female employment (FemaleEmp)** measured in millions, accessed from the World Bank WDI, is total female employment aged 15 and older. The **wage index (WageIndex)**, accessed from the IMF IFS and deflated by the consumer price index accessed from the World Bank WDI, is an index of real wage earnings with 2005 as the base year equal to 100.

The **labor force participation rate (LFPRate)**, accessed from the World Bank WDI, is the labor force as a percentage of the working age population. The total labor force comprises people ages 15 and older who meet the International Labour Organization definition of the economically active population: all people who supply labor for the production of goods and services during a specified period. It includes both the employed and the unemployed. While national practices vary in the treatment of such groups as the armed forces and seasonal or part-time workers, in general the labor force includes the armed forces, the unemployed, and first-time job-seekers, but excludes homemakers

and other unpaid caregivers and workers in the informal sector. The latter—the exclusion of informal workers from the definition of the labor force—turns out to be important for the interpretation of the econometric evidence discussed in the results section below. Labor force participation rate series for countries not available from the World Bank WDI were accessed from the IMF IFS as the labor force as a percentage of the population aged 15 and older. The **female labor force participation rate (FemaleLFPRate)**, accessed from the World Bank WDI, is the female labor force as a percentage of the female working age population.

Additional control variables are included. **Real GDP (rGDP)** is measured in thousands of constant 2005 international dollars and is accessed from the Penn World Tables version 7.0 (Heston, Summers and Aten 2011) as PPP converted GDP per capital (chain series) constant 2005 prices multiplied by the population. The **working age population (WorkingAgePop)** measures the number of people who could potentially be economically active as the total population between the ages 15 to 64, accessed from the World Bank WDI.

Four instrumental variables were considered to instrument POST. The **external debt-to-GDP ratio (Debt)** is total external debt (debt owed to nonresidents repayable in foreign currency, goods, or services)⁵ from the World Bank WDI as a share of GDP. The **external debt-to-GDP ratio interacted with the change in world interest rates (proxied by the U.S. Treasury Bill rate) (DebtTBill)**, is the same as above only interacted with the one-year change in the market yield on U.S. Treasury securities at 10-year constant maturity, available from the United States Federal Reserve. **Terms of trade (ToFT)** is the net barter terms of trade index from the World Bank WDI, calculated as the percentage ratio of the export unit value indexes to the import unit value indexes, measured relative to the base year 2000. The **five-year change in democracy score (Dem5)** is calculated as the five-year change in Polity IV's governing authority index (Gurr, Marshall and Jagers 2012). Index scores range from -10 (most autocratic) to 10 (most democratic). See Appendix Table 2 for a complete list of the variables, their definitions and their sources.

Table 1 reports summary statistics for each of the variables of interest.

6. Results

6.1 Time plots of labor-market outcomes

As previously mentioned, in equation (1) the coefficient of each *PreDummy(j)* and *PostDummy(j)* represents the cross-country average of the level of the labor-market outcome variable in its corresponding time period relative to the level the year of reform where time is equal to -1 the year prior to reform, 0 the year of reform, 1 the year after reform, etc. In Figure 2, we specifically plot the cross-country average relative unemployment rate in each year. There are no controls in the estimation of (1) other than country fixed effects in Panel A, while additional controls in Panel B include real GDP, the labor force participation rate and the working age population. Both panels clearly show that, while on average unemployment fluctuates both before and after reforms, the post-reform average level of the

⁵ Total external debt is debt owed to nonresidents repayable in foreign currency, goods, or services. Total external debt is the sum of public, publicly guaranteed and private nonguaranteed long-term debt, use of IMF credit and short-term debt. Short-term debt includes all debt having an original maturity of one year or less and interest in arrears on long-term debt. Data are in current U.S. dollars.

unemployment rate looks distinctly higher than the pre-reform average level of the unemployment rate. While the unemployment rate attains its lowest value a couple of years prior to reforms, it reaches its maximum level a couple of years after reforms, with the difference in the unemployment rates between these two points being 2.5 percentage points. This is just a documentation of empirical regularity rather than a statement of causation. If reforms accompanied by austerity measures are carried out to get out of a debt crisis arising in turn from many years of extravagant government spending (leading to high aggregate output and low unemployment), then clearly it is not reforms that are causing the higher unemployment. Also, a stock market crash can happen in anticipation of a bad real economic performance, i.e., the stock market is a good predictor of future economic performance. A poor financial situation and a poor performance of stocks often go together, while a financial crisis can trigger reforms. In this case again reforms cannot be responsible for higher unemployment rates. Despite these possibilities, we see from both Panel A (without controls other than fixed effects) and Panel B (with additional controls) of Figure 3 that employment levels post-reform are way higher than those pre-reform. Also, after reforms, employment has been rising consistently and relatively steeply for a long period of time. This is also true with the wage index presented in Figure 4. Pre-reform levels are falling while the post-reform wage index has a steep and consistent upward trend.

We next look at the overall labor force participation rate in Figure 5. In Panel A we find a declining trend prior to reforms and the labor force participation rate exhibits an upward trend after reforms. In Panel B, while there are more fluctuations throughout, the trend on average is upward sloping after reforms, but was downward sloping prior to reforms. In Figure 6, the female labor force participation rate also shows an upward trend with consistently positive levels relative to the reform year after reforms. Before reforms, negative relative female labor force participation rates are dominant in both Panel A and Panel B.

To dig a little deeper into the female labor-market outcomes, we plot the average female employment level each year relative to the reform year after controlling for country fixed effects in Panel A of Figure 7 and then in Panel B using the additional controls we have been using so far. With no controls other than fixed effects, female employment levels on average are higher before reforms in Panel A as compared to pre-reform levels. In Panel B, while on average female employment levels are higher pre-reform than post-reform, the negative trend pre-reform is replaced by a positive trend post-reform.

On average, after reforms, the unemployment rates are higher but all other labor-market outcomes on average look better both in terms of trends and levels. This does not mean anything in terms of causation. However, these are empirical regularities that are hard to ignore. Next we perform some OLS and IV fixed effects regressions (FE) to see if we can identify any causal effects of reforms on labor-market outcomes and, if so, whether this is qualitatively similar to the patterns observed in the plots presented in Figures 2-7.

6.2 Regression results

Tables 2 through 7 present the results of the estimation of specification (2), with a separate labor-market outcome as the dependent variable in each. In all these regressions we control for time and time squared. While five of the regressions (those presented in the odd-numbered columns) do not have any other

controls besides the country fixed effects, the remaining five regressions have other controls, namely real GDP, the labor force participation rate and the working age population. The OLS-FE regression results are presented in columns (1) and (2) and the IV-FE regressions in columns (3) through (10).

To better understand what is behind these results, additional regressions were performed that interact POST with two dummy variables, one for labor abundant countries and another for dictatorships.⁶ Labor abundant countries are identified as those with a ratio above one of its national share of world population to its national share of world GDP. Countries with a dictatorship are identified as those with a score of four on PolityIV's regulation of participation (PARREG) variable, defined as countries where regulation of participation is restricted. The PARREG variable can take a value between one and five. A value of one indicates unregulated political participation, two indicates relatively stable but enduring political groups, three indicates oscillation among multiple identity groups that restrict competing groups' political activities, four indicates some organized political participation permitted but significant groups/issues are excluded from the political process, and five indicates regulated political participation but with little use of coercion. Totalitarian party systems, authoritarian military dictatorships, personal dictatorships and despotic monarchies are typically coded with a score of four. These dummy variables are allowed to vary over time. Tables 8 through 10 present the results for these specifications with employment (total and female) and the wage index as dependent variables.

The OLS-FE regression results presented in columns (1) and (2) of the tables show similar outcomes to what we have seen in our figures above. In Table 2, the dependent variable is the unemployment rate. The coefficient of POST is positive and significant and shows that the unemployment rate on average is about a percentage point higher after reforms than before. This is true even though we are controlling for a time trend and the square of time, both of which are statistically insignificant. This shows there is clearly no time pattern of the unemployment rate except that the average level post-reform is clearly higher than the average level pre-reform. An expansion of the labor force participation rate is accompanied by a decline in the unemployment rate. For the OLS regressions, when the observations are restricted to only those for which there is information on all the basic and control variables and all the IVs, the OLS sample gets much more restricted. POST is now highly insignificant but is also negative. These results are not presented in our tables but are available upon request.

When employment (Table 3) and female employment (Table 7) are the dependent variables, the OLS-FE regression results show that the coefficient of POST is positive and significant when the additional control variables are included in the regression. For employment, the coefficient of POST is also positive and significant without additional controls. Employment (total and female) is higher on average in economies following structural reforms. Restricting the sample for the OLS regressions to the sample used in the IV regressions for employment makes the results even stronger (not shown in our tables). However, the OLS-FE results for the labor force participation rates (total and female) and wages are weak.

Simultaneously including the interaction terms with POST for employment (Table 8), female

⁶ Additional regressions where the sample was split between labor abundant vs. labor scarce countries and democracies vs. dictatorships did not yield any significant results.

employment (Table 10) and the wage index (Table 9) yield different results for the OLS-FE specifications. When the dependent variable is employment or female employment, we find that the coefficient on POST becomes significantly negative but the coefficient on POST interacted with the dummy for labor abundant countries is now significant and positive. For female employment and the wage index, the coefficient on POST interacted with the dictatorship indicator is negative and significant. Thus reforms are associated with higher total employment and constant female employment levels in labor abundant countries, but lower employment levels in labor scarce countries. Female employment and wages also decrease post reform in dictatorships.

To tackle the endogeneity of POST discussed in sections 2 and 3, we run the same fixed-effects specifications for each labor-market outcome variable as IV regressions, using alternative sets of instruments. The variable that is instrumented is POST. The instruments for regressions presented in columns (3) and (4) are external debt-to-GDP ratio, terms of trade and five-year change in democracy score. The one-year lagged value of POST is added as an instrument in columns (5) and (6). Columns (7), (8), (9) and (10) are respectively the same as (3), (4), (5) and (6), except that now the external debt-to-GDP ratio as an instrument is replaced with the interaction between the change in the world interest rate (proxied by the U.S. Treasury Bill rate) and external debt-to-GDP.

A valid instrument is one that is correlated with the endogenous regressor yet orthogonal to the errors. To determine the quality of the instruments, three empirical tests are used that provide evidence of the instruments' validity. First, to assess the correlation of the instruments with the endogenous regressor, it is sufficient to examine the significance of the excluded instruments in the first-stage regression. A commonly used statistic is the R^2 of the first-stage regression, after partialling out the effect of the exogenous left-hand side variables in the second-stage regression, referred to as the partial R^2 (Shea 1997). Although there is no threshold level, the instruments should be sufficiently relevant to explain a significant share of the variance of the endogenous regressor. As a rule of thumb, an estimated equation that yields a first-stage partial R^2 lower than 10% indicates a "weak instruments" problem. Second, we also report the Kleibergen-Paap rk Wald F statistic to test for weak instruments and compare them to critical values tabulated by Stock and Yogo (2005). Instruments are considered weak if the statistic falls below the critical value, unable to reject the null hypothesis that the equation is weakly identified.

Third, whether the instruments are orthogonal to the errors can be tested in an overidentified model where the number of instruments is greater than the number of endogenous regressors using the Hansen J statistic p-value (when allowing for robust and clustered standard errors). The Hansen J statistic is from a test of the hypothesis that the instruments are uncorrelated with the error term or that the overidentification restrictions are valid. A rejection would call this hypothesis into question and cast doubt on the validity of the instruments. The first-stage partial R^2 , the Kleibergen-Paap F statistic and the Hansen J p-value are reported in each of the IV results.

When the unemployment rate is the dependent variable, POST consistently has a negative coefficient sign and is significant in three of the eight regressions. Only in columns (5), (6), (9) and (10), where lagged POST is also included as an instrument, do both the first-stage partial R^2 , the Kleibergen-Paap F statistic and the Hansen J p-value support that the set of instruments possesses both ideal characteristics, namely a fair degree of correlation with the instrumented variable and the joint

orthogonality of the instruments with respect to the error term. (The first-stage regression results are available from the authors upon request.) The IV results from these columns imply that reforms on average lower the unemployment rate by about 1.5 percentage points when controlling for the labor force participation rate of the formal sector.

We next move to the employment level as a dependent variable (Table 3). The right-hand side variables and the exact set of instruments used in each column are the same as in the corresponding column numbers of Table 2. In most columns the coefficient of POST is positive, and is quite significant in four of the eight IV-FE columns. This is despite the fact that, in many of these columns, time and/or time squared have positive and highly statistically significant coefficients. This makes the result especially strong as these controls cover demographical changes and business-cycle effects on labor markets. There is also strong evidence for the positive effect of the size of the working age population. In columns (5), (6), (9) and (10) the first-stage partial R^2 , the Kleibergen-Paap F statistic and the Hansen J p-value do not reject that the set of instruments exhibit a fair degree of correlation with the instrumented variable and are exogenous to the error term. In columns (9) and (10), where the set of IVs consists of the external debt-to-GDP ratio interacted with the change in the U.S. Treasury Bill rate (to proxy for the world interest rate), terms of trade, five-year change in democracy score and lagged POST, the results of the Hansen J p-value are even stronger. Overall, the results are quite robust in those four columns, showing that structural reforms lead to an average employment increase of four million for the entire decade after reforms relative to the decade before reforms. In addition, because the labor force participation rate of the formal sector is being controlled for, the less conclusive results on unemployment after reforms suggests that some of the increase in employment may be coming from informal workers. The sample is heavily weighted by developing countries where informality is ubiquitous (since most developed countries were reformed throughout the period). We acknowledge, though, that this is only a possibility and difficult to evidence from the empirical results presented, given the analysis is carried out for a pooled sample at the country level.

The wage index is our next dependent variable (Table 4). We follow the same sequence of columns in terms of the methods of estimation, right-hand side variables and IVs used. Across all columns in Table 4 the coefficient of POST is positive, and is quite significant in seven of the eight IV-FE columns. Once again, this is despite the fact that in most of these columns, both time and time squared have positive and highly statistically significant coefficients. These are very strong results. There is also strong evidence in the IV regression for the negative effect of the size of the working age population, while the sign of real GDP is what one would expect and is significant. Except for columns (5) and (6), everywhere else the first-stage partial R^2 , the Kleibergen-Paap F statistic and the Hansen J p-value support that the set of instruments exhibits the ideal characteristics required of them. In the first stage of columns (3), the external debt-to-GDP ratio and terms of trade significantly increase the probability of reform. The lagged POST variable is also shown to significantly increase the probability of reform for its respective regressions.

Simultaneously including the interaction terms with POST for the wage index (Table 9) yields different results for the IV-FE specifications. In all columns the coefficient on POST remains positive, significant and stronger in magnitude than those observed without the interaction terms. But the coefficient on POST interacted with the labor abundant indicator is now negative throughout and

significant in seven of the specifications, suggesting that the increase in wages is also positive but muted in labor-abundant countries. The coefficient on POST interacted with dictatorships is not identified due to insufficient variance within the sample. The instruments pass all three tests of evidence of validity in three of the columns.

In Tables 5 and 6 we look at the overall and female labor force participation rate of formal workers. While there seems to be a positive time trend, POST is insignificant throughout. In half of the IV-FE columns, the three test results support that the requirements for IVs are satisfied. In Table 7, we dig deeper into female labor-market outcomes by looking at their employment levels. Only cases where POST is significant when instrumented are those where the instruments exhibit both ideal characteristics required of them. The coefficients however are not strong and, in our opinion, quite inconclusive. Overall, based on these regression results and Figure 7 discussed above, we cannot rule out the positive effect of structural reforms on female employment as well.

In sum, higher economy-wide real wages after structural reforms improve job prospects and provide incentives for workers to find jobs, consistent with higher levels of employment after reforms. But controlling for the labor force participation of formal workers and the weaker results on unemployment suggest this increase in employment may in part be coming from informal workers entering into the labor force. From a firm's perspective, higher real wages lead to lower employment (and, in turn, to higher unemployment in the economy) unless there are compensating productivity advances. Thus these results may also provide evidence for firm-level increases in productivity that allows firms to pay higher wages (while employing more workers). It may also signal the entry of new firms or the expansion of existing firms that now have access to a larger external market for export of their final products and for import of inputs that might be complementary to labor.

7. Conclusion

This paper argued that there is need for research on the impact of structural reforms on a comprehensive set of labor-market outcomes. Micro-level studies might not always be as valuable as cross-country macro-level studies to policy makers who have to decide on broad policy reforms, or even provide unbiased estimates of within-industry effects on labor since labor flows across sectors can result in attenuation biases. Therefore, this paper studied the impact of structural reforms on macro-level labor-market outcomes, namely the unemployment rate, the employment level, average wage index, labor force participation rates (overall and female) and female employment at the country level. Together these outcome variables capture the overall health of the labor market and the aggregate welfare of workers.

To our knowledge, there are only two major cross-country empirical studies that look at the impact of trade policy on unemployment rates. One is the paper by Dutt, Mitra and Ranjan (2009) and the other is by Felbermayr, Prat and Schmerer (2011a). Both papers show that countries that have less protectionist (more open) trade policies have lower unemployment rates. This is true both without any controls and after controlling for other policies that have a more direct impact on labor markets. Dutt, Mitra and Ranjan (2009) also find that the short-run impact of structural reforms is an increase in the unemployment rate followed by a reduction in the long run to a lower steady-state unemployment rate.

As mentioned in the introduction, there are several differences between our work and earlier literature. Unlike earlier work, our paper looks at a multitude of labor-market outcomes. Secondly, unlike previous studies, we include sample periods in our panel, such that for each country we cover up to 10 years before reforms and 10 years after reforms, which gives us greater confidence in our results, and allows us to track the dynamic response of labor markets to structural reforms. Thirdly, our focus is not limited to trade policies and we look at the impact of structural reforms in general (of which trade reforms are just one component) on labor-market outcomes. Finally, we are able to use credible (and seemingly valid) instrumental variables for our structural reform variable. These instruments are motivated by the theoretical literature on the political economy of structural reforms, and we provided econometric tests of their validity.

We have documented the trends on average (across countries) in our labor-market variables around the reform year for each country. This is done by controlling for country fixed effects and in another specification additionally controlling for real GDP, the labor force participation rate and the working age population. We have also run fixed-effects ordinary least squares as well as instrumental variables regressions of our labor-market outcomes on a reform dummy variable, a time trend, square of the time trend and the set of controls mentioned above.

Overall, we find that structural reforms lead to positive outcomes for labor, which is in sharp contrast to the widely held belief that reforms destroy jobs, increase inequality, make the rich richer and do not do much for the poor. Because we have controlled for the time trend and real GDP, our results show the impact of structural reforms on labor-market outcomes beyond what happens through its impact on growth. Redistributive effects in favor of workers, along the lines of the Stolper-Samuelson effect, may be at work.

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Figure 1
Histogram of Year of Reform

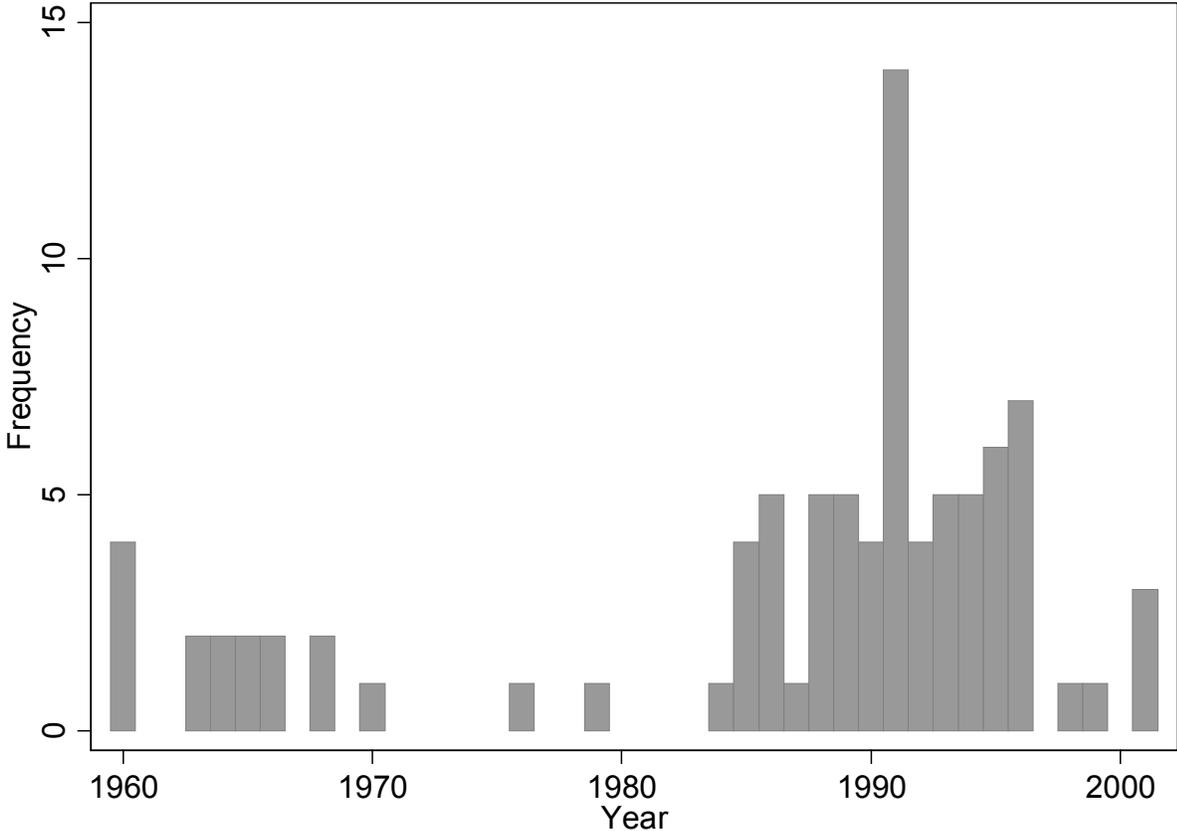


Figure 2

Dependent Variable: Unemployment Rate

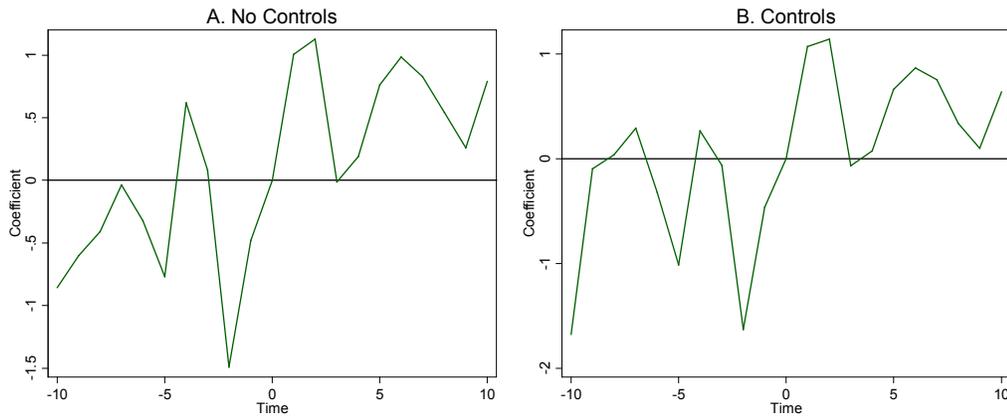


Figure 3

Dependent Variable: Employment

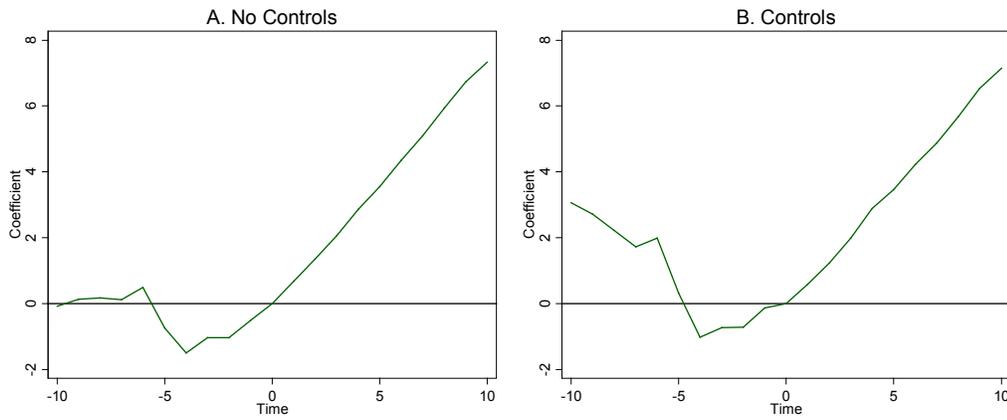


Figure 4

Dependent Variable: Wage Index

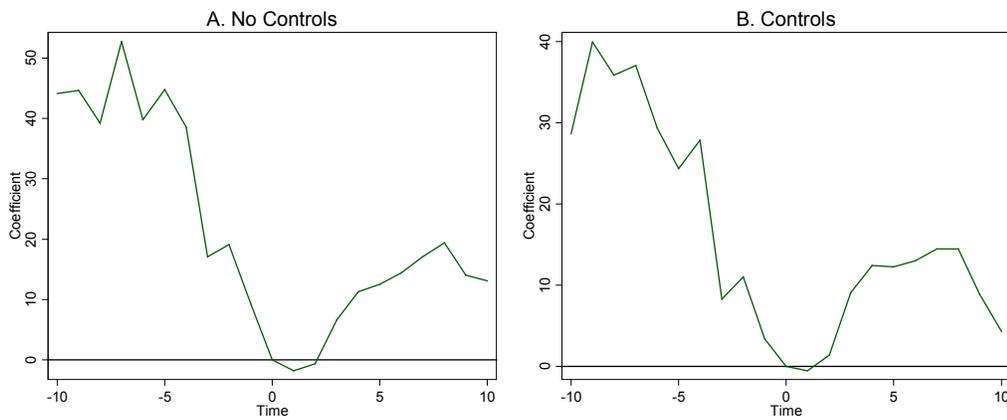


Figure 5

Dependent Variable: Labor Force Participation Rate

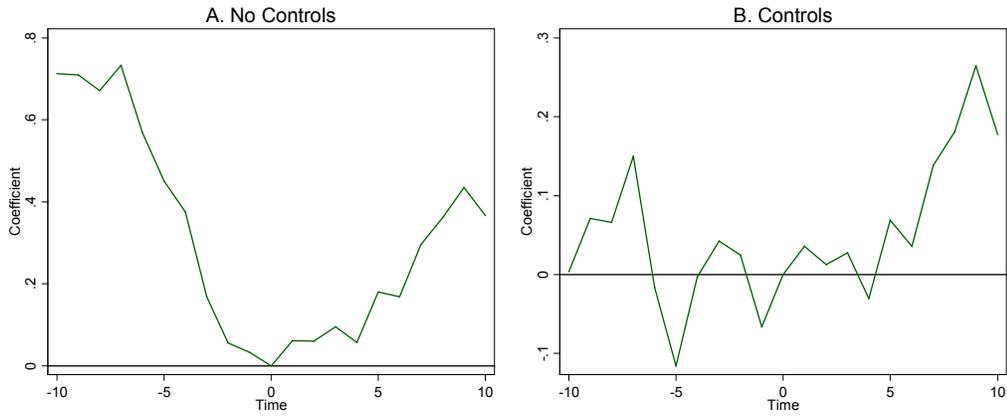


Figure 6

Dependent Variable: Female Labor Force Participation Rate

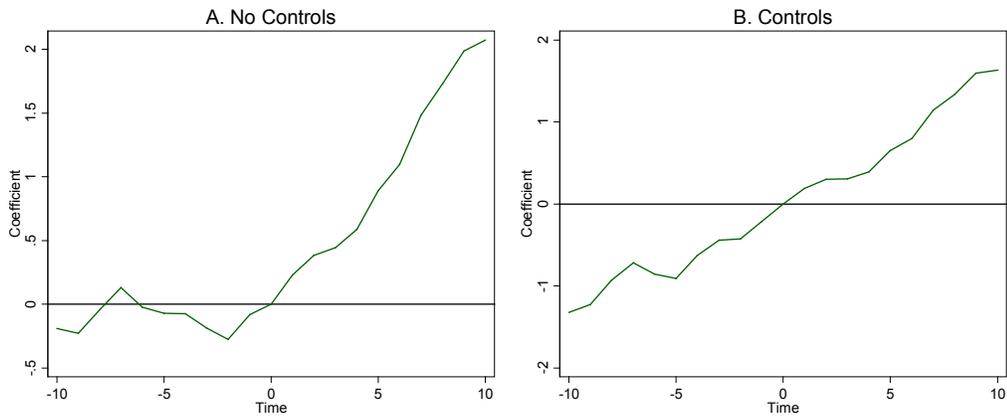
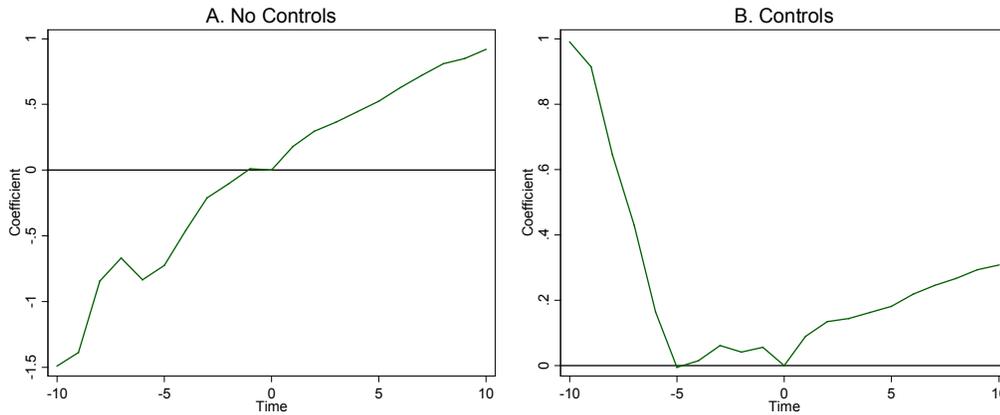


Figure 7

Dependent Variable: Female Employment



Note: Figures 2-7 plot, for each labor-market outcome, the estimated dummy variable regression coefficients from the regression specification:

$$Y_{it} = \beta_0 + \beta_1 X_{it} + \sum_{j=10}^1 \gamma_j PreDummy(j)_{it} + \sum_{j=1}^{10} \gamma_j PostDummy(j)_{it} + v_i + \varepsilon_{it}$$

where i denotes the country, t denotes the year, Y denotes the labor-market outcome variable under consideration, X includes control variables if specified, $PreDummy(j)$ is a dummy variable equal to 1 j years prior to reform (0 otherwise), $PostDummy(j)$ is a dummy variable equal to 1 j years after reform (0 otherwise), v is the country fixed effect and ε is the idiosyncratic error. Time is equal to -1 the year prior to reform, 0 the year of reform, 1 the year after reform, etc. The coefficient of each $PreDummy(j)$ and $PostDummy(j)$ variable represents the average level of the outcome variable in its corresponding time period relative to the average level the year of reform. The controls in Panel B include real GDP and the working age population in all regressions as well as the labor force participation rate with the exception of the regression in which this appears as the dependent variable.

Table 1
Summary Statistics

VARIABLES	(1) N	(2) max	(3) min	(4) mean	(5) sd
UnempRate	860	70.86	0.04	10.34	7.77
Emp	1352	79.54	0.07	8.50	12.93
LFPRate	1431	91.60	43.03	68.38	9.79
FemaleLFPRate	1420	91.80	12.90	55.24	16.73
WageIndex	295	536.60	0.00	111.20	101.10
FemaleEmp	800	30.96	0.04	2.99	4.63
rGDP-	1638	1900000000	298806	89820000	200200000
WorkingAgePop	1723	115400000	126252	11320000	18510000
DebtTBill	1245	4.92	-9.99	-0.23	1.15
Debt	1245	10.65	0.00	0.80	0.82
ToFT	1002	315.60	50.98	110.40	35.59
Dem5	1493	17.00	-15.00	1.47	4.74

Table 2

Dependent Variable: Unemployment Rate

VARIABLES	(1) OLS-FE	(2) OLS-FE	(3) IV-FE	(4) IV-FE	(5) IV-FE	(6) IV-FE	(7) IV-FE	(8) IV-FE	(9) IV-FE	(10) IV-FE
POST	1.00* (0.52)	1.12* (0.60)	-6.52** (3.03)	-5.11 (3.13)	-1.54* (0.90)	-1.56* (0.92)	-7.30 (6.78)	-5.17 (5.27)	-1.40 (0.91)	-1.42 (0.93)
Time	0.00 (0.10)	-0.02 (0.11)	0.49** (0.24)	0.34 (0.26)	0.12* (0.07)	0.07 (0.08)	0.55 (0.50)	0.34 (0.39)	0.11 (0.07)	0.06 (0.08)
Timesq	-0.00 (0.00)	-0.00 (0.01)	-0.01 (0.01)	-0.01 (0.01)	-0.01 (0.01)	-0.01 (0.01)	-0.01 (0.01)	-0.01 (0.01)	-0.01 (0.01)	-0.01 (0.01)
WorkingAgePop		-0.00 (0.00)		-0.00 (0.00)		-0.00 (0.00)		-0.00 (0.00)		-0.00 (0.00)
rGDP		0.00 (0.00)		0.00 (0.00)		0.00 (0.00)		0.00 (0.00)		0.00 (0.00)
LFPRate		-0.29* (0.16)		0.06 (0.17)		0.07 (0.17)		0.06 (0.17)		0.07 (0.17)
Observations	849	765	514	514	514	514	514	514	514	514
Number of Countries	67	59	48	48	48	48	48	48	48	48
R-squared	0.01	0.03								
Partial R-squared			0.03	0.03	0.42	0.42	0.02	0.02	0.42	0.42
Hansen J p-value			0.86	0.65	0.49	0.58	0.43	0.43	0.58	0.54
Kleibergen-Paap F statistic			5.45	5.42	500.47	466.94	3.07	3.18	381.01	368.91

Note: Robust and clustered standard errors at the country level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. POST is instrumented with the debt-to-GDP ratio, terms of trade, and five-year change in democracy score in columns (3) and (4). In columns (5) and (6) the instruments also include lagged POST. POST is instrumented with external debt-to-GDP ratio interacted with the change in world interest rates, terms of trade, and five-year change in democracy score in columns (7) and (8). In columns (9) and (10) the instruments also include lagged POST.

Table 3

Dependent Variable: Employment

VARIABLES	(1) OLS-FE	(2) OLS-FE	(3) IV-FE	(4) IV-FE	(5) IV-FE	(6) IV-FE	(7) IV-FE	(8) IV-FE	(9) IV-FE	(10) IV-FE
POST	0.99* (0.57)	1.31* (0.68)	13.62 (12.72)	8.44 (8.73)	4.27** (1.84)	4.18** (1.71)	2.75 (11.55)	-2.02 (8.07)	4.25** (1.83)	4.16** (1.71)
Time	0.31*** (0.07)	0.15** (0.07)	-0.31 (0.94)	-0.19 (0.69)	0.42*** (0.12)	0.14 (0.13)	0.54 (0.87)	0.63 (0.67)	0.42*** (0.13)	0.14 (0.13)
Timesq	0.04*** (0.01)	0.05*** (0.02)	0.07** (0.04)	0.08** (0.03)	0.06*** (0.02)	0.07*** (0.02)	0.06* (0.03)	0.06** (0.03)	0.06*** (0.02)	0.07*** (0.02)
WorkingAgePop		0.00** (0.00)		0.00* (0.00)		0.00* (0.00)		0.00* (0.00)		0.00* (0.00)
rGDP		-0.00 (0.00)		-0.00 (0.00)		-0.00 (0.00)		-0.00 (0.00)		-0.00 (0.00)
LFPRate		0.23 (0.22)		-0.16 (0.56)		-0.18 (0.54)		-0.21 (0.56)		-0.18 (0.54)
Observations	1,352	1,023	724	723	724	723	724	723	724	723
Number of Countries	84	72	60	60	60	60	60	60	60	60
R-squared	0.17	0.33								
Partial R-squared			0.02	0.02	0.41	0.41	0.02	0.02	0.41	0.41
Hansen J p-value			0.06	0.06	0.11	0.13	0.54	0.36	0.71	0.50
Kleibergen-Paap F statistic			2.89	2.88	518.30	513.40	3.41	3.52	509.16	504.79

Note: Robust and clustered standard errors at the country level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. POST is instrumented with debt-to-GDP ratio, terms of trade, and five-year change in democracy score in columns (3) and (4). In columns (5) and (6) the instruments also include lagged POST. POST is instrumented with external debt-to-GDP ratio interacted with the change in world interest rates, terms of trade, and five-year change in democracy score in columns (7) and (8). In columns (9) and (10) the instruments also include lagged POST.

Table 4

Dependent Variable: Wage Index

VARIABLE	(1) OLS-FE	(2) OLS-FE	(3) IV-FE	(4) IV-FE	(5) IV-FE	(6) IV-FE	(7) IV-FE	(8) IV-FE	(9) IV-FE	(10) IV-FE
S										
POST	-2.99 (6.79)	5.31 (10.70)	269.48*** (63.98)	202.83*** (45.63)	118.60*** (39.02)	188.90*** (3.59)	10.64 (18.01)	59.44*** (12.54)	61.69*** (18.06)	117.88*** (8.06)
Time	-1.52 (2.19)	-1.67 (1.72)	-39.48*** (5.99)	14.13*** (2.95)	-23.40*** (3.14)	14.81*** (1.62)	-11.90*** (1.11)	21.17*** (1.40)	-17.34*** (0.98)	18.30*** (1.52)
Timesq	0.28* (0.14)	0.19 (0.14)	2.06*** (0.37)	-0.63*** (0.20)	1.20*** (0.23)	-0.68*** (0.06)	0.59** (0.29)	-1.12*** (0.10)	0.88*** (0.32)	-0.92*** (0.08)
WorkingAge Pop		-0.00*** (0.00)		-0.00*** (0.00)		-0.00*** (0.00)		-0.00*** (0.00)		-0.00*** (0.00)
rGDP		0.00* (0.00)		0.00*** (0.00)		0.00*** (0.00)		0.00*** (0.00)		0.00*** (0.00)
LFPRate		1.74 (1.44)		4.60** (2.13)		4.59** (2.12)		4.46** (1.97)		4.51** (2.01)
Observations	274	220	47	47	47	47	47	47	47	47
Number of Countries	20	17	8	8	8	8	8	8	8	8
R-squared	0.11	0.45								
Partial R- squared			0.15	0.18	0.35	0.33	0.44	0.41	0.58	0.56
Hansen J p- value			0.31	0.29	0.29		0.46	0.68	0.34	
Kleibergen- Paap F statistic			16.52	2.27	161308.16	48175.53	25.20	51.11	100353.35	39253.04

Note: Robust and clustered standard errors at the country level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. POST is instrumented with the debt-to-GDP ratio, terms of trade, and five-year change in democracy score in columns (3) and (4). In columns (5) and (6) the instruments also include lagged POST. POST is instrumented with external debt-to-GDP ratio interacted with the change in world interest rates, terms of trade, and five-year change in democracy score in columns (7) and (8). In columns (9) and (10) the instruments also include lagged POST.

Table 5

Dependent Variable: Labor Force Participation Rate

VARIABLES	(1) OLS-FE	(2) OLS-FE	(3) IV-FE	(4) IV-FE	(5) IV-FE	(6) IV-FE	(7) IV-FE	(8) IV-FE	(9) IV-FE	(10) IV-FE
POST	0.04 (0.18)	-0.01 (0.19)	-0.44 (3.38)	-0.31 (3.13)	-0.24 (0.25)	-0.25 (0.24)	0.05 (3.90)	0.29 (3.49)	-0.24 (0.25)	-0.24 (0.24)
Time	-0.03 (0.04)	0.01 (0.04)	0.12 (0.26)	0.11 (0.23)	0.11*** (0.03)	0.10*** (0.04)	0.09 (0.30)	0.06 (0.26)	0.11*** (0.03)	0.10*** (0.04)
Timesq	0.01* (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)
WorkingAgePop		0.00 (0.00)		0.00 (0.00)		0.00 (0.00)		0.00 (0.00)		0.00 (0.00)
rGDP		-0.00 (0.00)		0.00 (0.00)		0.00 (0.00)		0.00 (0.00)		0.00 (0.00)
Observations	1,430	1,333	960	960	960	960	960	960	960	960
Number of Countries	73	73	62	62	62	62	62	62	62	62
R-squared	0.01	0.03								
Partial R-squared			0.01	0.01	0.41	0.41	0.01	0.01	0.41	0.41
Hansen J p-value			0.39	0.37	0.50	0.43	0.43	0.38	0.56	0.50
Kleibergen-Paap F statistic			8.40	8.12	1945.14	1934.15	2.79	2.71	1904.93	1898.33

Note: Robust and clustered standard errors at the country level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. POST is instrumented with the debt-to-GDP ratio, terms of trade, and five-year change in democracy score in columns (3) and (4). In columns (5) and (6) the instruments also include lagged POST. POST is instrumented with external debt-to-GDP ratio interacted with the change in world interest rates, terms of trade, and five-year change in democracy score in columns (7) and (8). In columns (9) and (10) the instruments also include lagged POST.

Table 6

Dependent Variable: Female Labor Force Participation Rate

VARIABLES	(1) OLS-FE	(2) OLS-FE	(3) IV-FE	(4) IV-FE	(5) IV-FE	(6) IV-FE	(7) IV-FE	(8) IV-FE	(9) IV-FE	(10) IV-FE
POST	0.04 (0.22)	-0.02 (0.24)	0.10 (5.45)	1.32 (5.24)	-0.34 (0.35)	-0.35 (0.34)	1.00 (6.80)	2.76 (6.30)	-0.34 (0.35)	-0.35 (0.34)
Time	0.11** (0.05)	0.15** (0.06)	0.24 (0.41)	0.13 (0.38)	0.28*** (0.05)	0.26*** (0.06)	0.18 (0.52)	0.03 (0.46)	0.28*** (0.05)	0.26*** (0.06)
Timesq	0.01*** (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)
WorkingAgePop		0.00 (0.00)		-0.00 (0.00)		-0.00 (0.00)		-0.00 (0.00)		-0.00 (0.00)
rGDP		0.00 (0.00)		0.00 (0.00)		0.00 (0.00)		0.00 (0.00)		0.00 (0.00)
Observations	1,419	1,322	960	960	960	960	960	960	960	960
Number of Countries	72	72	62	62	62	62	62	62	62	62
R-squared	0.07	0.19								
Partial R-squared			0.01	0.01	0.41	0.41	0.01	0.01	0.41	0.41
Hansen J p-value			0.40	0.39	0.48	0.35	0.58	0.57	0.48	0.36
Kleibergen-Paap F statistic			8.40	8.12	1945.14	1934.15	2.79	2.71	1904.93	1898.33

Note: Robust and clustered standard errors at the country level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. POST is instrumented with the debt-to-GDP ratio, terms of trade, and five-year change in democracy score in columns (3) and (4). In columns (5) and (6) the instruments also include lagged POST. POST is instrumented with external debt-to-GDP ratio interacted with the change in world interest rates, terms of trade, and five-year change in democracy score in columns (7) and (8). In columns (9) and (10) the instruments also include lagged POST.

Table 7

Dependent Variable: Female Employment

VARIABLES	(1) OLS-FE	(2) OLS-FE	(3) IV-FE	(4) IV-FE	(5) IV-FE	(6) IV-FE	(7) IV-FE	(8) IV-FE	(9) IV-FE	(10) IV-FE
POST	-0.01 (0.13)	0.18* (0.10)	0.76 (2.47)	0.76 (0.67)	0.12 (0.26)	0.57** (0.23)	4.27 (5.67)	0.63 (0.92)	0.12 (0.26)	0.57** (0.23)
Time	0.11** (0.05)	-0.02 (0.02)	0.08 (0.26)	-0.09 (0.09)	0.15** (0.06)	-0.07* (0.04)	-0.29 (0.59)	-0.08 (0.11)	0.15** (0.06)	-0.07* (0.04)
Timesq	-0.00 (0.00)	0.00* (0.00)	0.00 (0.01)	0.01 (0.01)	-0.00 (0.01)	0.01** (0.00)	0.02 (0.03)	0.01 (0.01)	-0.00 (0.01)	0.01** (0.00)
WorkingAgePop		0.00*** (0.00)		0.00*** (0.00)		0.00*** (0.00)		0.00*** (0.00)		0.00*** (0.00)
rGDP		-0.00 (0.00)		0.00 (0.00)		0.00 (0.00)		0.00 (0.00)		0.00 (0.00)
LFPRate		0.04*** (0.01)		0.07* (0.04)		0.07** (0.04)		0.07* (0.04)		0.07** (0.04)
Observations	800	785	575	575	575	575	575	575	575	575
Number of Countries	70	70	60	60	60	60	60	60	60	60
R-squared	0.29	0.82								
Partial R-squared			0.01	0.01	0.29	0.29	0.00	0.01	0.29	0.29
Hansen J p-value			0.46	0.42	0.69	0.56	0.51	0.28	0.42	0.44
Kleibergen-Paap F statistic			1.09	1.10	30.88	36.78	0.52	0.62	32.62	42.95

Note: Robust and clustered standard errors at the country level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. POST is instrumented with the debt-to-GDP ratio, terms of trade, and five-year change in democracy score in columns (3) and (4). In columns (5) and (6) the instruments also include lagged POST. POST is instrumented with external debt-to-GDP ratio interacted with the change in world interest rates, terms of trade, and five-year change in democracy score in columns (7) and (8). In columns (9) and (10) the instruments also include lagged POST.

Table 8

Dependent Variable: Employment

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	OLS-FE	OLS-FE	IV-FE	IV-FE	IV-FE	IV-FE	IV-FE	IV-FE	IV-FE	IV-FE
POST	-3.05*** (0.96)	-1.06 (1.18)	132.93 (187.52)	225.82 (434.69)	-12.40 (33.85)	33.46 (88.38)	2.60 (18.12)	17.38 (25.72)	-3.73 (25.39)	20.36 (35.46)
POST_Dictator	-0.03 (1.88)	0.67 (2.72)	-70.48 (89.42)	-115.73 (196.91)	0.88 (17.96)	-18.77 (36.86)	-5.73 (9.87)	-11.04 (11.04)	-2.82 (13.85)	-12.68 (14.53)
POST_Labor	4.76*** (1.53)	2.56** (1.18)	-66.50 (149.88)	-164.74 (406.16)	17.39 (32.90)	-26.89 (85.80)	6.47 (20.87)	-14.14 (32.84)	9.05 (25.06)	-14.39 (35.19)
Time	0.41*** (0.09)	0.18** (0.07)	-4.64 (4.77)	-5.86 (8.12)	0.51 (0.33)	-0.29 (1.00)	0.13 (0.70)	0.09 (0.78)	0.43* (0.25)	-0.12 (0.37)
Timesq	0.04*** (0.01)	0.05*** (0.02)	0.12 (0.07)	0.15 (0.12)	0.06** (0.02)	0.08** (0.03)	0.06** (0.03)	0.07** (0.03)	0.06** (0.02)	0.07*** (0.03)
WorkingAgePop		0.00* (0.00)		0.00 (0.00)		0.00 (0.00)		0.00 (0.00)		0.00 (0.00)
rGDP		-0.00 (0.00)		-0.00 (0.00)		-0.00 (0.00)		-0.00 (0.00)		-0.00 (0.00)
LFPRate		0.24 (0.23)		0.55 (2.26)		-0.09 (0.68)		-0.14 (0.60)		-0.13 (0.61)
Observations	1,174	990	713	712	713	712	713	712	713	712
Number of Countries	79	71	59	59	59	59	59	59	59	59
R-squared	0.21	0.34								
Partial R-squared			0.00	0.00	0.01	0.00	0.01	0.00	0.01	0.00
Hansen J p-value			.	.	0.04	0.04	.	.	0.71	0.84
Kleibergen-Paap F statistic			0.17	0.08	0.21	0.07	0.91	0.38	0.52	0.40

Note: Robust and clustered standard errors at the country level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. POST is instrumented with the debt-to-GDP ratio, terms of trade, and five-year change in democracy score in columns (3) and (4). In columns (5) and (6) the instruments also include lagged POST. POST is instrumented with external debt-to-GDP ratio interacted with the change in world interest rates, terms of trade, and five-year change in democracy score in columns (7) and (8). In columns (9) and (10) the instruments also include lagged POST.

Table 9

Dependent Variable: Wage Index

VARIABLES	(1) OLS-FE	(2) OLS-FE	(3) IV-FE	(4) IV-FE	(5) IV-FE	(6) IV-FE	(7) IV-FE	(8) IV-FE	(9) IV-FE	(10) IV-FE
POST	21.21 (16.53)	14.95 (15.19)	433.41*** (75.55)	394.14*** (44.79)	421.89*** (72.24)	366.84*** (23.90)	264.59*** (71.42)	103.81*** (21.83)	310.73*** (75.73)	125.09*** (21.97)
POST_Dictator	-19.33*** (6.04)	-23.03*** (7.42)								
POST_Labor	-35.11** (16.36)	-9.97 (10.19)	-285.06*** (62.10)	-144.47*** (18.89)	-300.53*** (42.71)	-140.19*** (19.83)	-261.27*** (54.81)	-44.59** (21.25)	-252.75*** (55.90)	-6.86 (20.53)
Time	-2.19 (2.52)	-2.29 (1.88)	-33.59*** (7.88)	5.73** (2.68)	-31.09*** (4.69)	7.04*** (1.53)	-17.55*** (2.29)	19.30*** (1.69)	-23.17*** (2.39)	17.99*** (1.93)
Timesq	0.31* (0.16)	0.23 (0.19)	1.40** (0.59)	-0.03 (0.18)	1.25*** (0.40)	-0.12 (0.08)	0.58 (0.48)	-0.98*** (0.11)	0.89* (0.49)	-0.90*** (0.10)
WorkingAgePop		-0.00*** (0.00)		-0.00*** (0.00)		-0.00*** (0.00)		-0.00*** (0.00)		-0.00*** (0.00)
rGDP		0.00* (0.00)		0.00*** (0.00)		0.00*** (0.00)		0.00*** (0.00)		0.00*** (0.00)
LFPRate		1.18 (1.48)		2.46 (2.26)		2.50 (2.15)		3.78** (1.57)		4.41** (1.90)
Observations	269	215	47	47	47	47	47	47	47	47
Number of Countries	20	17	8	8	8	8	8	8	8	8
R-squared	0.20	0.47								
Partial R-squared			0.19	0.09	0.21	0.11	0.20	0.20	0.21	0.20
Hansen J p-value			0.17	0.35	0.18		0.94	0.60	0.46	
Kleibergen-Paap F statistic			34.44	42.79	85.92	48.46	73.60	3.87	965.78	4.86

Note: Robust and clustered standard errors at the country level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. POST is instrumented with the debt-to-GDP ratio, terms of trade, and five-year change in democracy score in columns (3) and (4). In columns (5) and (6) the instruments also include lagged POST. POST is instrumented with external debt-to-GDP ratio interacted with the change in world interest rates, terms of trade, and five-year change in democracy score in columns (7) and (8). In columns (9) and (10) the instruments also include lagged POST. The coefficient on POST interacted with dictatorships is not identified due to insufficient variance within the sample.

Table 10

Dependent Variable: Female Employment

VARIABLES	(1) OLS-FE	(2) OLS-FE	(3) IV-FE	(4) IV-FE	(5) IV-FE	(6) IV-FE	(7) IV-FE	(8) IV-FE	(9) IV-FE	(10) IV-FE
POST	-0.33* (0.17)	0.06 (0.11)	-441.90 (3,142.36)	2.15 (10.42)	-5.59 (19.65)	0.67 (8.83)	-70.28 (115.76)	1.02 (12.18)	-41.74 (48.40)	3.47 (11.82)
POST_Dictator	-0.02 (0.14)	-0.29** (0.11)	-1.66 (18.43)	-0.43 (0.57)	-0.97 (1.56)	-0.37 (0.50)	-1.75 (2.99)	-0.40 (0.33)	-1.68 (2.06)	-0.44 (0.48)
POST_Labor	0.33*** (0.12)	0.18 (0.12)	403.52 (2,861.52)	-1.30 (10.38)	6.18 (20.56)	-0.01 (9.08)	69.53 (106.69)	-0.09 (13.09)	44.16 (49.88)	-2.90 (12.12)
Time	0.11** (0.05)	-0.02 (0.02)	4.80 (33.77)	-0.10 (0.09)	0.13 (0.08)	-0.07* (0.04)	0.36 (1.46)	-0.11 (0.13)	-0.01 (0.14)	-0.07* (0.04)
Timesq	-0.00 (0.00)	0.00* (0.00)	-0.20 (1.38)	0.01 (0.01)	-0.00 (0.01)	0.01** (0.00)	-0.01 (0.07)	0.01 (0.01)	0.01 (0.01)	0.01** (0.00)
WorkingAgePop		0.00*** (0.00)		0.00** (0.00)		0.00*** (0.00)		0.00** (0.00)		0.00** (0.00)
rGDP		0.00 (0.00)		-0.00 (0.00)		0.00 (0.00)		0.00 (0.00)		-0.00 (0.00)
LFPRate		0.04*** (0.01)		0.07* (0.04)		0.07* (0.04)		0.08** (0.04)		0.07** (0.03)
Observations	785	770	564	564	564	564	564	564	564	564
Number of Countries	69	69	59	59	59	59	59	59	59	59
R-squared	0.29	0.83								
Partial R-squared			0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Hansen J p-value			.	.	0.23	0.72	.	.	0.74	0.82
Kleibergen-Paap F statistic			0.01	0.19	0.36	0.13	0.11	0.12	0.33	0.12

Note: Robust and clustered standard errors at the country level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. POST is instrumented with the debt-to-GDP ratio, terms of trade, and five-year change in democracy score in columns (3) and (4). In columns (5) and (6) the instruments also include lagged POST. POST is instrumented with external debt-to-GDP ratio interacted with the change in world interest rates, terms of trade, and five-year change in democracy score in columns (7) and (8). In columns (9) and (10) the instruments also include lagged POST.

Appendix Table 1
Countries in Dataset and Year of Liberalization

Country	Reform Year	Unemployment Rate	Employment	Wage Index	LFP Rate	Female LFP Rate	Female Employment
Albania	1992	X	X	X	X	X	X
Argentina	1991	X	X		X	X	X
Armenia	1995	X	X	X	X	X	X
Australia	1964	X	X	X			
Austria	1960		X	X	X		
Azerbaijan	1995	X	X		X	X	X
Bangladesh	1996	X	X		X	X	X
Barbados	1966		X				
Benin	1990	X	X		X	X	X
Bolivia	1985	X	X		X	X	X
Botswana	1979				X	X	
Brazil	1991	X	X		X	X	X
Bulgaria	1991	X	X		X	X	X
Burkina Faso	1998	X	X		X	X	X
Burundi	1999		X		X	X	X
Cameroon	1993	X	X		X	X	X
Cape Verde	1991		X		X	X	X
Chile	1976	X	X		X	X	
Colombia	1986	X	X		X	X	X
Costa Rica	1986	X	X		X	X	X
Cote d'Ivoire	1994	X	X		X	X	X
Cyprus	1960		X				
Czech Republic	1991	X	X	X	X	X	X
Dominican Republic	1992	X	X		X	X	X
Ecuador	1991	X	X		X	X	X
Egypt, Arab Rep.	1995	X	X		X	X	X
El Salvador	1989	X	X		X	X	X
Ethiopia	1996	X	X		X	X	X
Finland	1960	X	X				
Gambia, The	1985		X		X	X	X
Georgia	1996	X	X		X	X	X
Ghana	1985		X		X	X	X
Guatemala	1988	X	X		X	X	X
Guinea	1986		X		X	X	X
Guinea-Bissau	1987		X		X	X	X
Guyana	1988	X	X		X	X	X
Honduras	1991	X	X		X	X	X
Hungary	1990	X	X	X	X	X	X
Indonesia	1970	X	X				
Ireland	1966		X				
Israel	1985	X	X	X	X	X	X
Jamaica	1989	X	X		X	X	X
Japan	1964	X	X	X			
Jordan	1965						
Kenya	1993		X		X	X	X
Korea, Rep.	1968	X	X				
Kyrgyz Republic	1994	X	X	X	X	X	X
Latvia	1993	X	X	X	X	X	X

Lithuania	1993	X	X	X	X	X	X
Macedonia, FYR	1994	X		X	X	X	X
Madagascar	1996	X	X		X	X	X
Malaysia	1963		X				
Mali	1988		X		X	X	X
Mauritania	1995	X	X		X	X	X
Mauritius	1968						
Mexico	1986	X	X	X	X	X	X
Moldova	1994	X	X		X	X	X
Morocco	1984	X	X		X	X	X
Mozambique	1995		X		X	X	X
Nepal	1991	X	X		X	X	X
New Zealand	1986	X	X	X	X	X	X
Nicaragua	1991	X	X		X	X	X
Niger	1994	X	X		X	X	X
Pakistan	2001	X	X		X	X	X
Panama	1996	X	X		X	X	X
Paraguay	1989	X	X		X	X	X
Peru	1991	X	X		X	X	X
Philippines	1988	X	X		X	X	X
Poland	1990	X	X	X	X	X	X
Romania	1992	X	X	X	X	X	X
Sierra Leone	2001		X		X	X	X
Singapore	1965	X	X				
Slovak Republic	1991	X	X	X	X	X	X
Slovenia	1991	X	X	X	X	X	X
South Africa	1991	X	X		X	X	X
Sri Lanka	1991	X	X	X	X	X	X
Sweden	1960	X	X	X			
Taiwan, China	1963		X				
Tajikistan	1996		X		X	X	X
Tanzania	1995	X	X		X	X	X
Trinidad and Tobago	1992	X	X	X	X	X	X
Tunisia	1989	X	X		X	X	X
Turkey	1989	X	X		X	X	X
Uganda	1988		X		X	X	X
Uruguay	1990	X	X		X	X	X
Venezuela, RB	1996	X	X		X	X	X
Yugoslavia, FR (Serbia/Montenegro)	2001	X	X				
Zambia	1993	X	X		X	X	X

Note: This table lists the countries included in the dataset and the year of liberalization from Wacziarg and Welch (2008). In principle, the liberalization date is the date after which all of the Sachs-Warner openness criteria are continuously met (however data limitations often imposed reliance on country case studies of trade policy). Countries that according to Wacziarg and Welch (2008) had liberalized their policies prior to 1960 or that had not liberalized by 2001 are excluded from our analysis.

Appendix Table 2
Variable Definitions and Sources

Variable name	Variable definition	Sources
Dependent variables:		
Unemployment rate (UnempRate)	The percentage of the labor force that is without work but available for and seeking employment.	IMF IFS World Bank WDI ILO KILM OECD LFS Regional agencies Country-specific sources
Employment (Emp)	In millions, includes employees, the self-employed, unpaid family members that are economically engaged, apprentices, and the military (aged 15 and older for series from the World Bank WDI).	The conference Board TED World Bank WDI
Female employment (FemaleEmp)	In millions, aged 15 and older.	World Bank WDI
Wage Index (WageIndex)	Index of wage earnings with 2005 as the base year equal to 100.	IMF IFS
Labor force participation rate (LFPRate)	Labor force as a percentage of the working age population. Total labor force comprises people ages 15 and older who meet the ILO definition of the economically active population: all people who supply labor for the production of goods and services during a specified period. It includes both the employed and the unemployed. While national practices vary in the treatment of such groups as the armed forces and seasonal or part-time workers, in general the labor force includes the armed forces, the unemployed, and first-time job seekers, but excludes homemakers and other unpaid caregivers and workers in the informal sector.	World Bank WDI IMF IFS
Female labor force participation rate (FemaleLFPRate)	Female labor force as a percentage of the female working age population.	World Bank WDI
Endogenous independent variable:		
POST	A dummy variable that takes the value of 1 every year after a country's reform year.	Wacziarg and Welch (2008)
POST_Dictatorship	POST interacted with a dummy variable that takes the value of 1 for countries with a dictatorship. Countries with a dictatorship are identified as those with a score of four on PolityIV's regulation of participation (PARREG) variable, defined as countries where regulation of participation is restricted. The PARREG variable can take a value between one and five. A value of one indicates unregulated political participation, two indicates relatively stable but enduring political groups, three indicates oscillation among multiple identity groups that restrict competing groups' political activities, four indicates some organized political participation permitted but significant groups/issues are excluded from the political process, and five indicates regulated political participation but with little use of coercion. Totalitarian party systems, authoritarian military dictatorships, personal dictatorships and despotic monarchies are typically coded with a score of four.	PolityIV

POST_Labor	POST interacted with a dummy variable that takes the value of 1 for labor abundant countries. Labor abundant countries are identified as those with a ratio above one of its national share of world population to its national share of world GDP.	World Bank WDI.
Control variables:		
Time (Time)	Equal to -1 the year prior to reform, 0 the year of reform, 1 the year after reform, etc.	
Time squared (Timesq)	The square of time	
Real GDP (rGDP)	In thousands of constant 2005 international dollars, as PPP converted GDP per capita (chain series) constant 2005 prices multiplied by the population.	Penn World Tables v. 7.0
Working age population (WorkingAgePop)	Number of people who could potentially be economically active as the total population between the ages 15 to 64.	World Bank WDI
Instrumental variables:		
External debt-to-GDP ratio (Debt)	Total external debt (debt owed to nonresidents repayable in foreign currency, goods, or services) as a share of GDP.	World Bank WDI
External debt-to-GDP ratio interacted with the change in world interest rates (DebtTBill)	The external debt-to-GDP ratio interacted with the one-year change in the market yield on U.S. Treasury securities at 10-year constant maturity.	World Bank WDI United States Federal Reserve
Terms of trade (TofT)	Net barter terms of trade index, calculated as the percentage ratio of the export unit value indexes to the import unit value indexes, measures relative to the base year 2000.	World Bank WDI
Five-year change in democracy score (Dem5)	five-year change in PolityIV's governing authority index, where index scores range from -10 (most autocratic) to 10 (most democratic).	PolityIV
Lagged POST	A dummy variable that takes the value of 1 every year two years after a country's reform year.	Wacziarg and Welch (2008)

Note: IFS = International Financial Statistics. WDI = World Development Indicators. KILM = Key Indicators of the Labor Market. TED = Total Economy Database. LFS = Labor Force Statistics.